

THE VENTRIS–CHADWICK CORRESPONDENCE AND THE DECIPHERMENT OF LINEAR B: A DENIER, A DISSENTER AND A DUBIOUS CONCLUSION

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The correspondence between Michael Ventris and John Chadwick, housed in the Mycenaean Epigraphy Room in the Faculty of Classics, Cambridge, provides valuable insights into the decipherment of Linear B and the collaboration between the two men which produced first ‘Evidence for Greek dialect in the Mycenaean archives’ (Ventris and Chadwick (1953)) and then *Documents in Mycenaean Greek* (Ventris and Chadwick (1956)). The letters also reveal interesting information about the relationship between Ventris and Chadwick and other scholars of the day. This article examines their relationship with Arthur Beattie, who never accepted the decipherment, and Leonard R. Palmer, who disagreed fundamentally with many of their interpretations of the texts. A file of correspondence containing letters from 1956, discovered only after the publication of Andrew Robinson’s biography of Ventris (Robinson (2002)), casts doubt on the conclusion that, perhaps in part owing to difficulties with Palmer, Ventris had lost interest in Linear B immediately before his death.

Introduction

The Mycenaean Epigraphy Room in the Faculty of Classics at Cambridge houses a complete set of the extant correspondence (310 letters and postcards, with enclosures and some additional letters from others) between Michael Ventris and John Chadwick, which took place between 9 July 1952 and 27 July 1956. Chadwick had kept Ventris’ letters, and acquired his own letters to Ventris, after the latter’s death, from his widow Lois. At least

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I should like to thank Dr Anna Judson and the managers of the Chadwick Fund, whose labour on the one hand and generosity on the other in 2012 made it possible for the bulk of the work on the correspondence from which this paper emerged to be conducted on Crete; to our host in Plakias, Stavros Androulidakis; and to the officers of the Cambridge Philological Society, at whose invitation I first presented it. I am grateful to an anonymous reviewer who caught a number of infelicities. The remaining faults are my own.

twenty-two further items of correspondence are missing. After Chadwick's death, the files in which he had kept the correspondence remained as he left them until Andrew Robinson consulted them to conduct research for his biography of Ventris (Robinson 2002). Only after Robinson's visit did John Killen discover a third file containing the letters from 1956.

As a preliminary to conservation, the contents of the files were catalogued and scanned by Anna Judson in 2012, the work being funded by a grant from the Chadwick Fund. The present writer transcribed and edited the correspondence, largely in the summer of 2017, with a view to the production of an edition and making the images and searchable text available under the auspices of the Cambridge Linear B Research Archive website, <http://calibra.classics.cam.ac.uk>.

The letters, which were exchanged for the most part daily (and sometimes more than once per day) document the initial contact between the two men, their collaboration first on 'Evidence' (Ventris and Chadwick 1953) and then on Documents (Ventris and Chadwick 1956), and the preparations for the first international colloquium on Mycenaean studies at Gif-sur-Yvette in 1956. They also provide valuable insights into their relationships with other scholars with interests in the new field of Mycenaology. The present paper, which represents the first results of a larger project on the Ventris–Chadwick collaboration viewed through the lens of the correspondence, looks at two such relationships, those with Arthur Beattie and Leonard R. Palmer. Beattie denied the correctness of the decipherment to his dying day. His animus seems to have been against Ventris in particular, to have been personal rather than academically grounded, and was vicious. Palmer accepted the decipherment but remained vehemently opposed to many of the interpretations of the tablets which Ventris and Chadwick made elsewhere and especially in Documents, his review of which (Palmer 1957b) was savage. Their relationship was complex: Palmer seems to hold Chadwick rather than Ventris responsible for all he finds fault with, but while in their letters Ventris and especially Chadwick find him a frustration, they collaborated closely – on the indices for the Gif colloquium, for example – and Chadwick and Palmer continued to work together on *Studies in Mycenaean Inscriptions and Dialect* after Ventris' death.

Arthur Beattie

Chadwick had been taught by Beattie while at Cambridge, and it was the (assistant) lectureship which fell vacant on Beattie's appointment to the Chair of Greek at Edinburgh which Chadwick took up in October 1952. He first appears in the correspondence on 25 July 1952, when Chadwick writes that he is 'very interested [in the decipherment] ... He is an expert on Greek dialects, and might like to take an active part.' Beattie would indeed take an active part, but as the chief agitator against Ventris' solution.

Chadwick must have written to Beattie before this, or shortly afterwards, asking for advice on textbooks and collections of inscriptions to use in his lectures on the Greek dialects – lectures which Beattie had given before his departure to Scotland – and

mentioning Ventris' work. Beattie's reply, dated 1 August, begins with the unhelpful observation only that the relevant books are in the Faculty and University Libraries, and a complaint that his copy of Buck's *Greek Dialects* 'was stolen by a college thief in [his] last year at Cambridge' (and one is tempted to wonder whether this act of larceny was perhaps one of the factors which would come to contribute to his attitude to the scholars of perfidious Albion in due course). He then goes on, 'I am much interested in Ventris' work, and should be very grateful if you would persuade him to let me have a look at his stuff. Although I found his article intriguing, I confess that the manner of its presentation did not convince me.' He has no objection, he says, to the language being Greek; indeed, he 'reached the same conclusion some years ago, soon after writing an article (mercifully unpublished) to prove the contrary'. He then raises two specific points: how do they know that words ending in $-\varphi\iota$ are datives, and that the q -series represents 'a genuine labiovelar guttural and not some development from it'. These are fair questions. In fact, the forms in $-\phi\iota$ are (with one or perhaps two exceptions)¹ instrumental or locative, although they express functions which were expressed by the dative in later Greek. The q -series could represent a development of the Indo-European (IE) labiovelars, though that detail hardly matters. Beattie ends, 'many other queries suggest themselves to me, but I had better keep them to myself until I have seen something of the exposition'.

What is the 'article' to which Beattie refers? It cannot be Ventris 1940, which espouses an Etruscan solution. It may be Worknote 20,² but the term 'article' seems hardly appropriate, and Beattie, who was not among the original recipients, later alleges several times in print that the Worknotes were kept secret by Ventris during his lifetime and by his supporters after his death. It may be the 'Experimental Mycenaean Vocabulary', a copy of which Ventris sent to Chadwick with his first letter; but it is not clear how Beattie would have seen it: certainly not from Chadwick, who writes to Ventris on 4 August – three days after Beattie's letter – to thank him 'for the spare copy of your vocabulary, which I intend to send on to Professor Beattie, as he seems very interested'. Nor had Ventris sent it to Edinburgh yet. Chadwick had spoken with Paul Maas about Ventris before 17 July, and again in more detail the following week, and in his letter of 1 August Beattie refers to Maas' support for the decipherment, so perhaps it was Maas who supplied him with a copy. Again, however, this seems unlikely, for on 25 July Chadwick tells Ventris, in the same paragraph in which he mentions his second conversation with Maas, that he has shown nobody the 'grid', that nobody had asked to see it, and that he had 'taken pains

1 On KN Ld(2) 787 the phrase $ki\text{-}to\text{-}pi$ may have the sense of a final dative 'for chitons' (Doria (1968) 772, following Milani (1958) 108; Killen (1979) 171). KN L 104 has $pa\text{-}we\text{-}pi$, which might similarly be 'for cloaks' (Doria (1968) 772).

2 The Worknotes, originally privately circulated, and some other of Ventris' working materials, are now all published as Ventris and Sacconi 1988.

to impress on everyone that it is your solution and it is for you to publish it as you think fit'. It must therefore be the transcript of Ventris' broadcast on the BBC's Third Programme which had aired on 1 July (Ventris 1952a) and which was subsequently published in *The Listener* on 10 July (Ventris 1952b).³ The comment that 'the manner of its presentation did not convince me' also suggests this, since in the popular format of the broadcast Ventris gave no details of his working.

On 16 May 1953 Carl Blegen wrote to Ventris with details of the now-famous 'tripod tablet', known today as **PY Ta 641**, whose syllabic signs spell out descriptions of tripods and of vessels with four, three and no handles alongside ideograms depicting tripods and vessels with the corresponding numbers of handles. This would rightly be hailed as an independent corroboration of the decipherment, for it was a tablet which Ventris did not use – nor could he have – in determining his syllabic values, but which could be successfully read with those values. On 24 June 1953 Chadwick gave a talk on the decipherment at the Society of Antiquaries in which he presented **Ta 641**. He reports that Beattie was present and 'swallowed the tripods & went away good-naturedly complaining that "this must be the most irregular writing system on record"'. Chadwick writes on 10 July that he has had a letter from Beattie, 'who confesses himself not yet convinced, though obviously shaken by the tripods'. In the same letter Beattie asked for a copy of **Ta 641** (then not yet published by Blegen), which Chadwick says he supplied. 'I fancy he will come round when he has given it some close study.' Beattie was not to be convinced, however. On 5 December Chadwick writes, 'I'm very sorry about this, for I expected him to give us some help. But he's an odd chap, and may have some preconceived ideas of his own. He is certainly not in the best company.'

If this was initially a cause of sadness, it became a cause for concern. Beattie was supposed to provide the chapter on language in Homeric Greece to the *Wace–Stubbings Companion to Homer*, a draft of which Stubbings showed to Chadwick. Chadwick complains (28 July 1954) that it contains a number of errors, but worse, it omits all reference to the decipherment other than a comment that 'recent attempts to decipher Linear B as Greek were less than convincing'. He goes on to say that Beattie was likely to be in the running to be the new Professor of Comparative Philology following Jopson's retirement in 1955, and that 'you may be sure I take a dim view of this. I have recommended Stubbings to show it to Palmer to obtain an unbiased opinion.' The intention was surely to make sure that Palmer was on side should he be consulted about Beattie's suitability. In the end, W. S. Allen was appointed to the chair, and the chapter on Homeric language was written by Palmer when the volume appeared in 1962.

On 11 August 1955 Chadwick addressed the Triennial Meeting of the Classical Societies in Oxford, giving a progress report on the decipherment. The *Manchester Guardian* for that day (p. 3) reports that there were no dissenting voices. Chadwick himself, in a letter to Ventris of

³ Later reprinted in Ventris and Sacconi (1988) 363–7.

the same date, says, ‘No one that I have met [at the meeting] has any real doubt about the decipherment, and Arthur Beattie, who was not there, is reported to be left in a minority of one.’

Still, Chadwick hoped that Beattie would come round. He had asked for details of the Oxford talk, which Chadwick supplied on 3 October. Beattie’s reply of 6 October is worth quoting in full:

Dear Chadwick,

Thank you for your letter of 3rd October. I am returning your notes entitled ‘Mycenaean Progress Report’ herewith. They have been very useful.

I accept your verdict on horses and asses. At the same time, I should remark that it is somewhat hazardous to substitute a q in your script for what ought to be a kw. Then again, it seems curious that in the word *ka-ra-ko* you substitute k for what apparently should be a q. Then again, you put a w into *μάροθων* although you have taken one out of *i-qa*.⁴ The phonology of this language seems to be in a state of flux!

I agree that one man might deal with all the seeds and plants in your list, but it seems to be in the realm of the barely possible rather than the wholly possible; but perhaps new evidence will come to light.

Yours sincerely,

A. J. Beattie

Chadwick (14 October 1955) describes it as ‘very grudging to say the least’. In fact, *ka-ra-ko* and *ma-ra-tu-wo* are two of the forms to which Beattie will object in his first rebuttal of the decipherment (Beattie (1956) 9 n. 5), on which he must have been working at the time.⁵ On 23 October Chadwick reports that he had met Beattie while lecturing in Glasgow the previous weekend, that they ‘greeted each other somewhat sullenly, but he didn’t open his mouth at question time’. By now, to Ventriss (letter of 26 October 1955), Beattie is ‘a dunderhead’.

The existence of Beattie’s forthcoming rebuttal was known to Chadwick certainly by 20 February 1956, when he tells Ventriss that it has been accepted by the *Journal of Hellenic Studies*, ‘despite, I am told, efforts at suppression by Webster and Turner . . . I can’t help thinking it will do his reputation no good.’ It appeared in print in the month of Ventriss’ death.

4 Typically of Beattie’s complaints, there is a grain of truth. Sanskrit *áśvaḥ* shows that the etymon for Greek ἵππος (Mycenaean *i-qa*) and Latin *equus* is *ekwos; but IE *kw evidently becomes *k^wk^w in Proto-Greek, or at least identical with its outcome, and the Linear B writing system does not write geminate consonants. There are in any case further unsolved problems with the initial vocalism and aspiration in the Greek word, which cannot derive straightforwardly from *ekwos by Greek sound laws. On *ka-ra-ko* / *qa-ra-ko* see below, n. 5.

5 Beattie’s complaint about *ka-ra-ko* (then read on **MY Ge 105**) was that Chadwick had equated it with Attic βλήχων ‘pennyroyal’, seemingly requiring an initial voiced labiovelar (see e.g. Chadwick in Bennett and Chadwick (1958) 107). But in fact forms with initial plain velar are found in the dialects (Ionic γλήχων, Doric γλάχων, Boeotian γλάχων and γλάχῶ), and it is these to which Chadwick expressly compares it. In any case, the reading *ka-ra-ko* is uncertain (Bennett (1958) 102), and is generally not accepted today.

What was the cause of Beattie's hostility and his refusal, even in the face of compelling evidence and despite Chadwick's fervent hopes, to 'come round'? It was not that he had any objection to the language being Greek – as he had written to Chadwick, he had already come to that conclusion himself. Nor, as Chadwick supposed, did he have any preconceived ideas of his own – indeed, in the first of his tirades in the *Glasgow Herald* (Beattie 1959b), he explicitly says, 'First let me make it clear that the critics have no alternative solution to offer. Their task is limited to proving that Ventris' methods and results are without value.'

The JHS rebuttal (Beattie 1956) itself is instructive. As a cogent argument it is mercilessly (and wittily) destroyed by Treweek (1957),⁶ who notes that while individual statements are usually true (if also frequently misrepresentations of what Ventris actually did and said), 'the general effect of a paragraph of such sentences is to mislead the reader in the desired direction, as is done in certain advertisements and company prospectuses' (p. 14). An example from the beginning of the article (Beattie (1956) 1) will suffice as illustration:

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.

Taken individually, these statements are largely unobjectionable. But the order of the statements is duplicitous, for it naturally invites us to infer that Ventris started from the assumption that the language was Greek. Beattie then takes this as a fact in the remainder of this and his other attacks. It is a deliberate misrepresentation of the truth: as Ventris and Chadwick (Ventris and Chadwick (1953) 84) very clearly state, 'the linguistic features evident in the new material have forced us to the conclusion that the language is ... not only Indo-European but specifically Greek' (my emphasis). If Beattie really thinks that the order of events is as he claims, then either he has not read the sentence in 'Evidence' (surely impossible), or he thinks that Ventris and Chadwick are lying; but he cannot say that, because he cannot prove it. Alternatively, he does not believe in his own order, but deliberately leads the reader astray to undermine Ventris and strengthen his own argument. Either way, it is a masterful piece of what would now be called 'spin'.

⁶ As an example, see the instances of sets of 'triplets' used to counter Beattie's criticism, 'beat-ing beat-tie beat-itude ... dim-ity dim-wit dim-ple ... tri-pe tri-vet tri-ckle' (p. 15); or the suggestion (p. 19) that to properly calculate 'Beattie numbers' (the number of potential phonetic words which a Linear B spelling could generate) would require a 'Frequency Analysis Record Computed Exponentially'.

Elsewhere he appears to display a perverse resistance to the idea that language can change. For example (Beattie (1956) 5), ‘Α κτοίνα is not a farm or field, as Mr Ventris thinks, but a village community ... and κτίμενος would not, as Mr Ventris thinks, mean “established”, nor even “cultivated”, but merely “built”, “constructed” ... The whole thing is nonsense from beginning to end.’ By the same false logic, τράπεζα should not mean ‘table’ in classical Greek because it means ‘bank’ in the modern language. He goes on, ‘It is unnecessary to remark that ὄνοτιρες, ἐντεσδόμος and Φανάκτηρος are fictitious or that ἐνέενσι is morphologically unacceptable.’ He does not explain why ἐνέενσι is ‘morphologically unacceptable’; Chadwick in his first letter to Ventris (13 July 1952) had accounted for this form by deriving it from **esenti* with the *e*-grade of the stem extended into the plural from the singular, and he had published the derivation in Chadwick (1954) 12.

By ‘fictitious’ Beattie means ‘not attested in later Greek’; but if Homer can have βασιλεύετος, why is Φανάκτηρος so troublesome? And again, by the same argument the ancient Greek word ἀσάμινθος must be ‘fictitious’, because it is absent from modern Greek. As Treweek (1957, 17) shrewdly observes, ‘If the texts differ from our preconceived ideas of Mycenaean, we must revise our ideas ... we must not like Beattie stick to *mumpsimus*’; and (n. 7) ‘It is sheer folly for any linguist to maintain his opinions about what a language might be or should be in the fact [*sic*] of texts which show what it is.’ It is indeed baffling that a linguist like Beattie could ‘stick to *mumpsimus*’; but though this be madness, yet there be method in ‘t. Note the *praeteritio* ‘It is unnecessary to remark that ...’: this is a passage of rhetoric, not of linguistic argument. The repeated ‘as Mr Ventris thinks’ is calculated to impugn Ventris’ credentials. Indeed, these comments are preceded by the statement, ‘But anyone acquainted with the Greek tongue can see that this is not a Greek text.’ This is patently not true: to many people who are familiar with Greek, including Chadwick, **PY En 609** does look like a Greek text. But the implication is that because Ventris sees this as Greek, his knowledge of Greek is flawed.

Later on (Beattie (1956) 5) he complains that ‘because [the system of spelling rules] is so imprecise, it enables Mr Ventris to discern Greek words in groups of syllables that look entirely un-Greek to the classical scholar’ (my emphasis). This I think is the nub of it: Ventris is not a classical scholar, and it is in Beattie’s eyes an affront to the academy that an amateur should have succeeded where eminent scholars did not. His attitude was doubtless not helped by his having first read of the decipherment in a popular rather than scholarly account in *The Listener*. It is telling, I think, that Chadwick is mentioned only once, as the collaborator on ‘Evidence’ (Beattie (1956) 1). Chadwick is absent even when Beattie complains (1956, 6) that ‘[f]rom comparative philology we know something of the sound system of Greek in the Mycenaean age or thereabout. It bears little relation to Mr Ventris’ decipherment.’ But then Chadwick must be left out, because he, too, knew something about the sound system of Greek in the Mycenaean age, and were he mentioned here it would weaken Beattie’s undermining of Ventris’ expertise.

The thorn in Beattie's side is, of course, **PY Ta 641**, the 'tripods' tablet which provided independent corroboration of the validity of the decipherment. In order to deny this validity he must somehow dismiss the evidence of this tablet. In Beattie (1956) 9–17 he attacks Ventris' interpretation of it, and tries, with increasingly desperate and incoherent arguments, to show that the equations between ideograms (depicting tripods, vessels and number of handles) and their corresponding Greek descriptions are coincidental. These wholly unconvincing arguments – essentially amounting to a claim that the fact that the sequences *ti-ri-*, *qe-* and *to-ro-* appear elsewhere unconnected with the numerals 'three' and 'four' invalidates the connection between the words *ti-ri-jo-we/qe-to-ro-we* and the three- and four-handled ideograms – are comprehensively demolished by Treweek (1957, 22–3).

In Beattie (1958) he tries a different tack, alleging that Ventris had seen **Ta 641** before writing Worknote 20, and that far from being corroborating evidence, the material in the tablet had been used in the original determination of sound values. He makes exactly the accusation that Ventris feared in his letter to Chadwick of 17 July 1952, that he had 'pre-cooked the material'.

When Blegen (1959) gave the full chronology of the discovery, cleaning, joining and reading of the two halves of **Ta 641**, and thus showed that Beattie's version of events was impossible, Beattie changed tack again and in two articles in the *Glasgow Herald* (Beattie (1959b), (1959c)), as well as in Beattie (1959a), he accepts that Ventris had not seen **Ta 641** itself before Worknote 20, but alleges he had seen a different but similar tablet which he had used and kept secret (or destroyed). 'It is impossible to say', he says (Beattie 1959c), 'who can have found the tablet or how Ventris obtained a copy of it.' There is of course only one person who can have found this mythical earlier tablet, viz. Blegen; and there is only one way Ventris could have got hold it, viz. if Blegen gave it to him. We are intended to infer that Blegen was in on the scam. This too is implied in the sentence 'Since the archaeologist who found the known tablet insists that he cannot be mistaken on [the date of its discovery], it is preferable to believe that another tablet was the source of Ventris' information' – Blegen 'insists' (but may not be trustworthy); so, Beattie implies, either Blegen is lying on this point, or he is concealing an earlier tripod tablet. But, of course, Beattie is careful not to say so in so many words: unlike the dead, the living can sue for defamation.

The same willingness not to let the truth get in the way of alternative facts and a good conspiracy theory can be seen in the statement of Beattie (1958) 61: 'Although the contents of all the Worknotes were guarded from the public eye by the author during his life and are kept unpublished by his supporters in England since his death, a complete set has recently come into the possession of the Edinburgh University Library.' Now Davison (1960, 22) reports that Beattie had in May 1958 sent him a copy of Worknote 20 with an attached essay by Chadwick stating that by permission of Lois Ventris, microfilm copies of all of the Worknotes were available from the library of the Institute of Classical Studies. Davison says that Beattie claimed he had written the line about Worknotes being guarded from the public before he knew about the microfilms, and perhaps he did. But

why then did he not remove it, when he was still working on the paper? And why does he say near the beginning of Beattie (1959b) that '[Ventriss' followers] have not published his "Worknotes". (No. 20 alone has been circulated privately, and that by me)'; and why does he end Beattie (1959c) by asking, 'Will his followers now bring forward the full text of the "Worknotes"?'? He knew very well that the full text was already available.

In leaving Beattie aside, we may finally note the curious coincidence of the subtitle of his second *Glasgow Herald* article, 'Comparison with the Piltdown hoax', and a comment made by Ventriss to Chadwick on 8 December 1953, apropos of an invitation to speak at the Second Congress on Classical Studies at Copenhagen the following year: 'All this rather relies on good new stuff coming out of the Pylos during winter & early summer: I don't want anybody brandishing Piltdown jaws. I heard through Piggott, incidentally, that Beattie was very anti-Old-Achaean.'⁷

L. R. Palmer

On 14 March 1953 Ventriss writes that he had recently attended a lecture on Neolithic pottery given in London by Blegen, and there he had (apparently for the first time) met 'Prof (?) Palmer, who seems to have been discussing our work with people at Cambridge, and whom I put on to Huxley at Oxford for a copy of the "spelling rules"'. The 'people at Cambridge' did not include Chadwick himself. Chadwick makes no comment in his reply.

On 6 November 1953 Chadwick gave a lecture on the decipherment at King's College London, at which Palmer was present. In a letter to Ventriss dated 9 November he writes, 'Palmer fired off a salvo of criticisms and then retired to catch a train before I could bring answering fire to bear on him. We shall be hearing more of him before long, for he is obviously working all out to catch up on us.' Chief among Palmer's criticisms on that occasion, which he expressed in a letter to Chadwick on 15 November and would later publish in Palmer (1954b), was the interpretation on KN Sd 4405.a of the phrase *wi-ri-ni-o*, *o-po-ḡo* as *winnios ompok^w*'s 'the rail (?) is of fig wood' (Ventriss and Chadwick (1953) 100). Chadwick rejected Palmer's alternative *wīnnios* 'of leather' on two grounds: (a) he believed at the time that word-initial *wr-* was spelt *r-* (Ventriss and Chadwick (1953) 91); and (b) the *o-po-ḡo* were sometimes made of ivory, indicating that a rigid substance was required. Palmer's argument against the omission of *w-* in initial *wr-* is beautiful and compelling: if *w-* is omitted, then all words spelt *wV₁-rV₂-* will contain a full vowel in the first sign, not merely an orthographic 'dummy' vowel. In that case, since Greek does not exhibit vowel harmony in adjacent syllables, the distribution of vowels in the two first signs should be random. Since there are sixteen possible sequences of *wV₁-rV₂-* (*wa-ra*, *wa-re*, *wa-rí*, *wa-ro*, *wa-ru*, *we-ra*, etc.; there being no *wu-* sign, at least so far identified,

7 'Old Achaean' was Chadwick's preferred name for the newly discovered dialect, on the grounds that 'Mycenaean' was ambiguous between the dialect of 'the Mycenaeans' as a whole, and the specific variety spoken at Mycenae.

there can be no **wu-ra* etc.), four of which (*wa-ra-*, *we-re-*, *wi-ri-*, *wo-ro-*) are isovocalic ($V_1 = V_2$), we would expect isovocalic sequences to occur with the frequency $4/16 = 0.2$. In 1953 Palmer reports that Bennett's *Index* contains twenty-six words beginning *wV-rV-*, of which eighteen are isovocalic, i.e. a frequency of 0.692, which is significantly higher than the expected 0.2, showing that the distribution of vowels was skewed in favour of isovocalism, which in turn suggested that some of the vowels in the first sign were 'dead' ones. (For the sake of completeness I have rerun the calculation using the words known today: I count forty-nine words beginning *wV-rV-*, of which thirty-four are isovocalic, giving, coincidentally, a frequency of 0.694.)

Palmer did not take the next step of determining how unlikely the omission of *w-* in *wr-* was. The χ^2 test is a simple way of determining how likely it is that two samples are drawn from the same population. In this case we can compare the observed distribution of isovocalic and heterovocalic sequences ($O_I = 18$, $O_H = 8$) to the distribution expected on the null hypothesis that both vowels are full and thus randomly distributed ($E_I = 26 \times 0.2 = 5.2$, $E_H = 26 \times 0.8 = 20.8$). The χ^2 statistic, defined as the sum of $(O - E)^2 / E$, is a measure of how different the observed and expected distributions are. In this case $\chi^2 = (18 - 5.2)^2 / 5.2 + (8 - 20.8)^2 / 20.8 = 39.38$. We compare this value to the critical value obtained from a statistical table for a desired confidence level (say, 95%, in which case the critical value is 3.84). If the computed value is greater than the critical value, as here, then we can be appropriately confident that the two distributions are different, and the null hypothesis is rejected. Alternatively, we can use statistical functions in a spreadsheet or other computational tool to calculate from χ^2 the probability that the distributions are significantly different. In this case there is a vanishingly small chance that the observed distribution is drawn from a population where isovocalism occurs with frequency 0.2 – too small for Excel to distinguish it from 0. (The same results are obtained if the modern numbers are used, $O_I = 34$, $O_H = 15$, $E_I = 0.2 \times 49 = 9.8$, $E_H = 39.2$, $\chi^2 = 74.7$.)

Chadwick was not to be convinced, at least not straight away: he writes to Ventris (29 September 1953), 'I feel his arguments are unconvincing unless he can produce other examples of initial F being written before r. But I do feel the force of his suggestion for *o-po-qo* [= *opok^won* "cheek-piece"]; and this, if right, does alter the whole picture.' In reality, no such further proof was required, nor did Palmer need to explain 'why the digamma is undoubtedly missing in some words at any rate': he had already cogently demonstrated that *wr-* was written with plene spelling, and that therefore interpretations in which *wr-* was written simply *r-* were either wrong, or were prima facie evidence that their etymologies were wrong. Chief among them was *ri-jo* = $\rho\acute{\iota}\omicron\nu$; for this was the word that prompted Ventris to propose, on the basis of Boisacq's etymon $\text{F}\rho\acute{\iota}\omicron\nu$, s.v., and 'the allegation that it makes position in Homer' (letter of 21 July 1952), that *wr-* was not written in full (letter of 16 July 1952).⁸ Chantraine (Chantraine et al. (1999) s.v.) subsequently gives the etymology of $\rho\acute{\iota}\omicron\nu$ as 'obscure', if it is to be connected with *ri-jo*.

⁸ This justification is made in response to the question by Chadwick (letter of 17 July 1952) whether $\rho\acute{\iota}\omicron\nu$ has initial **w-*.

In any event, by the time of the writing of *Documents*, Ventris and Chadwick had adopted both of Palmer's suggestions *wi-ri-ni-jo* = *wīrīnios* and *o-po-qo* = *opok^won*, and amended the spelling rule for initial *wr-*. The correspondence does not reveal what caused them to change their minds.

In December 1953 Ventris, Chadwick and Palmer agreed to write comments on the interpretation of the as-yet unpublished Pylos tablets discovered by Blegen that summer (whose text Bennett had sent to Ventris); to do so independently, and to send their individual work to Bennett before meeting to compare notes. Ventris writes (13 December 1953), 'I think it would show [him] how far we agree, and to bring Bennett into the group more closely.'⁹ While Ventris and Chadwick agreed closely, however, Palmer did not. Chadwick writes (3 February 1954), 'I shall be interested to see Palmer's reactions, as we seem to disagree with him so much; I hope he will not be too proud to admit he was wrong in some cases – after all he has a position to uphold, which neither of us have, so we can afford to make mistakes.'

This disagreement seems to have been the beginning of the schism, such as it was, between Palmer and Chadwick. Before this, other than the argument over *wi-ri-ni-jo* already discussed (and in which Palmer's views were eventually accepted in any case) there is little indication in the correspondence of any strife. In June 1953 Chadwick consulted him about the 'missing' *-w-* in *i-je-re-ja*, and Palmer was 'quite prepared to accept the view that the etymologists have been wrong' in deriving such feminines directly from the masculines in *-ευς*. 'Palmer's *e-re-u-te-re* is an ingenious idea', writes Chadwick on 5 December 1953. And in the letter quoted above outlining the procedure for communicating their views on the new Pylos tablets, Palmer is clearly one of 'the group' into which Bennett is to be brought more closely.

After February 1954, however, it is always Ventris who says things like 'Palmer suggested very brilliantly that *a-ro-* [is for *ἀρόματα*] or 'Palmer's *worneia* = lambswool is excellent'. Chadwick, on the other hand, when he reports (19 November 1954) that Palmer has given him two missing words on **PY Ad 691**, *e-ke-ro-qo-no* = *enkēro-k^woinoi* 'wage-earners' and *pa-wo-ke* = *par-worgoi* 'auxiliaries', adds, 'you may know them already – our liaison is not perfect over Palmer'. Clearly Chadwick knew that Ventris was in closer contact, and did not always report on it. On 9 February 1954 he writes, 'In writing to Palmer I ventured to

9 Ventris mooted to Chadwick the idea of getting Bennett to collaborate on the book which would become *Documents* in a letter of 24 June 1953, 'having in mind that his special contribution is his unrivalled knowledge of the signary and the originals'. Chadwick was receptive (letter of 26 June). Myres (letter to Chadwick of 27 July) says, 'The partnership of yourself, Ventris and Bennett is obvious and excellent' – and given the timing, we may surmise he means partnership in the authorship of the book, which presumably Chadwick mentioned in the 'long letter' to which Myres refers. Wace, too (letter to Chadwick of 8 January 1954), says that he hopes they will be able to get Bennett to cooperate. In the end, of course, Bennett was not involved directly in the writing of *Documents*. Ventris (letter of 2 June 1954) says, 'I feel he [Bennett] has been able to ventilate his ideas enough in this book [= Bennett (1955)] ... so that he will not have time to cooperate with us in detail.' Chadwick replies (8 June) that he is sorry that Bennett cannot be persuaded to cooperate, but adds, 'though I am not sure if we will not get on better without him, if he still wants to make all sorts of reservations [about the language being Greek]'.

question the validity of his argument from semantic parallels' – referring, surely, to Palmer's reconstruction of an inherited Indo-European feudal structure on the basis of etymologically unrelated terms with similar semantics in Mycenaean and Germanic, as expounded in Palmer 1954a and later in his inaugural lecture (Palmer 1955a) – 'I gather I have touched on a sensitive area.'

On three occasions (all after the Pylos disagreement) Chadwick accuses Palmer of plagiarism: 'I have noticed not infrequently that Palmer is quoted to me as the source of something I told him' (31 August 1955); 'I thought of *lawiaiai* quite independently, and you pointed out that Georgiev had it too; now I see Palmer is trying to make it his' (1 November 1955); and apropos of a paper on the *o-ka* tablets at the London Mycenaean Seminar, 'Almost 80% of it was precisely what we have printed in Documents (and I said so at the end, so I hope I'm not in his bad books now) ... [He] was obviously disconcerted to find he was not first in the field. How much could he have gleaned from us? I did discuss it with him in December.'

In reference to Palmer's (1955b) reaction to *The Mycenaean Tablets* (Bennett 1953), Chadwick writes (20 August 1955), 'there are masses of suggestions thrown out at random or on the flimsiest of evidence. Really if we do not apply more stringent standards we might as well give up the attempt, for everyone will devise his own system and idea of the texts. (Much as Andrews has done. Some frightful rubbish in his last letters; [PY]An 43¹⁰ is now scanned as hexameters.)' (Col. P. B. S. Andrews had begun corresponding with Chadwick in October 1953. Chadwick considered him to be 'some sort of crank'; Ventriss did not disagree, but slightly more generously allowed that 'he doesn't seem entirely clueless'. Having entertained him at lunch at the end of November 1953, Ventriss was less generous: 'He isn't entirely a prepossessing character, in fact he has a little of the fat-boy character of the school-yarn [sic]; what he does for a living I don't know, but I gather he does a lot of thinking on the lunatic fringe of archaeology. The trouble is, he knows rather a bit more philology than the average crank, and he's likely to come out with a reasonable result or two *malgré lui*.' Andrews went on to be a participant of the London Mycenaean Seminar, and develop a theory – which has gained no traction – that some of the undeciphered signs comprised a sixth vowel series. To compare Palmer with him is quite extraordinary.)

Whether or not Chadwick's criticism is justified can be judged from the following argument (Palmer (1955b) 36): a man called *te-wa-ro* appears on MY Oe 111, and a man called *te-wa-jo* on PY Fn 324 (then Fn 03).¹¹ Similar alternations are found between *ka-ra-e-*

10 This tablet is now referred to by the identifier PY An 519 (see n. 11 below). Far from being poetry, it is one of a set of documents which lists groups of men, referred to as *o-ka*, probably military in nature, stationed at various locations on the coast under the command of named individuals.

11 Palmer prints the references to these Pylos tablets in the original numeration of Bennett (1951), which Ventriss and Chadwick also use in Documents. While Documents was in production Bennett (1955) re-edited the tablets and printed them with a new numeration based on the original dig inventory numbers, but too late for Ventriss and Chadwick to amend their own text. For the convenience of the reader consulting Palmer's article, I have given here references in both systems.

i-jo on **KN Fp 345** and *ka-ra-e-ri-jo(-jo)* several times on the same series; between *ka-ra-u-ro*, **PY An 192** (then An 16), and *ka-ra-u-jo*, **An 424** (then An 09; the reading is now *ka-ra-ḏo-ro*); between *mi-ra-ti-ja*, **PY Aa 798** (then Aa 17), and *mi-ra-ti-ra*, **Ab 382** (then Ab 17); and *te-qi-ri-jo-ne*, **PY Fn 187** (then Fn 01), and *te-qi-jo-ne*, **Un 219** (then Un 11). From this it follows that Mycenaean scribes had difficulty rendering the Greek phoneme /l/, ‘a sound evidently missing from Aegean’, and so he is able to interpret *o-pi-i-ja-pi* (**KN Sd** series) as a compound whose second member is the Homeric word ἰλλᾶς ‘rope by which an animal is led’. However, the *te-wa-ro* at Mycenae and the *te-wa-jo* at Pylos (and also Knossos) are patently not the same man. If *mi-ra-ti-ja* is, as usually understood, *Milātiai* ‘women of Milātos’, then *mi-ra-ti-ra*, which is a hapax, if it is the same word, is clearly a spelling mistake (so *Documents*²). *ka-ra-e-ri-jo* is a month name at Knossos, and again the *ka-ra-e-i-jo* form is a hapax (and not an example of the alternation or *r-* and *j-*, unless in the absence of a sign *ji* the scribe has written *i*). *te-qi-ri-jo-ne* is a personal name, and *te-qi-jo-ne*, which is a hapax, looks more like a case of the scribe having mistakenly omitted the sign *-ri-*. The evidence for an *r/j* confusion is therefore reduced to the dubious example of *ka-ra-e-(r)i-jo*, and of *te-wa-ro* / *te-wa-jo*, provided that both are representations of the same name (even though it is not the same man). Chadwick succinctly describes the difference of opinion in a letter of 3 October 1955:

The difference of opinion between Palmer and myself over proper names is not superficial. To say that they provide the only evidence for significant points of language presupposes that they are correctly interpreted; and since any check on the interpretation is by their nature ruled out, then they are all guesses (more or less plausible) in *vacuo*. I do not believe ‘any significant points of language’ should be deduced from guess work; they can only come in at a later stage, when they may help to support a hypothesis already formed. I still cling very strongly to what I wrote about personal names in *Doc[ument]s*. Those who possess vivid imaginations will argue with me; but I think you are on my side, and I suspect a majority of reputable scholars too.

Chadwick anticipated Palmer’s hostility to the interpretations of the tablets in Part II of *Documents*. On 31 August 1955 he writes to Ventris, on the subject of compiling the index to the volume, ‘I think perhaps Palmer and Strabo should not be treated alike, because one can object; and I suspect the first thing the former will do is turn up his name to discover how much we accept and how much we reject of his suggestions, and write us an angry letter.’ Palmer’s reaction came not in an angry letter but in his review of *Documents* (Palmer 1957b)). After a fulsome justification of the decipherment – essentially a rebuff of Beattie (1956) – the criticism begins: ‘It is when we turn to the interpretation of the texts in the second half of the book that disappointment will be felt and even alarm’ (Palmer 1957b) 564). Then in discussing the interpretation of **PY Jn 829**, in a phrase worthy of Beattie himself, he says, ‘the authors have thrown caution and common sense to the winds’.

He accuses them of ‘using the spelling rules to ransack the lexica’, a method which, ‘though it occasionally yields results of self-evident correctness, is most dangerous’. Instead he insists that we should ‘establish the meaning of the syllabic groups as far as possible by the analysis of the contexts in which they occur’. He gives an example of the working of his ‘combinatory method’ (Palmer (1957b) 576ff.) criticising the Documents interpretation of the heading of **PY Ta 711**. The heading reads as follows: *o-wi-de*, *pu₂-ke-qi-wi-ri*, *o-te*, *wa-na-ka*, *te-ke*, *au-ke-wa*, *da-mo-ko-ro* (the sign *85 had not yet been identified as *au*). The first four sign groups are undisputed: ‘Thus/how Phugeg^wrins saw when the king ...’; nor is there any doubt that *te-ke* is *t^hēke*, the aorist of *τίθημι*, nor that *au-ke-wa* is a man’s name and the object of the verb. What, however, is *da-mo-ko-ro*? Palmer accuses Ventris and Chadwick of failing to attempt a ‘determination of the status of the word’, and merely asserting that it is a title equivalent to *ko-re-te* and *qa-si-re-u*. He observes that the same word is found on a ‘cloth’ tablet (**KN L 642**), and that ‘we ask in vain whether on the numerous CLOTH tablets there is a single instance of a title being found in the corresponding place of the structure’; and that it is also found on **PY On 300**, where it is parallel to the man’s name *te-po-se-u*. From this he infers that *da-mo-ko-ro*, too, must be a personal name, which he etymologises (by ransacking the lexica, for there is no other way with names) as *Dāmoklos*. It follows that since the man with the double-barrelled name *au-ke-wa* *Dāmoklos* is the sole object of *t^hēke*, the verb cannot mean ‘appointed’; and elsewhere (Palmer (1957a)) he takes it as ‘buried’.

The mistake is the same as that of the undergraduate who comes across an unknown word in a passage of unseen translation, guesses it – albeit with an educated guess – and then twists the meaning of the word (s)he does know to accommodate the guess. In this case the whole of the Ta series becomes a catalogue of grave goods for a man who clearly must be very important, and perhaps a member of the royal family. It is not only the purpose of the tablet which he reconstructs based on the guessed sense of *da-mo-ko-ro*, but details of the Pylian dynasty itself. It is also self-evidently dangerous to argue that a word cannot be a title because it does not occur in the same contexts as other titles when the corpus is so small; and indeed Olivier (1967) shows that on **KN C 7058** *da-mo-ko-ro* occurs in the same context as the known title *du-ma* on **KN C 1030**.

Compare this to Ventris and Chadwick’s approach (Ventris and Chadwick (1956) 335ff.). The forms *wa-na-ka* = *wanaks* and *te-ke* = *t^hēke* are evidently *wanaks* and *t^hēke*. The former is well attested. The latter must be a verb, since neither of the two remaining words in the *hote* clause can be, and *t^hēke* is the obvious form which fits. In any event, these two words are not disputed. The form *au-ke-wa* occurs in a list of names in **PY An 192.4** (the immediately preceding name, in .3, incidentally being qualified with the title *du-ma*), and from the order *au-ke-wa* is most probably the object of *t^hēke*. Of the senses of *τίθημι* which admit a person as their object, the local meanings (‘put, place’ etc.) are ruled out as they require a prepositional phrase or suitable adverb of place, and ‘bury’ is (probably) ruled out as requiring an overt phrase such as ‘in the tomb’. This really leaves the sense ‘make/appoint someone something’, which requires an attributive noun

or adjective and has good Homeric parallels. That means that *da-mo-ko-ro* must be the required attributive noun or adjective. Given that it appears to be a compound whose first member is *dāmo-*, it is easy to see how an official title fits the bill. It is true that Ventris and Chadwick do not explicitly justify their interpretation as a title. They do mention, however, that *au-ke-wa* recurs on **PY Jo 438.23** (then Kn 01) ‘as an official of places contributing gold’. One of the other men listed on this tablet is *te-po-se-u* – recall that his is the name that *da-mo-ko-ro* sits parallel to in **On 300**, which is one of Palmer’s pieces of ‘evidence’ that the latter is also a name – and here he is described, as are several other men, as being a *ko-re-te*, a well-known title of a local official. The parallels between *au-ke-wa* and *te-po-se-u* the *ko-re-te* on **Jo 438**, and between *da-mo-ko-ro* and *te-po-se-u* on **On 300**, are what justifies the statement that *da-mo-ko-ro* is similar in sense to *ko-re-te*. The information which allows one to reconstruct the justification for *da-mo-ko-ro* being a title is in fact there, if the reader cares to unpack it. Interestingly, Ventris and Chadwick erroneously state that *au-ke-wa* is found on **Jo 438.21** instead of .23; it is *te-po-se-u* who is listed in .21, and who they evidently had in mind when they wrote this comment.

It will be seen that the process of interpretation followed by Ventris and Chadwick is patently not one of ‘ransacking the lexica’. In fact, exactly like Palmer, they extract as much information as possible from the context(s) in which the words are found. The difference is that, like the skilled unseen translator, they use the portions they do understand to illuminate the portions they do not, and not, like Palmer, vice versa.

Palmer’s criticism (1957b, 574f.) of the Documents discussion of **PY Tn 316** (= Ventris and Chadwick (1956) no. 172, listed under its old classification as Kn 02) is interesting, because the correspondence allows us to trace the development of Ventris and Chadwick’s thought on the tablet. The text is reproduced below as given in Documents, with the sides in the order given there, but with modern editorial conventions. The side labelled by Documents as ‘Reverse’ is the flatter side. The case for supposing the more curved side, Documents’ ‘Obverse’, was written first is that lines have been ruled only on the top half of the ‘Reverse’, which also shows signs of having been erased. The supposition is therefore that the scribe originally wrote a different text on the flat side, as normal, which he erased; he then turned over the tablet and wrote a new text beginning on the curved side, which he filled to the point where he needed to turn over and continue on the flat side. The case for taking the flat side as written first is that it bears the heading *po-ro-wi-to-jo*, taken to be the name of the month *Plowistos* (Palmer at the London Seminar on 26 May 1954), and since Bennett (1955) the flat side has been called the recto, the curved side the verso. In view of this potential confusion, in the following discussion I propose here to refer to the two sides as curved and flat and label their lines .c1, c.2 etc. and .f1, .f2 etc.

PY Tn 316 (ex Kn 02, text as printed in Documents p. 286)

‘Obverse’, now ‘verso’ (curved side):

.c1	{	i-je-to-qe , po-si-da-i-jo , a-ke-qe , wa-tu
.c2		do-ra-qe , pe-re , po-re-na-qe , a-ke
.c3a pu-ro	}	-ja
.c3		AUR *215 ^{VAS} I MUL 2 qo-wi-ja , [. .] , ko-ma-we-te-

.c4	{	i-je-to-qe , pe-re-*82-jo , i-pe-me-da-ja-<jo?>-qe , di-u-ja-jo-qe
.c5		do-ra-qe , pe-re-po-re-na-qe , a-<ke> , pe-re-*82 AUR+*213 ^{VAS} I MUL I
.c6 pu-ro	}	i-pe-me-de-ja AUR *213 ^{VAS} I di-u-ja AUR+*213 ^{VAS} I MUL I
.c7		e-ma-a ₂ , a-re- ja AUR *216 ^{VAS} I VIR I

.c8	{	i-je-to-qe , di-u-jo , do-ra-qe , pe-re , po-re-na-qe a-kè
.c9		di-we AUR *213 ^{VAS} I VIR I e-ra AUR *213 ^{VAS} I MUL I
.c10 pu-ro	}	di-ri-mi-jo di-wo , i-je-we , AUR *213 ^{VAS} I [] vac.
.c11		vacat

.c12 angustum

.c13	{	vacat
.c14		vacat
.c15 pu-ro	}	vacat
.c16		vacat

reliqua pars sine regulis

‘Reverse’, now ‘recto’ (flat side)

.f1	po-ro-wi-to-jo	
.f2	{	i-je-to-qe , pa-ki-ja-si , do-ra-qe , pe-re , po-re-na-qe
pu-ro	}	
.f3		a-ke , po-ti-ni-ja AUR *215 ^{VAS} I MUL I

.f4	ma-nà-sa , AUR *213 ^{VAS} I MUL I po-si-da-e-ja AUR *213 ^{VAS} I MUL I
.f5	ti-ri-se-ro-e , AUR *216 ^{VAS} I do-po-ta AUR *215 ^{VAS} I

.f6 angustum

.f7	{	vacat
.f8		vacat
.f9 pu-ro	}	vacat
.f10		vacat

reliqua pars sine regulis

It is clear that the tablet records offerings of gold (and it is now thought also silver) vessels, and possibly men and women, to various divinities – among them *i-pe-me-de-ja* = *Ip^hemedēiāi* and *di-u-ja* = *Diwyāi* in .c6, *e-ma-a₂* = *Hermāhāi* in .c7, *di-we* = *Diwei* and *e-ra* = *Hērāi* in .c9, *po-ti-ni-ja* = *Potniāi* in .f3 and *po-si-da-e-ja* = *Posidāēiāi* in .f4. That the offerings take place at various shrines (*po-si-da-i-jo* in .c1, *pe-re*-*82-*jo*, *i-pe-me-da-ja*-<*jo*?>, *di-u-ja-jo* in .c4, *di-u-jo* in .c8, all derived from the names of their associated divinities) is shown by *pa-ki-ja-si* in .f2, which is clearly locative. The tablet is ruled into sections introduced by the word *pu-ro* written in large signs. The last section on each side has *pu-ro* but is otherwise blank. Each completed section begins with the formula *i-je-to-qe* ‘name(s) of shrine(s) *do-ra-qe*, *pe-re*, *po-re-na-qe*(_s), *a-ke*, except that .c1–2 has *a-ke-qe*, *wa-tu* inserted after the shrine name *po-si-da-i-jo*. The word *do-ra* is presumably *dōra* referring to the vessels, *pe-re* and *a-ke* look like parts of *φέρω* and *ἄγω* respectively used of inanimate and animate offerings, which means that *po-re-na* may be a word referring to the men and women listed. Little more than that is clear, even today.

Although *i-je-to* looks like a form of the verb ἵημι (middle/passive *hiento(i)ʔ*), as Ventris and Chadwick (1956, 284) note, the fact that *pa-ki-ja-si* is locative makes it difficult to associate it ‘with the sense “send to”, and [it is] preferable to take it as the description of an activity which takes place at the different localities’; they record Palmer’s suggestion of a verb ‘to sacrifice’ connected with *ἱερός* rather than with ἵημι (though homophonous with it). If *i-je-to*, *pe-re* and *a-ke* are middle/active, then they note that it is difficult to find them a subject. Palmer took *pe-re* to be *p^hrē*, the aorist passive of *φέρω* and *a-ke* as *agē*, aorist passive of *ἄζομαι*, ‘ritually purify’. Ventris and Chadwick note the suggestion of tense (but not of meaning; this may be what causes Palmer to complain, p. 574f, that ‘VC misrepresent my views’ – it is not clear in fact that they do). In any event, Ventris and Chadwick do not accept Palmer’s interpretation, but they do not say why. We may surmise from what is said, viz. that if *i-je-to* is indicative it can only be present or imperfect, that the change of tense from present/imperfect to aorist is unacceptable. They also observe that in Homer the *-η* formation ‘is generally regarded as having been restricted to intransitives’, but allow that there may be some ‘specifically passive forms’ in Mycenaean. In their translation Ventris and Chadwick (1956, 287) adopt a third possibility, viz. that *i-je-to*, *pe-re* and *a-ke* are imperatives, but do so ‘without any strong conviction’, and noting the absence of imperatives elsewhere. The latter two both mean ‘bring’ for inanimate and animate offerings respectively, but for *i-je-to* Documents is cautiously vague, printing ‘perform a certain action’ (the italics, which are original, indicating the uncertainty of the authors).

Now Palmer (1957b, 574) evidently believes that Ventris and Chadwick had accepted his views that (a) the tablet is a calendar (a possibility that Ventris and Chadwick (1956) 286 do record), and (b) that it therefore follows that since *i-je-to* cannot, because of the locatives, mean ‘send’, it cannot be from ἵημι. ‘All this was accepted by VC’, he says, ‘but their original version of *i-je-to* survives in the Vocabulary, where it is discussed as some part of ἵημι “send”. The resultant diagnosis of the document as part of a calendar of offerings was not followed up. But if VC agree that *i-je-to* represents some action performed in a

shrine and recorded in a calendar of offerings, the verb must have some religious significance.’ All of this is adduced in support of his statement at the beginning of this section of his review that ‘the authors have fallen between two stools. There is clear evidence that a change of position occurred while the book was in the making, and there has been imperfect adjustment.’

Ventris’ original interpretation of the formula (presented at the London Seminar on 15 April 1954) had been ‘Let that which is Poseidon’s be released (from the store of precious vases) [= *i-je-to*]; and take (people) from the town, and take (them) to carry the gifts in procession.’ Late in the day, Palmer thinks, Ventris and Chadwick abandoned this interpretation in favour of his, but did not fully adjust what they had already written in *Documents*. As mentioned already, the correspondence allows us to track the development of their thoughts on this tablet.

Ventris and Chadwick’s early attempts to make sense of the tablet while writing up ‘Evidence’ in 1952 were hampered by the word divider not having been spotted in *pe-re*, *po-re-na-qe* in .c2, .c8 and .f2. Chadwick wondered whether it might be a compound in *περβολ-* or *φερε-* but admitted the word was ‘quite beyond’ him. In October he spotted that *Δῦφία* is the name of a goddess in Pamphylian, but the form *di-u-ja-jo* in .c4 baffled Ventris, who thought that *pe-re-**82-*jo* and *i-pe-me-de-ja* were genitives of the divine names. He therefore also proposed that the vowel of *82 was *o* to give an *o*-stem genitive in *-o-jo*. Chadwick spotted that *di-u-ja-jo* was an adjective *Diwiaios* from *Diwia* similar to *Potniaweios* from *Potnia*, but rejected *po-se-da-i-jo* as a derivative of Poseidon, preferring to see a compound of *posi-* and *δοτέομαι*. ‘I still don’t feel we have really solved Kn 02’, he wrote on 20 October, ‘though bits of it seem to make sense. I don’t like the imperative tone; and there are still too many words which are not quite right.’

Ventris, extraordinarily, suggested amending the text so that it makes sense, either by changing *i-je-to* to *i-je-ro* = *hieron* in agreement with the ‘divine adjectives’ (he had by now abandoned the idea of genitives), or by changing *pe-re-po-re-na* to ‘*pe-re-po-re-wa?* = *περφερέες* “gift carriers”?, parallel object to *do-ra*’ (and the interpretation ‘gift carriers’ would survive for *po-re-na* once the word divider was discovered); but he was wary of emending words which were repeated. Chadwick wisely advised waiting to see a photograph first. On 12 November Chadwick wrote, ‘I w[oul]d prefer the abandonment of all attempts to interpret Kn 02, and merely give the names.’

In June 1954 Ventris was working at the British School in Athens and autopsied the Pylos documents. It was then that he abandoned the idea that *wa-tu* was really *wa-tu-de* (to enable a sense ‘bring to the town’), and saw that *pe-re*, *po-re-na* had the divider. The tablet was not discussed further until *Documents* was in preparation.

The two authors divided the first responsibility for the various chapters and sections of *Documents* between themselves. The author responsible for each section typed a draft and sent it to the other for comment. The revised draft reflected the agreed outcome of discussion, or recorded the agreement to disagree. This is what Chadwick meant when he wrote that it is ‘hardly possible to apportion responsibility’ for any section to one author or the other. Chapter 9, ‘Proportional tribute and ritual offerings’, fell to Ventris

in the first instance. It was originally proposed (Chadwick, 8 June 1954) that the Vocabulary should be ‘a joint effort, as it is basic to the whole interpretation’. In the end Ventris took responsibility for compiling the card index and typing it up. The indexing was completed on 25 November 1954, and the typed version was sent in duplicate for Chadwick’s scrutiny on 4 December. The spare copy was to serve for sending back and forth with amendments, and both authors were to keep it under ‘constant check’ as they wrote and revised their sections for Part II. Additionally Chadwick undertook a final revision of the manuscript before it was submitted to the Press on 7 June 1955 (an event recorded in Chadwick’s famous postcard to Ventris written in Linear B).

Ventris had therefore compiled the initial Vocabulary entry for *i-je-to* by the end of 1954, some two months before he began to write the relevant section of Chapter 9. On 8 February 1955 he wrote to ask Chadwick’s views ‘about Palmer[’s interpretation] in general’, and in particular on *i-je-to*, and the tenses, voice and subjects of the various verbs, and requested a ‘token translation (lots of ?? allowed!)’ of the first two lines of the curved side. Chadwick replied by return:

The facts that are clear to me are: (a) this is a religious context, (b) a number of deities are mentioned by name, some of whom are familiar from Greek, but some are not – there must have been a lot of local heroes of whom we know nothing, (c) that the offerings are gold vessels and human beings. On the score of probability, I should think the chances of *a-ke/pe-re* being some part of *ago* and *phero* overwhelming; Palmer wants to connect *a-ke* with *hagios* because of his interpretation of *i-je-to*. I am rather sceptical of his new verb **hiemi*, if only because it would have clashed with the regular *hiemi*; but of course you can say this is why it never survived. If *i-je-to* means ‘send’, it might be middle rather than passive, and the number could be singular. Could we then translate: ‘The priest of Poseidon sends (as his own offering) and is bringing to the city (the following): he is bearing gifts and bringing — ... etc.’ Past tenses might be more suitable, but can *i-je-to* be anything but present or imperfect? In line 4 I rather suspect that we have a wrong form and it should be read *i-pe-me-de-ja-jo-qe*. *a* is fairly clearly wrong for *a-ke*. I’m far from thinking this really solves the problems, and I may be barking up quite the wrong tree; but how else can you fit in the *di-u-jo/di-we*, *pe-re*-*82-*jo/pe-re*-*82? If you make it the temple (or better shrine) you are left entirely without a subject; unless *i-je-to-qe* is really a noun! I like the suggested *dospotāi*; I’m not so sure that *di-we i-je-we* is our old friend *di-wi-je-u*. No views at all on *po-ro-wi-to-jo*, except that I should prefer it to mean ‘cont[inued]’.

Now this is a year after Palmer gave his own views at the London Seminar in February and May 1954. At this stage Chadwick does not accept *po-ro-wi-to-jo* as a month name (though Documents would) and so implicitly does not necessarily accept that the tablet is a calendar – a view he repeats on 21 February (‘I think you are rash to say that only a calendar will explain the blank lines in Kn 02; I can think of other (though less convincing) explanations’) and 9 May (‘I don’t

go for the calendar’); and *Documents* would print an alternative explanation, viz. that all the processions took place on the same day, and the blank entries represent two which were expected but which did not happen. He does not accept Palmer’s verb < *isH-, nor *phrē* and *agē*, but still sees a form of ἵημι. He does not think Palmer is inventing a new verb ἄγω (cf. ‘VC misrepresent my views’). If *i-je-to* is indicative it can only be present or imperfect, and so *pe-re* and *a-ke* cannot be aorist. Nor does he think they are imperatives, and so takes *po-si-da-i-jo* etc. – despite the obvious toponym *pa-ki-ja-si* – as masculine adjectives to provide the missing subjects.

Ventris subsequently drafted his section on **Ta 316** and sent it to Chadwick on 16 February. (A letter of 10 February says he is sending a ‘token piece’ of Chapter 9, which does not include **Ta 316**; the letter of 16 February says ‘another chunk of Chapter 9’ is enclosed, and there will be more to come ‘dealing with the Knossos **Fp**- series etc’. This ‘chunk’ must therefore contain the pages for **Ta 316**, and the Pylos **Ma** and **N**- and Knossos **Mc** series). Chadwick did not read this section until May.

On 9 May he writes, ‘I am alarmed to discover how far our views have diverged on KNor [sic; sc. Kn 02] ... I feel I must ask you to revise some parts of your translation and commentary, especially after seeing the original. It is undoubtedly one of the most important texts, and we shall to some extent be judged by what we make of it ... I can’t think of any interpretation which is not free from grave objections; I have a strong suspicion that some more emendations will be necessary to make sense of the text.’ Amongst his criticisms are:

- (1) Ventris has given *i-je-to* a sense ‘let flow’ which is ‘frankly impossible – I would rather have Palmer’s invention’. This occasions the anodyne ‘perform a certain act’, to which Ventris first changed it; Chadwick later commented that this ‘has a vaguely indecent ring to it’, leading to the printed version ‘perform a certain action’.
- (2) Ventris has rendered *qo-wi-ja* ‘cow-girls belonging to Komawens’. Chadwick comments ‘this isn’t Hollywood!’, and suggests that like *ko-ma-we-ti-ja* it is the name of a local divinity (‘a sort of Pasiphae?’).
- (3) *po-re-na* must be a noun; on the evidence of *te-re-ja-e* (and we may add *e-re-e*) the infinitive of a ‘contract’ verb *p^horeō* ought to be *po-re-e*.
- (4) Chadwick is ‘completely opposed to the idea of imperatives on the tablets; if commands are expressed they are recorded as facts (How the watchers are guarding the coast; this much E. will give, etc.)’.

Ventris revised the section immediately and sent the new version the next day, 7 May. In the accompanying letter he says:

I had better produce you a new version of Pages 10–13 to insert into Chapter 9. I don’t guarantee that we shall come to any agreement, but at least I’ll phrase it to take proper account of your own views ... On the question of the syntax of the introductory formula of Kn 02 I despair of producing a version which will properly hold water.

But until I get from you an alternative translation (which I'd like to have anyway for the record, seeing that we disagree), I'm inclined to plump for my imperatives as at least giving some sort of sense ... Some of your other objections are pretty conclusive, and I will take evasive action.

A postscript explains that '[a]s the Sunday post won't get to you any faster if I leave this till this afternoon' he has had time to produce and enclose the revised text.

Chadwick's reply, 9 May, is conciliatory. 'I feel caddish', he says, 'for being so destructive when I have nothing convincing to offer in its place.' He declines to offer an alternative translation, and says, 'I should prefer to suppress our disagreement if you are not wholly convinced of the imperatives.' The revised draft had said that the imperative interpretation was adopted 'by Ventris', which two words were subsequently dropped; and on Chadwick's suggestion the caveat that imperatives had not been identified elsewhere was added. Chadwick again repeated that he didn't 'go for the calendar' and offered the alternative that 'this is a hasty record jotted down as the processions arrive at the palace from the surrounding shrines; hence some lines were left blank for other gifts which were expected to materialize and didn't; but I don't feel sure enough to state this so boldly in print – at least without your agreement'. This suggestion underlies the printed sentence 'An alternative explanation of the blank entries might conceivably be that the tablet records a series of processions all occurring on the same day, but that some of those for whom provision was made *failed to materialize*' (my emphasis).

Palmer is right, therefore, that the translation and commentary on **Tn 316** were changed, but he is wrong in the particulars. The change did not happen especially late in the day, but in the ordinary run-of-the-mill process of ironing out disagreements between the two authors—of which this exchange is a particularly clear example. The initial disagreement may have been unusually strong; and it is clear, I think, that Chadwick remained wholly unconvinced of the imperatives and that the agreed text is a fig leaf for his continued disbelief. But it is untrue for Palmer to say that Chapter 9 accepts his views: it mentions, but does not endorse, his interpretation of *i-je-to*; and since his interpretation of *a-ke* depends upon his version of *i-je-to*, which Ventris and Chadwick do not accept, there is no need for them to mention it. As we have seen, Chadwick is well aware of the verb Palmer thinks *a-ke* is from. Likewise the idea of a calendar is mentioned, but not endorsed. Indeed, Chadwick's letter of 9 May observes that Palmer's comment (1954a, 53) '[t]hat Pylos was in a state of emergency before its final destruction is suggested by an interpretation of the difficult tablet Kn 02, which records a series of offerings to a number of divinities' rather precludes Palmer himself from thinking it is a calendar.

Will the charge stick at least that the text of Chapter 9 and the Vocabulary reflect different stages of the authors' belief? Here is the Vocabulary entry as printed:

ḫi-je-to, PY 172 = Kno2. Tense uncertain, see p.285. *hietoi* or *hientoi* 'is, are being sent'? *hietō*, etc. 'let it, them, send' or 'be sent'? [ἦημι Il. 1.479 + .]

In setting out his vision of the Vocabulary, Ventris writes (20 November 1954), ‘I feel the main discussion of the possible meanings and forms should appear in the Part II sections ... The vocabulary ... will only have the bare bones of the possible identifications, referring by page reference to the fuller discussion.’ That is exactly what the entry for *i-je-to* does. It is clear from the preceding discussion that Ventris and Chadwick rejected Palmer’s verb connected with *ἱερός* and continued to believe that *i-je-to* was a part of *ἱημι* even if the specific sense ‘send’ was inappropriate in the context. But they could hardly print ‘perform a certain action’ in the Vocabulary. Instead they print a queried “send” or “be sent” and refer the reader to the discussion in Chapter 9. And this seems to me a perfectly legitimate thing to do, because if it is part of *ἱημι* its sense must somehow derive from the basic meaning ‘send’. Although the Vocabulary entry was written before Chapter 9, entries were continuously revised as the sections of Part II were written.

Now it is true that the double obelos is used in the Vocabulary to mark ‘those forms which can, allowing for historical evolution, be directly equated with Homeric or classical forms, and have corresponding meanings which fit the context of the tablets with virtual certainty’. It is bold to claim that this is true of *i-je-to* when it is rendered ‘perform a certain action’ in the text. Is the † the ‘smoking gun’ which proves Palmer’s accusation? Possibly; but I find it hard to believe that it has been left over from a previous iteration, not only because of the ‘constant check’ to which the Vocabulary was subjected, but also because of the cross reference to page 285. Because the proofing was conducted with page proofs from the outset, which were sent in small batches, corrected and returned, the page references were inserted at the revised proof stage. The offending † could easily have been struck when the page number was inserted. It would have had no knock-on effect on pagination etc.

In any event, the ‘difference of opinion’ was sufficiently great that Chadwick agitated that Ventris and not Palmer should be invited to give the J. H. Gray lectures in 1957. On 20 December 1955 he writes to urge Ventris to accept the invitation, which he had won ‘by a short head with Palmer as runner up; so if you refuse, he may be asked’. Then in his last letter to Ventris, of 27 July 1956, he says, ‘Gray Lectures. I hope you are going to do this because I was told that Palmer might be asked if you didn’t. I shall in any case be in a position to sabotage anything on the tablets as I have eight lectures to give in the Michaelmas Term.’

Ventris himself became exasperated. In November 1955 Palmer circulated an attack on the Ventris–Chadwick interpretation of the **PY Ta** series, expounding views which would become Palmer (1957a). On 5 December Chadwick wrote to Ventris saying, ‘I take it you are holding firm in the face of the circulated attack.’ Ventris replied (6 December), ‘I was thinking of typing a reply to Palmer, but haven’t the time or energy.’ He enclosed three pages of a foolscap manuscript, heavily revised and annotated, which address the criticism of the word *a-mo-te-wi-ja*, which Ventris and Chadwick interpreted ‘of the harmost’s type’, denoting a quality of vessel, and Palmer understood as *hamotēwīā* ‘decorated with a chariot’. He told Chadwick, ‘make what use you like with it’.

This letter marks the first suggestion that Ventris may be tiring of Linear B. Two weeks later (21 December) he writes that he will be starting ‘a year’s architectural research job beginning in January’ (the Architects’ Journal research fellowship, which he later abandoned). He assures Chadwick that it will not stop him attending the colloquium in Paris, nor slow down his share of the proof-reading of *Documents*. ‘But it means I shan’t be able to devote time to other major commitments. Once the two present pieces of typing are done [the Vocabulary and Bibliography for Paris], there’s not much for me to do anyway except argue with Palmer, and that comes better from you.’ Chadwick replies on New Year’s Eve, ‘I really thought you had abandoned architecture for archaeology . . . I hope I can still count on your support for the duels with Palmer and others.’ Ventris never addresses that point in the correspondence.

It is easy to construct from this a narrative such as that of Robinson (2002), who paints a picture of an increasingly depressed Ventris until ‘[o]nce again, as in 1948 when he abruptly cut off from Sir John Myres’s publication of *Scripta Minoa*, Ventris’s inner conflict had surfaced disruptively. In the final year of his life, it would become even more acute’; and then in the next chapter, ‘Triumph and tragedy’, he hints that Ventris’ death may have been suicide. But Robinson’s version of events is based on misconceptions caused in part by the fact that the third file of correspondence, from 1956, was unavailable to him.

The typing of *Documents*, he says, was undertaken by Ventris ‘partly on a Varityper at University College London’. This is incorrect. The Varityper, an electric typewriter with interchangeable font shuttles, produced copy ready for reproduction by litho-plates, stencils or photo-offset printing; there would have been no point in using one to produce the typed manuscript of *Documents*, which was set in hot metal by Cambridge University Press (with specially made blocks for the ideograms and other Linear B glyphs, which a Varityper could not have produced). As we have seen, the individual chapters and sections of *Documents* were typed in draft by both Ventris and Chadwick individually, and the author responsible for the initial draft retyped the version which was submitted to the Press.

In December 1955 UCL borrowed a Varityper and housed it ‘in a spare corner’ for Ventris to produce the reproduction-ready copy of the *The Knossos Tablets* (Bennett, Chadwick and Ventris (1956)). Work by Ventris, Chadwick and Bennett in the Iraklio museum had revealed that there were numerous errors in the ‘Browning Version’ of the Knossos tablets (Browning (1955)), which had been produced from the drawings and photographs in *Scripta Minoa II* and Ventris’ drawings of some of the fragments in the range 5000–6068. A further series of fragments, labelled 7000–8075, had been discovered by Chadwick in the museum in 1955, and it was evident that action of some sort was needed to bring the ‘Browning Version’ up to date. Ventris originally envisaged producing a volume of addenda and corrigenda. He writes in November 1955 that producing a new edition from scratch ‘would be even more of a hell of a job than this is going to be’, but at the beginning of December, after calculating that a complete edition would contain 3,603 entries, as opposed to 2,625 if only corrections and additions were included, he persuaded Turner at the Institute of Classical Studies that a complete edition was a

worthwhile enterprise. This he began in December 1955 and completed the following January, and it is this work which Robinson conflates with the typing of *Documents*.

On 6 December Ventris complains, 'It's quite a job, as I am having to check through about six lists & books simultaneously.' According to Robinson this was said in connection with the production of the index of sign groups for the Paris Colloquium the following year – a meeting which Robinson 'senses Ventris's heart was not in' – a claim which comes in a paragraph which begins with Ventris complaining about Palmer 'frittering our whole energy away in discussion', and moves – via 'I have the feeling that 95% of our time gets used up on administration' and Palmer 'showering both Turner and Webster with rather magisterial missives' – to the letter of 21 December when he announces his *Architects' Journal* fellowship. To claim this as a narrative of increasing disenchantment leading to withdrawal from the field is very misleading.

The complaint of 6 December is certainly made in the context of *The Knossos Tablets*, not the index and bibliography for Paris; and Robinson crucially omits the conclusion of the sentence 'but it's well worth it'.

The comment about 'frittering our whole energy away in discussion' is a misquotation. On 5 October 1955 Ventris actually writes:

Palmer writes to me today, and incidentally regrets not being able to shove some of his pet personal names in. I propose to continue adamant about postponing Index D at any rate until the rest is in good shape, or we shall fritter all our energy away in discussion.

'Index D' was a proposed index of personal names which Lejeune had also requested be compiled for Paris. In phone calls with both Chadwick and Palmer it had been agreed that this index was unnecessary and, given its size, impractical, and that it should be abandoned. Nobody is 'frittering [their] energy away in discussion', let alone Palmer. Palmer regrets the loss of an opportunity to promote some of his interpretations of personal names. The 'frittering away' of energy is a hypothetical in the event that Index D were to be compiled. The lament about administration is aimed, if at anyone, at Lejeune, whose idea the indices were. Palmer was a collaborator in their production; and the 'magisterial missives' with which Palmer showered the London Seminar organisers were in connection with his interpretation of the Pylos Ta series, and have nothing to do with the Paris meeting at all.

What allows Robinson to spin this narrative together is his mistaken but understandable belief that Ventris' letter of 21 December was his last. As Bendall (2005) says, the file of the Ventris–Chadwick correspondence for 1956 had not been discovered in the archives of the Mycenaean Epigraphy Room when Robinson was researching his book; but he knew that Ventris had attended and been fêted at the Paris meeting, and his description of the events does not support his earlier claim that 'Ventris's heart was not in [it].' Some of the 1956 letters could be read in such a way as to fit the story. On 2 June 1956, for example, in addition to listing some readers' queries on the proofs, which Ventris

answers, Chadwick expresses surprise on discovering that Ventris has included in the Vocabulary words from the as-yet unpublished tablets excavated at Pylos in 1955, and asks whether Bennett knows and approves. Ventris replies, 'If you feel very strongly, delete: but I don't expect to write much more on Myc., and it's a great pity not to have the thing complete. I'll carry the can if necessary' (my emphasis).

But although Ventris writes less frequently in 1956 than before – there are only thirteen letters to his last on 13 June, most are no more than one side, and they largely summarily deal with final corrections to Documents – they do not show a declining interest in Linear B. On 22 January he lends Chadwick his copy of Bennett (1955), but wants it back as soon as Chadwick's own arrives; and he mentions that he has just corrected the proofs of his Eranos article. On 20 February he looks forward to doing an article for the *Times* to coincide with the Paris meeting, and asks what he might talk about in the J. H. Gray lectures; two days later he is 'inclined to accept' the invitation to give them. On 28 May he says he will attend the next meeting of the London Seminar.

Most tellingly, perhaps, on 13 June Ventris responds to a suggestion from Chadwick that in the Mycenaean 'perfume' tablets discovered in 1955, *wo-do-we* was *wordowen* 'rose-scented', and *pa-ko-we* was connected with *σφόκος* 'sage'. Ventris says, 'I had got as far as *σφακόεν* myself – far from having given up interest, he, too had been working on the new material. He then questions two points of etymology.

It would be foolish, I think, to read too much into the brevity of Ventris' 1956 letters. He was, after all, working on the *Architects' Journal* fellowship (as well as taking an extended holiday in March), and the main and pressing business was the correction of the revised proofs of Documents and the compilation of its index. Although no letters from Ventris to Chadwick survive after 13 June, it is clear that the two men remained in contact. A letter from Chadwick dated 21 July thanks Ventris for sending him 'a typescript of H. Lambridis' article', which guarantees at least one more letter after that of 13 June, and reveals that he, Ventris and Palmer had intended to collaborate on future instalments of *Studies in Mycenaean Inscriptions and Dialect*, with Ventris taking care of Slavonic and Scandinavian literature, and being responsible for editing.

There is no mention of Ventris' withdrawing from Mycenaean work in Chadwick's letters between 31 December and his last letter, of 27 July, which reveals that Ventris had been trying to contact Chadwick by telephone, apparently over some last-minute problems with the book. The rest of the letter is mostly concerned with distribution of free copies (Palmer to be a recipient). He also hopes that Ventris will agree to give the Gray lectures, and says:

I am sorry to gather from some of your remarks that you are intending to cut down your work on Mycenaean in future. Of course there will not be so much or such exciting work; but I in particular should be very sorry if we could not continue to collaborate. You no doubt realise that we (and you in particular) will be much in demand when the book gets out.

Given that Chadwick had not mentioned this since December, perhaps Ventris raised it again in their telephone call. Perhaps he had also expressed doubts about the Gray lectures. Chadwick's letter is not incompatible with that view, but there is no way we can know. Chadwick's last words are on his expected future movements – he will be away for ten days from the middle of August, and again a month later – and to wish Ventris a good holiday. As Bendall (2005) says, this implies, at least, that they expected to speak again. If there had been a psychological crisis, it is not at all evident in Ventris' correspondence with Chadwick. Of the import of his letter of resignation from the *Architects' Journal* fellowship, let others speculate: $\beta\omicron\upsilon\varsigma \ \acute{\epsilon}\pi\iota \ \gamma\lambda\omega\sigma\sigma\eta \ \mu\acute{\epsilon}\gamma\omicron\varsigma \ \beta\acute{\epsilon}\beta\eta\kappa\epsilon$.

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