

ΕΣΤΙ ΠΥΛΟΣ ΠΡΟ ΠΥΛΟΙΟ

It is perhaps inevitable that in a field as restricted as Mycenaean studies it is impossible to move without treading on someone's toes. Certainly I am conscious of having mine very nastily jarred by the new study by S. Hiller¹ of the geography of the Pylian kingdom, a subject which has occupied my attention for many years past². My last publication was too late to appear in more than a footnote in Hiller's book, and there is another still at the time of writing in the press. Had he come to conclusions similar to mine, I should have welcomed them; had he convinced me I was wrong, I would have cheerfully confessed my errors. But I regret to say that I believe Hiller to have made a few fundamental errors, and as a result to have reached conclusions very different from mine. What is useful about such a book is that it compels one to stop in one's tracks and go back to the basic points long since established, to see if perhaps the wrong choice was made at a distant fork, and thus to re-open the question from that point. Obviously, if my basic decisions, especially that governing the northern frontier, are wrong, almost all my work since is worthless; equally if Hiller is wrong, a large amount of his work is worthless. I propose therefore not to write a review, but to examine his arguments in detail to see if I can still support my own opinions.

The book divides very clearly into two parts: the Mycenaean and the Homeric geography. I agree with Hiller that the two need to be treated separately, though I differ about how they are to be reconciled. I begin therefore with the Mycenaean section.

¹ *Studien zur Geographie des Reiches um Pylos nach den mykenischen und homerischen Texten.* Österreichische Akademie der Wissenschaften, Veröffentlichungen der Kommission für mykenische Forschung, herausgegeben von Fritz Schachermeyr Wien 1972.

² *Documents in Mycenaean Greek*, pp. 139-145; «The Two Provinces of Pylos», *Minos* 7, 1963, pp. 125-141; *The Minnesota Messenia Expedition*, ed. W. A. McDonald and G. R. Rapp, Minneapolis 1972, pp. 100-116.

The tablets found in the Mycenaean Palace of Ano Englianos abound in place names, but the writers failed to include a map to locate them for us. We have two problems: to equate the Linear B spelling with a known or at least plausible Greek name (e.g. is *ko-ri-to* to be interpreted as *Κόρινθος*?); and to identify the geographical site to which the name refers. If we can solve the first problem, this may solve the second, though owing to the regrettable habit that names have, Greek no less than others, of wandering in the course of centuries, even this is often doubtful. The name of Πύλος was plainly that of the Palace where the tablets were found; but from there it had moved by the fifth century B.C. to a site about 8 km. away at the north of the Bay of Navarino; and it has now migrated even further south to the other end of the Bay. The same may well be true of any name which appears both on the Mycenaean tablets and in the historical sources. Indeed, we might observe that many Mycenaean names (e.g. Pisa, Lousoi, Leuktron, Erkhomenos) later appear in other parts of the Peloponnese, either because they were common appellatives of the pre-Greek population, or because they were carried thither by migrants from Messenia after the destruction of the Palace and the depopulation which distinguishes the LH III C period.

It needs to be recognised that any attempt to locate the Mycenaean names on a map must depend upon a series of linked hypotheses. Any one of these conclusions may be wrong, and when one hypothesis is built upon another, the doubts have to be multiplied, not added. We can only check our results by continually comparing them with the physical landscape of the south-western Peloponnese. I regret to say that I find in Hiller's book many places where this factor seems to have been disregarded. For instance, he transfers men for coastguard duty on the north shore of the Messenian Gulf from a hypothetical site east of Olympia; it would be interesting to know what route they took through the mountains of western Arcadia —there are few possibilities— and why the rulers of Pylos should have organised their kingdom into two narrow strips running north and south, instead of into the naturally cohering geographical units

We need too to keep in full view the results of the exhaustive surface survey of the south-western Peloponnese by W. A. Mc

Donald and R. Hope Simpson³. Where their map is blank, they have sought for Mycenaean sites in vain; and in fact a very clear pattern of settlement emerges. There is a prominent cluster of sites around the lower Alpheios valley, with a few outliers north and south on the coast; another centering on the Kyparissia river valley, again with outlying sites up and down the coast; a group around Englianos and the hinterland of the Bay of Navarino; and a group in the Pamisos valley, occupying the heartland of Messenia, again with outliers on the coasts. Quite apart from any ancient evidence, we could predict that these are the principal areas to be considered in determining the extent of the kingdom administered from Pylos. The relative absence of known Mycenaean sites in Arcadia may of course be due to lack of sufficient exploration⁴; but it is clear that the Mycenaeans preferred to settle within sight of or at least easy reach of the sea, and until the existence of sites has been demonstrated in the Arcadian mountains, it would seem undesirable to consider this area as a strong candidate for inclusion in the Pylian realm.

Thus while Hiller agrees with me that the more remote of the two Mycenaean Provinces lies east of the mountain range of Aya which is such a prominent feature of the landscape seen from Englianos, he is not content to equate it with the great Messenian valley watered by the Pamisos and its tributaries, as well as lesser rivers, but extends it indefinitely northwards, apparently to the middle course of the Alpheios, and even the area east of Olympia, although he is never very explicit about its frontiers. Likewise he is frequently tempted to include the western shore of the Messenian Gulf in this Province.

It would be impossible to refute Hiller's arguments one by one without writing a book of at least equal length. Fortunately I think this is unnecessary, for I remark three fundamental errors from which all the rest flow. If I can show that he is wrong here, I shall have vindicated my own theory.

The first is his identification of *Pi-*82* (p. 17) with the town

³ *Minnesota Messenia Expedition* (see note 2), pp. 117-147, 264-321.

⁴ Hope Simpson in his *Gazeteer of Mycenaean Sites*, London 1965, lists in Western Arcadia only Palaikastro (Trypes), on the Alpheios between Andritsaina and Dhimitsana, p. 41, no. 92.

called in classical times usually Φεαί, which is reliably placed in the neighbourhood of modern Katakolo. This identification was proposed by L. R. Palmer ⁵, and of course depends upon the suggestion that *82 has the value *ja*₂. This suggestion has not met with any general support, since the value was invented precisely to permit the equation of these names; the other uses favour rather the value of *sa*₂, which I attempted to make more exact by demonstrating the likelihood of a value *swa* ⁶. Hiller does not seem to be aware of the fact that the so-called «doublets» in the Linear B signary are all signs with specialised values; there are no cases of two signs which are not distinguished in some way ⁷. He is equally nonchalant about the vowel of the first syllable, which is later ε; but here it must be admitted that the substitution of Mycenaean *i* for classical ε does occur, apparently in non-Greek names and words like *di-pa*/δέπας. None the less, this is another reason for preferring the interpretation of *Pi*-*82 as Πίσα, or as I suggested, *Piswa*. It also needs to be added that whereas Φεαί is a plural, *Pi*-*82 can be shown to be a singular, but this too is not a compelling argument.

Hiller remarks (p. 17, n. 8) that it does not much matter which solution is accepted, since both sites lie north of the Alpheios. True, but then either depends upon the assumption that the Mycenaean name refers to the classical site; and as many other examples prove, this is a highly dangerous supposition. He finds comfort in some more supporting equations, notably *Me-ta-pa* = the town of the Μετόπιοι mentioned in an inscription from Olympia ⁸, *U-ru-pi-ja-jo* = Ὀλυμπιαῖοι and *O-ru-ma-to* = Ἐρύμανθος.

The first of these is less convincing than it seems, because we are unable to locate so far the Μετόπιοι of the Elean inscription. As I pointed out (Hiller, p. 18, n. 9), sites such as Olympia contained inscriptions erected by cities as far distant as South Italy; but the use of the Elean dialect may suggest that one of

⁵ *Minos* 4, 1956, p. 132.

⁶ *Minos* 9, 1968, pp. 62-65.

⁷ See M. Lejeune, «Doublets et complexes», *Proceedings of the Cambridge Colloquium on Mycenaean Studies*, ed. L. R. Palmer and J. Chadwick, Cambridge 1966, pp. 135-149.

⁸ Schwyzer, *Dial.* 414.

the two towns at least was within that area, and presumably north of the Messenian region controlled by Sparta down to the 4th century. But as so often, there are several towns known to bear this name later, one of them in Aetolia, and even if this Metapa lay in Triphylia, it could well be another example of a Mycenaean name which was carried by migrants to a new site in the disturbances which followed the Mycenaean collapse. It is a tenuous prop to support a fundamental pivot of Hiller's reconstruction.

For the other two equations, Ventrīs and I are responsible, since it was we who used the argument that their association in the Mycenaean texts confirmed the rather risky phonetic equations⁹. I have of course long since abandoned these identifications for three reasons. First, because they are both based upon a superficial resemblance or Gleichklang: *o-ru-ma-to* could only yield *|Orumanthos|* and there is no reason to postulate a shift of *o* to *e* (it is different from the vowel assimilation which accounts for the opposite shift in Ἐρχομενός > Ὀρχομενός), and *u-ru-pi-ja-jo* can only be brought into relationship with Ὀλυμπία by postulating another gratuitous vowel-shift; the suggestion of P. B. S. Andrews¹⁰, *|Wrupiaioi|* is rather more attractive on phonetic grounds.

Secondly, the distance between Olympia and Mt. Erymanthos is considerable, and the inclusion of the mountain, or perhaps a homonymous town on its slopes, seems to push the northern frontier well beyond Katakolo. Thirdly, the further analysis of *u-ru-pi-ja-jo* suggests that it is an ethnic of a tribal group rather than citizens of an otherwise unmentioned town. There is no positive evidence to suggest that either of these names belongs to the far north, and some that they belong to the south-western sector.

But the principal objection to placing the Pylian frontier so far from the capital is a geographical one. A Mycenaean kingdom, like any other before modern times, will have frontiers defined by natural features; the modern habit of drawing lines on a map is irrelevant here, and we must constantly remind ourselves that

⁹ *Documents*, p. 145.

¹⁰ See Hiller, p. 51, n. 47.

the Mycenaeans possessed no maps. A frontier had to be physically visible and, if possible, defensible. This is true whether one thinks of the *limes* of the Roman Provinces or the borders of the classical Greek city-states. Thus any plausible suggestion for the area controlled by Pylos must be bounded by natural obstacles.

Fortunately the sea defines the western and southern boundaries, while to the east of the Pamisos valley rises the massive bulk of Taÿgetos, as formidable a barrier as could be imagined. The only real doubts are concentrated on the northern frontier. To the north-east the natural line is clearly defined by the mountains which hem in the Messenian valley and rise to the plateaux of Arcadia. If this is not regarded as the frontier of the kingdom, it will have to extend deep into Arcadia, for which there is no evidence either on the tablets or from archaeological discoveries.

Thus we have only to ask what line at right-angles to the coast will match such a natural frontier to determine the limits of the kingdom. Proceeding north from the Palace the first natural line is at Kyparissia, where the mountains running north and south approach close to the sea. From a military point of view this would make a satisfactory frontier, and in medieval times its acropolis was occupied by a castle. But it would allow very little space for the three districts known to be north of Pylos (*Pi-*82*, *Me-ta-pa*, *Pe-to-no*)¹¹; and it would leave outside the kingdom the rich Kyparissia river valley with a dense population proved by the number of sites known, and offering the only easy route from the west coast to the Messenian valley north of the Bay of Navarino.

The next natural obstacle is provided by the River Neda, or more exactly the Tetrasi mountains just to the south of it, which again approach close to the sea, at a point where a steep conical hill narrows the coastal route to a true pass. This offers a continuous barrier connecting eventually with the Taÿgetos range on the east, and the resultant line is clear and defensible.

¹¹ Hiller has apparently ignored the clear evidence of Tn 316 that *Pu-ro* and *Pa-ki-ja-ne* are very close. The conclusion is supported by the evidence of the E-series, especially En 659, that royal officials and craftsmen have their land-holdings in the administrative district of *Pa-ki-ja-ne*. Hiller's map (p. 227) places *pa-ki-ja-na* between the Neda and the Kyparissia river.

But if Hiller is right in including Olympia and Pheai (Katakolo), we must seek a frontier much further north. Not even the Alpheios will serve, if sites to the north of it are included; and the known Mycenaean sites on both sides of the river belong geographically to a single group. Cape Ichthys is not an obstacle even if a landmark, and we should have to consider the coast at least as far as Kyllini. If the Hither Province includes the whole west coast of the Peloponnese, there is no natural frontier for the Further Province to match; and although an extension northwards to absorb the Alpheios valley and its surroundings is conceivable, this area is focussed on the coast and has no easy line of communication with the Messenian valley. From Hiller's decision to equate *Pi-*82* with Katakolo flow a great many awkward consequences.

For instance, there is no evidence whatever on the tablets to suggest a coastal location for *Pi-*82*. There is also the positive evidence, ignored by Hiller, that *Pi-*82* and its surroundings are good places for large flocks of sheep¹². Hiller has correctly seen that *Pi-*82* and *Me-ta-pa* have connections with the Further Province; a study of the geography of the area shows that the west coast has a natural line of communication with the interior via the Kyparissia river valley and the Soulima valley. It is of course true that the Alpheios also offers a route into the interior; but the upper Alpheios valley naturally connects only with its lower reaches, and it would be nonsense to group this area administratively with the Pamisos valley rather than the Olympia area. I believe therefore that *Pi-*82* may be plausibly located within the Kyparissia river valley, probably well inland in good sheep country, perhaps near the Mycenaean site of Malthi excavated by Valmin. *Me-ta-pa* will then lie in the same valley but nearer the coast.

At this point I must take up the second of the fundamental errors which I find in Hiller's work: the order of the *o-ka* tablets. It is agreed by all that An 657 must be the first of the set making up this composite document, since it carries an introductory line before the first example of the *o-ka* formula, which recurs twice

¹² Cn 131 (2371 sheep); Cn 719 (480 sheep); Cn 643 (101 goats). From Cn 131.6 it is evident that *ma-ro-pi* is a subdistrict of *Pi-*82*; the addition of the figures for *ma-ro-pi* would much increase these figures.

on each of the five tablets. Equally it is agreed that An 661 must be the last, both because of its poor state of preservation (it may well have been at the top of the pile as stacked on the shelves of the Archive Room), and because it refers to *Ti-mi-to-a-ke-i*, one of the Seven Towns of the Further Province, and *Ne-do-wo-ta-de* = */Nedwonta-de/*, the river Nedon on which the modern city of Kalamata stands. The doubts concern the other three members of the set (An 519, 654 and 656). There do not appear to be any internal criteria of composition or format by which their order can be reconstituted; the argument must be based upon their contents.

It is common ground that the function of this set of tablets is the establishment of a coast-watching force to guard against an enemy landing¹³. Hiller seems to grasp the point that about 800 men are too few to guard such a long coastline; it is considerably longer on his view than mine, but he is forced to assume that the northernmost sector is left undefended. But he does not seem to have studied the military implications of the disposition of these troops; if they were really the major force available to meet an invasion, it would be criminal folly to split them up into small units (the largest is 110 men) and arrange them around the coast. Obviously these men are not the Pylian army, but auxiliaries of some kind who are assigned to look-out duties. It is unfortunate that the operation orders for the major forces have not survived, though I believe we can deduce their disposition from this same document. The *o-ka* detachments are an early warning system designed to give the commander of the army as much notice as possible of an enemy landing.

Each of the ten paragraphs of the composite document, each beginning with a man's name in the genitive followed by *o-ka*, must relate to the ten sectors into which the coastline has been divided. It is agreed that the document begins at the north on the west coast and proceeds south to Cape Akritas, and thence north and east to the river Nedon in the Messenian gulf. With the exception therefore of the second paragraph of the last tablet (An 661. § 2), and possibly the penultimate paragraph (An 661. § 1), the sectors must follow from north to south.

¹³ *Minos* 7, 1963, p. 133; Hiller, p. 30.

An 656. § 2 refers to *A-ke-re-wa* as the location both of the *o-ka* of *Dwoios* and of 60 *ko-ro-ku-ra-i-jo*. *A-ke-re-wa* is one of the nine principal districts of the Hither Province and occupies sixth place in the standard list. *Ka-ra-do-ro* appears in An 661. § 1 and occupies eighth place in the list. Since the seventh (*E-ra-to / Ro-u-so*) and ninth districts (*Ri-jo*) do not appear anywhere on the *o-ka* tablets, it is reasonable to infer that An 656. § 2 stands next to An 661. § 1.

If this argument is accepted, the only question remaining is whether the second *o-ka* tablet is An 654 or An 519. An 519. § 1 places an *o-ka* at *Ro-o-wa*, which is shown to be an important port by its mention in first place on the two «rower» tablets, An 1 and An 724. In the latter it is associated with men of high rank, the *Lawagetas*, *E-ke-ra₂-wo* and *Me-nu-wa*. It is therefore likely to be near the Palace, and may well be its harbour, which for geographical reasons will be at the north of the Bay of Navarino, possibly the curious little crescent-shaped bay now called Voidhokiliá. There is thus some reason to place *Ro-o-wa* in the district of *Pa-ki-ja-ne*, fourth on the standard list, in which the Palace is situated.

It now follows that An 654 with its mention of *Metapioi*, men of *Me-ta-pa*, second on the list, must deal with a more northerly sector; if so, it must be the second tablet of the set. This arrangement secures the maximum economy, men being employed at the minimum distance from their homes. A minor advantage of this order is that the men called *ko-ro-ku-ra-i-jo* are concentrated in one area, the southern peninsula, represented by An 656. § 1, 2 and An 661. § 1. But too much cannot be made of this point, since the other terms applied to the men do not appear to cluster in the same way on either scheme. It may be observed, however, that the *U-ru-pi-ja-jo* belong in An 654. § 2, An 519. § 2 and An 661. § 2, sectors IV, VI and X on my scheme, which would locate them roughly in the latitude of the Palace and a little to the south on both sides of the peninsula. I have suggested elsewhere that these ethnic names may possibly indicate elements of a subject population living in the less fertile regions of the kingdom, and not regarded as reliable enough to serve with the regular army. Equally their commanders can be shown to be local dignitaries, not members of the royal household.

The alternative theory for the ordering of the *o-ka* tablets, put forward by Schmitt-Brandt¹⁴ and adopted without much discussion by Hiller, is that An 656 occupies the second place and An 654 the fourth, leaving the other three tablets in the positions I have assigned to them. The basic reason for this inversion is the identification of *o-*34-ta* as a place name, for it occurs both in An 519. § 2 and in An 654. § 1. This would be difficult to explain except on the assumption that An 519 immediately preceded An 654. It is therefore necessary to test the evidence for such an identification of *o-*34-ta*.

First, it must be observed that no certainly identified place name, excluding ethnic adjectives, occurs in two different paragraphs throughout the series. The repetition of *O-wi-to-no* in An 657. § 2 as well as § 1 is illusory; the word was obviously inserted as an afterthought, and must have stood for the ethnic, which is one sign longer; it is parallel to *a-pu₂-ka-ne*. Since we believe that each paragraph deals with a separate geographical sector, we should not expect an overlap of this kind; but it is always possible to postulate special reasons why it occurs only here.

Secondly, *o-*34-ta* occurs nowhere else on the Pylos tablets; so that no independent check is possible. However, the spelling *o-*35-ta* occurs at Thebes Ug 3, where analogies show that it is a man's name; and the form *o-*34-ta-o* in the new Thebes tablets (TH Of 33.1) is beyond doubt the genitive of the same name. This proves what we have long suspected, that **34* and **35* are variants of one and the same sign. Thus we can regard it as certain that *o-*34-ta* is elsewhere used as a man's name. It is, however, still possible for what is at one site a place name to be a personal name at another¹⁵; nor is it to be excluded that some sort of descriptive term should also be used as a name¹⁶. Thus although the evidence from Thebes creates a slight bias against a place name, it is not in itself enough to decide the question.

We must therefore look to the analysis of the *o-ka* series to

¹⁴ R. Schmitt-Brandt, «Die *oka*-Tafeln in neuer Sicht», *SMEA* 7, 1968, pp. 69 ff., see Hiller, pp. 30-34.

¹⁵ As *ra-pa-sa-ko*, PY Cn 131. 7, is almost certainly identical with the classical place name Λάμψακος.

¹⁶ *Ka-ke-u* occurs as a name as well as the descriptive term for «smith» (PY Jn 750.8).

determine the interpretation of *o-*34-ta*. In An 654. § 1 we read *ku-ru-me-no-jo*, *o-ka*, *pe-ri-te-u*, *wo-ne-wa*, *a-ti-ja-wo*, *e-ru-ta-ra*, *o-*34-ta*, *me-ta-pi-jo*, *ke-ki-de*, VIR 50. *Pe-ri-te-u* is a man's name at Knossos (B 5025.1, C 954.2), so we have here the pattern in which the word immediately following *o-ka* is a man's name (as An 657. § 2, An 661. § 1), not a place name (as An 657. § 1, An 656. § 1, § 2, An 661. § 2; the other cases are ambiguous). It was clearly unnecessary to specify the location of the *o-ka* on all occasions, presumably because the writer assumed it would be evident from the name of the commander. Since we know from Aq (formerly Sn) 64.5 that *Ku-ru-me-no* was *ko-re-te* of *I-te-re-wa*, it is perhaps not too rash to conjecture that this was the location of his *o-ka*. Unfortunately this does not help to place it on the map, since the name elsewhere occurs only on Jo 438¹⁷.

Nor is it an invariable rule that the location of each contingent of coastguards is stated. The *o-wi-ti-ni-jo o-ka-ra₂* of An 657.4 are presumably at the command post of *O-wi-to-no*, unless *su-we-ro-wi-jo* is a place name, which is possible though unlikely. The same is true of the *ke-ki-de* of An 654.4, for *wa-ka-ti-ja-ta* certainly looks more like a personal name. It is therefore by no means certain that a place name is indispensable here before the mention of *me-ta-pi-jo ke-ki-de*. Two other possibilities are open; that *o-*34-ta* is the name of one of the officers of the *o-ka*; or that it is a further designation of the *ke-ki-de* of Metapa.

If it is uncertain whether *o-*34-ta* is a place name here, An 519. § 2 is even more ambiguous. We are dealing here with a subordinate section under the *o-ka* of *Ke-wo-no*. The first word of this section, *a₂-te-po*, occurs nowhere else. *De-wi-jo* recurs at Aq (formerly An) 218.10, where it might be a man's name, a patronymic, or even a place name. It is not impossible that *a₂-te-po* is a place name, or that together with *de-wi-jo* it makes up a compound place name. The third word, *ko-ma-we*, is certainly elsewhere a man's name, *|Komāwens|*. The fourth word, *o-*34-ta-qe* ought therefore to be another man's name, linked to *ko-ma-we* with the normal enclitic *-qe*. There appears to be no obvious objection to this; but the hypothesis that it is a place name en-

¹⁷ On the impossibility of drawing geographical conclusions from this document see below, p. 54.

counters a severe difficulty in the presence of *-qe*. Schmitt-Brandt (p. 78, n. 10) passes lightly over this difficulty, suggesting that it would be obviated by making *ko-ma-we* into a place name too, or regarding it as a scribal variant in a pre-Greek name. The only parallel in this series is *ke-ki-de-qe* at An 657.13, where again *-qe* appears to be redundant; the two contingents of 30 *o-ka-ra* and 20 *ke-ki-de* are linked together (as is the phrase introducing the *e-qe-ta* which always begins with *me-ta-qe*); but their location is not specified, unless *e-ra-po ri-me-ne* looks forward rather than back to the *e-qe-ta*. I cannot agree with Schmitt-Brandt that the location of these contingents is *O-wi-to-no* (see above, p. 48).

I therefore reject the argument that *o-*34-ta* is a place name and see no obstacle to the order An 654, 519, 656 proposed above. But there is a further consequence of transposing the positions of An 654 and An 656, which Hiller has appreciated. It is then impossible for *Ka-ra-do-ro* in An 661.5 to be the same town as the *Ka-ra-do-ro* which stands eighth in the standard list of the Hither Province, since *A-ke-re-wa*, the sixth, is now separated from it by no less than four *o-ka* (An 519 and An 654) or 40 % of the coastline.

Hiller's argument for the existence of a second *Ka-ra-do-ro* in the Further Province (pp. 26-27) is, to say the least of it, a curious one. It revolves around Ae 398, which refers to the *qa-si-re-wi[-ja]* of a man called *A-pi-ka-ra-do-jo* with a note indicating that it is in the *Pe-ra-ko-ra-i-ja* or Further Province. Now *A-pi-ka-ra-do* is apparently taken by Hiller as representing *Ἀμφιχάρδος, though a derivative of χάρδος neuter (only known in West Greek) should be -χάρδης; it would therefore seem easier to connect it with κλάδος. What it does not closely suggest is χάρδρος (or χάρδρα), the common name for «ravine», which is to be recognised in *Ka-ra-do-ro*. Yet even if an official in the Further Province had a name derived from χάρδρος, this does not prove that he was named after a local town. Names are often derived from distant places, as witness the man at Pylos called *Tu-ri-si-jo* (Sa 758), who is presumably named from Cretan Τυλισός.

What is more important is that if a name is repeated in two different parts of the kingdom, it will be given a distinguishing epithet, if confusion is likely. There were two places called *pu-ro*; but the second, in the Further Province, is called *pu-ro ra-u-ra-ti-jo*

or *ra-wa-ra-ti-jo* (Ad 664, Cn 45), or more often simply *ra-u-ra-ti-ja* or *ra-wa-ra-ti-ja*¹⁸. It is incredible that if *Ka-ra-do-ro* in An 661 did not mean the well-known town of the Hither Province, it would not be further qualified. In fact, the town mentioned at An 661.5 is again referred to simply as *Ka-ra-do-ro* in Na 543, and there is no doubt that this is the same place because it is again connected with *ko-ro-ku-ra-i-jo*; in this case there is no context to indicate its location.

Hiller makes great play with the «new fact» that men are drafted from the far north of the kingdom to guard the coastline of the Messenian Gulf¹⁹. It is rather an hypothesis, and one which from the geographical point of view is so improbable, that we shall only accept it if a more plausible explanation cannot be found. On Hiller's view the most northerly sector of the coastline, between Cape Ichthys and the mouth of the Neda is left undefended and a small number of men²⁰ is sent to the other extreme of the kingdom. They would either have to travel a very arduous route through western Arcadia (in modern terms, via Dimitsana, Karytena and Megalopolis), or a very roundabout route via Olympia, the west coast, the Kyparissia river valley and the Messenian valley. On my view with the frontier on the Neda, the whole of the coastline is covered by the guard organisation, and men are deployed at points on the coast close to their home towns. It does not appear to me difficult to choose between these alternatives.

If then *o-*34-ta* is a description of the troops, there is no difficulty other than the apparent piling up of epithets in both cases. If *o-*34-ta* is a man's name, as the parallel from Thebes suggests, there are the minor difficulties of two men having the same name, and the absence of a place name to locate the 50 men of An 654.4.

¹⁸ Hiller constantly refers to this town as *ra-wa-ra-two*, a piece of ignorance which at this date shakes one's confidence in his understanding of Linear B. The problem of the two values attributable to *66, *tja* and *two* was definitively resolved by M. Lejeune, *Minos* 8, 1967, pp. 100-114; the value *two* belongs to a sign found only in PY An 261, now *91, which was previously taken as a variant of *66. For *ra-wa-ra-two* therefore read throughout *ra-wa-ra-ta₂*.

¹⁹ P. 76: «Diese Tatsache ist, wie mir scheint, bisher bei der historischen Bewertung der *oku*-Tafeln nicht genügend berücksichtigt worden.»

²⁰ An 654.4: 50 men, An 661.13: 30 men; total 80.

There are plenty of other cases where the same name recurs at different places in the Pylian kingdom; and, as suggested above, the 50 men may well be at *I-te-re-wa*, unless we infer from their ethnic that they were at *Me-ta-pa* itself.

A further consequence of the disturbance in the order of the *o-ka* tablets is the distribution of *e-ge-ta*, for there are eleven of these important officials irregularly distributed over the ten *o-ka*. The comparison of the two schemes is best shown in a table, where the sectors are numbered I to X, that is, from north to south, but with sector X in the east, inside the Messenian Gulf. The two orders coincide on the first, third and fifth tablets: E = *e-ge-ta*.

Sector	Schmitt-Brandt	Chadwick
I	657 —	657 —
II	E E	E E
III	656 E E	654 E
IV	E E E	—
V	519 —	519 —
VI	E	E
VII	654 E	656 E E
VIII		E E E
IX	661 E	661 E
X	E	E

It is clear that this asymmetrical distribution must have a reason. On Schmitt-Brandt's theory seven of the *e-ge-ta* are clustered in sectors II - IV, that is the coast of Tryphylia and the Kyparissia - Prote region, with the other four scattered around the south of the Peninsula and the Gulf. On mine, three *e-ge-ta* are located in the north, sectors II - III, which I identify with the areas just north and south of Kyparissia; six are around the Bay of Navarino, sectors VI - VIII, and one each in the south-east of the peninsula and the head of the Gulf. On my view this distribution is exactly what on strategic grounds I should expect to be the disposition of the Pylian army to counter the threat of an enemy landing. The obvious point of danger is around the Bay of Navarino, and this is where six of the eleven *e-ge-ta* are located. The

other points requiring protection are the Kyparissia area, since a landing there would threaten the inland sites and open the way to the head of the Messenian plain, and the north shore of the Gulf. Three *e-ge-ta* are assigned to the first of these areas; the other has only one, but another is probably on the west shore of the Gulf within easy range; and the forces around Navarino could quickly march across to intercept an enemy landing in the Gulf area. I have therefore suggested that each of the *e-ge-ta* is the commander of a regiment of the army. No alternative has been offered to account for the other grouping.

The third error is one of method. After much hesitation and testing, the hypothesis that the standard list of the Nine Towns represents a geographical order has been generally accepted. But the reduction of the two-dimensional relationship of sites on the ground to the single dimension of a list involves some falsification. Even if the list starts in the north and finishes in the south, we must allow for a certain amount of fluctuation between east and west; and it is only the convenient fact that all down the west coast of the Peloponnese a narrow strip of land separates the mountains from the sea that enables us to treat the order as a north-south one. Some of the districts must obviously be coastal, others inland; and wherever the frontier lies, we have to account for the dense settlement in the Kyparissia river valley, and the hinterland of the Bay of Navarino.

If we accept, as Hiller does, the identification of *Ri-jo*, which is plainly */Rhion/* with the *Ῥίον* given by Strabo as the old name of the town now called Koroni, we must place the southern end of the list, not at the southernmost point of Messenia, which is the inhospitable cliff of Cape Akritas, but east of the Cape in the Messenian Gulf. The consequences of this location need to be carefully considered, for each of the Nine Towns is the local capital of its district, thus the district of *Ri-jo* must include the other sites in the south of the peninsula facing towards the gulf, such as the important one at Longá. This in turn restricts the coast line available for the Further Province to the zone between about Petalidhi and the River Nedon, since we agree that the eastern frontier probably lies here. The *o-ka* tablets have only one sector (An 661. § 2) allocated to the Further Province; hence the 15 km. or so at the north of the Gulf will represent about

10 % of the total coastline of the kingdom, if the sectors are of roughly equal size.

It is clear therefore that the north-south order does not simply follow the west coast, but that it curves to the east at its southern end, and may well do the same at the northern end, since there is nothing to suggest a coastal site for *Pi-*82*. When we come to consider the Further Province, we have only one example of the standard list, the continuation from the Hither Province in Jn 829. It is conceivable that between *Ri-jo* and *Ti-mi-to-a-ke-i* there is a complete break, and the second part of the list begins again in the north. But it is more natural to assume that the scribe having mentally doubled Cape Akritas to reach *Ri-jo* would carry on northwards into the Messenian Valley. I have discussed the location of the towns of the Further Province elsewhere²¹ and I do not propose to say more here, because the only other evidence that *Ti-mi-to-a-ke-i* lies in the south of the Province is precisely the reference in the *o-ka* tablets; hence one would enter a circular argument.

This evidence for a geographical order in the standard list is convincing. What is much more doubtful is Hiller's attempt to elevate this into a general principle. He draws diagrams (Schema I-XII, pp. 229-244) to display the spatial relationships of place names not merely in the standard list and the *o-ka* tablets, but wherever lists of names occur. For instance, Schema VII includes the names from Jo 438, although on p. 41 he quotes my warning against relying on this list for geographical information. Four consecutive lines of this tablet (9-12) list [*Ro-*]u-so, *Pa-ki-ja-ni-[ja?]*, *A-pu₂-ja* and *Ka-ra-do-ro*; these are respectively 7th, 4th, 5th and 8th on the standard list. In lines 26-28, we have the sequence: *Pi-*82* (1st in the Hither Province), *E-ra-te-re-wa-o* (5th in the Further Province), *A-ke-re-wa* (6th in the Hither Province). It is obviously unsound to use this document at all for geographical proximity, much more to select arbitrarily three lines of it for use in a diagram.

This of course does not mean that there is nothing to be learnt from studying the collocations of place names on the tablets. If two places are constantly mentioned in the same context they

²¹ *AJA* 77, 1973, pp. 276-8.

are quite likely to be close. But it is unnecessary to assume that they will always be mentioned in the same order, or indeed that there is any natural linear order for places scattered over an area. The evidence, such as it is, for connections between the place names of the Pylos tablets has been assembled by Mr A. P. Sainer and is now in the course of publication ²². He is, however, exceedingly cautious in attempting to transfer these connections to the map, for the plain fact is that our information is regrettably insufficient.

The effect of these errors is to make the first part of Hiller's work an interesting academic exercise, but one wholly devoid of utility. It is a pity so much intelligent reasoning should lead nowhere.

The second part of the book is rather different: the location of the places mentioned in the *Iliad* and *Odyssey* as lying in the realm of Nestor. That it is again a careful and intelligent discussion no one can deny; but again I am confronted with a basic flaw which vitiates all the conclusions. Hiller never states, any more than most of the other writers on Homeric subjects, his basic assumption that Homer has accurate information on the geography of mainland Greece. Yet he proves with penetrating analysis that the Pylos of Nestor's story of the cattle raid and the war with Elis (*Iliad* XI) lies in Triphylia, but the Pylos of Telemachus' journey in the *Odyssey* is in Messenia. At the very least, the two poets are using different maps. It does not seem as yet to have been sufficiently appreciated that the discrepancies in the Homeric geography are such that it is impossible to credit the poet or poets with any accurate knowledge at all.

Yet this is perhaps the one certain fact which has emerged from all the books on the subject: that there is no agreement between their authors, and all are compelled to resort to conjecture to justify the Homeric account, when it is evident that the account itself is inconsistent. If we imagine an 8th century poet working in Ionia, is it not highly probable that his knowledge of the geography of mainland Greece will not go beyond the main outlines? To expect an accurate account of the details is absurd; none of his audience would have been able to convict him of

²² In *SMEA*.

error, for in reply to any criticism he would surely have said that the names were different at the time of the Trojan War.

All the same, we need not suppose that Homer invented all the names of minor towns with which he sprinkles the *Catalogue* and other narratives. We all nowadays believe that Homer had at his disposal a great mass of traditional verse, some of which doubtless contained lists of places. Whether Homer knew their spatial relations may be doubted, for these details have nothing to do with the story; they are adventitious ornaments whose function is primarily decorative, which is why they are so often tricked out with romantic epithets or enlivened by incidental stories.

The long debate on the historicity of the *Catalogue of the Ships* ought surely to be brought to a close. It is obvious that the *Catalogue* does not have to be treated as a coherent whole; there is no unity about it. Just because it appears to have been originally designed for a different poem from the *Iliad*, it does not follow that Homer in helping himself to an inherited passage incorporated it *exactly as it stood*. We must take a very poor view of Homer if we believe he would not have modified and adapted it, omitting what he found irrelevant or uninteresting and inserting details to make it more attractive. Everything we know about the composer of the *Iliad* proves that he transformed the earlier material he used, in content as in language. There is not a single line in the *Catalogue* which we can accept as historical, even if the original poem was of much earlier date, when the true facts were known.

At the same time we must not, and need not, press scepticism too far. The broad picture of Greece split up into a series of minor kingdoms is, in the main, convincing and some at least of the capital cities (Mycenae, Tiryns, Knossos, Pylos, Athens) seem to have had palaces in the late Bronze Age. It is a little disconcerting to find that Thebes, which certainly had a palace, has disappeared from the *Catalogue*; but much ingenuity has been expended on accounting for its absence, though this is much more likely due to the pre-Homeric tradition than historical truth.

The vital fact ignored by Hiller, with less justification than earlier writers on this subject, is that, where we can make a comparison, the Homeric and the Mycenaean lists of names do not

tally. It is not just that there are minor discrepancies. Of the sixteen major towns in the kingdom of Mycenaean Pylos, there is precisely one which reappears on the Homeric list: it is *Helos*, the marsh, a common enough name in all parts of Greece. The Mycenaean *Helos* lies in the Messenian valley; but nothing in the Catalogue suggests that Nestor controlled anything but a strip of territory along the West coast, and the Messenian valley is in the *Odyssey* a separate kingdom. If we ask for confirmation that the Homeric names were known to the Mycenaean rulers of Englianos, we get an almost equally dusty answer: *Pulos* and *Kuparissos* (Homer: *Kuparisseeis*) are on the tablets, and the second of these is not one of the major towns as the Catalogue implies. For the rest, there is hardly a trace, unless the Mycenaean *Amphigenea* (?) is Homeric *Amphigeneia*; there is no reason to suppose the Mycenaean name is that of a town rather than a woman. The equation of *A-pu₂* with *Aipu* must be abandoned; *pu₂* is certainly not *pu* but very probably *phu*, and it is odd that the name is never spelt with *a₃* if it really begins with *Ai-*.

Hope Simpson and Lazenby²³ were of course well aware of this difficulty when they came to discuss this section of the *Catalogue*. Their method is to take all the ancient evidence (much of which derives from geographers of the Roman period) to identify the site of each Homeric name, and then to test this archaeologically to see if there is evidence for Mycenaean occupation on or near it. Since most cities and towns of classical Greece were on or near sites occupied in Mycenaean times for obvious geographical reasons, the resultant coincidence is hardly surprising. Only if Mycenaean sites were relatively rare would it prove anything; but Hope Simpson himself has proved how densely many areas of Greece were settled in Mycenaean times.

The suggestion put forward to explain the failure of the *Catalogue* names to appear on the tablets, or those on the tablets to rate a mention in the *Catalogue*, is that the date of the two documents is different. The tablets are agreed to date to the LH III B period, the very period during which Troy VII A was destroyed. Hope Simpson and Lazenby attempt to solve the dilemma by

²³ R. Hope Simpson and J. F. Lazenby, *The Catalogue of the Ships in Homer's Iliad*, Oxford 1970.

postulating a LH III C date for the *Catalogue*. Relatively little is known about Messenia in this period, but two facts are certain: the area was much depopulated, and the Palace of Pylos was a burnt-out ruin. The Pylos of the *Catalogue* cannot therefore be Ano Englianos. Either it refers to yet another, so far undiscovered, Pylos, or Homer's sources are not wholly accurate.

Hiller does not seem to be much worried by this dilemma. He should be. Having proved that the Iliadic and Odysseian Pyloses are different, he is content to save the phenomena by placing the Pylos of the Telemachy in Messenia, hence at Englianos, and that of Nestor's war (*Iliad* XI) in Triphylia. The latter he then proceeds to identify with the Mycenaean *Pu-ro Ra-wa-ra-ti-jo*, the Pylos of the Further Province. Since he extends this province indefinitely northwards, he finds it possible to locate this Pylos at the site described by Strabo (8.3.14) as «30 or a little more stades from the sea». It is hard to see how a site as little as 6 km. from the sea could be in the Further Province, and even if we assume Strabo to be in error over the distance, no Mycenaean site has been discovered in this area sufficiently far inland to justify its attribution to the Further Province, and it is hard to see where it could be. It would in any case be far from the famous tombs of Kakovatos.

It seems to me unnecessary to pursue this theme further, for I do not accept that the *Catalogue*, as it now stands, is anything but an 8th century adaptation of some earlier poem. Nor do I believe that Homer's knowledge of the west coast of the Peloponnese was any more accurate than his account of the Ionian islands (*Od.* IX 21-28), where he totally misdescribes the appearance and position of Ithaca. In order to supply the detail which makes a bald description convincing Homer had to supply Pylos with some minor towns; I cannot imagine that he ever supposed pedants would spend years, nay, centuries, arguing over the location of the names he added for artistic reasons. This does not mean that the names were invented; it does mean that Homer used them without being able to envisage their actual location. The journey of Telemachus to Sparta is remarkably devoid of detail; what there is has no purpose but to demonstrate the length of the journey, and Homer had no access to a Mycenaean road-map, much less had he a personal acquaintance with the terrain.

It is all too often forgotten that the *Odyssey* is a work of fiction: historical fiction no doubt, but none the less we cannot pretend that every fact recorded actually happened. If we dismiss as a poetic device the frequent appearances of Athena to guide Telemachus, why should we demand that the details of his journey correspond to fact? Aristotle saw clearly the difference between historical and poetic truth; and he laid emphasis on the need for the poet's invention to be plausible (κατὰ τὸ εἰκός). The account of Telemachus' journey at least, excepting divine interventions, is that, which is more than can be said for Odysseus' adventures before his return to Ithaca.

This is a much wider subject than Hiller attempted to deal with in the second part of his book. But I think the assumptions which underlie his discussion, and those by countless other authors, need to be brought out into the open and critically examined. Homer composed for our entertainment and instruction, a feat he achieves almost as effectively now as when he first devised the epics, or whatever part of them we allow him to have composed. He was not an historian, and it is high time scholars ceased to treat him as if he were. The argument about Pylos has gone on for more than 2000 years; now that at last we know rather more about Mycenaean Messenia than Homer did, perhaps we can hope that scholars will turn to more fruitful subjects.

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