

A DORIC FIFTH COLUMN?

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In an article which appeared in *Acta Classica XXI* (1978) Dr. P. A. L. Greenhalgh discusses the destruction of many of the Bronze Age sites and palaces (or power-centres as he prefers to call them) at the end of the Late Helladic IIIB period, and in seeking to discover a cause for this destruction, he brings back an invasion by Dorian Greeks as the main reason for the collapse of the Mycenaean civilization. In his article he uses evidence connected with three important problem areas in Late Bronze Age Aegean history: (a) the archaeological evidence for the destructions, (b) the “Dorian invasion” as the immediate cause of the destructions and (c) the development of the Greek dialects during and following this period—all three areas bristling with difficulties and controversies. The article is written in an eloquent and flowing style; Greenhalgh uses arguments which at first reading seem persuasive, and advances a number of interesting ideas based on analogies from later times, some of which are basically sound and have much to recommend them. So, e.g. on p.23 he uses the analogy of the Battle of Agincourt in A.D. 1415 in support of his suggestion that the inhabitants of Greece in the Late Bronze Age were unable to stand up to and defeat the attacking enemy who did not “play according to the rules” of warfare as they knew it. As support for his contention that an invasion by a foreign force need not leave any archaeological trace of the invaders he adduces the parallel of Slavic penetration and settlement of the Peloponnese in the Byzantine period, for which until recently there was no archaeological evidence (see p.28 and references there). These are interesting parallels and deserve to be taken seriously, provided one bears in mind that they are analogies and may not be applicable in every detail.

I propose now to examine Greenhalgh’s treatment of the subject under the headings indicated above, to see whether the conclusions he reaches are supported by his findings.

ARCHAEOLOGICAL EVIDENCE

Here I do not need to say much. In a recent article, Elaine Ball (Ball, 1979) has shown many of the weaknesses in Greenhalgh’s treatment of the archaeological evidence—his incomplete and selective use of source material, his failure to exploit material which might have been of importance to support his contentions, his subtle altering of the emphasis of the evidence. I add a few examples of his dependence on incomplete or insufficient evidence. In his discussion of sites in the Argolid, he takes little account of the most recent excavations at Mycenae, Tiryns and Argos, the reports of which must certainly have been available to him before the publication of his article. For Mycenae and Tiryns, one can point to the relatively long duration of the LH IIIC period as revealed by Mylonas at Mycenae (Schachermeyr, 1976, p.105) and the German School under Kilian at

Tiryns (Kilian, 1976-7, pp.26f.). There is evidence at Tiryns of a thorough reorganisation of the site in LH IIIC, which gives a very different impression from the picture of "low-level occupation" painted by Greenhalgh for the Argolid at this time. For Argos, Greenhalgh states that the archaeological record is unclear; the reports of the French excavations on this site (Schachermeyr 1976, p.135 with references there) tell of two layers dating from the Late Helladic period under the later temple of Aphrodite; both these layers (one dated to LH IIIB, the other to LH IIIC) show signs of destruction, thus contradicting Greenhalgh's contention that Argos had been spared destruction in the LH IIIB period. For Laconia, too, Greenhalgh makes no reference to the excavations of Catling on the site of the Menelaion between 1973 and 1976 (Catling, *JHS Archaeological Reports* 20-23) even though they would have served to confirm the destruction of the site in the LH IIIB period.

Miss Ball has adequately exposed the weakness of Greenhalgh's arguments in dealing with the situation in Boeotia. I may merely add that recent excavations at Orchomenos (Spyropoulos 1974/5, 19; Schachermeyr 1976, p.175) have revealed a Mycenaean building, perhaps to be identified as a palace on the grounds that the building contained three wings, that may be restored each to consist of a porch, ante-room and megaron with central hearth. There are many fragments of Mycenaean frescoes, one of which shows a boar (and stag?) hunt with huntsmen wearing boars' tusk helmets and carrying spears. If Greenhalgh had taken this new material into account, he would have had less reason to criticize Hope Simpson for identifying Orchomenos as a palace on the basis of a bit of plaster. He would also have had further support for his dating of the destruction of Orchomenos to the end of LH IIIB, for the recent excavations show no pottery later than the LH IIIB destruction level.

Ball has already pointed out that according to Spyropoulos the destruction of the palace at Thebes should be dated to the end of LH IIIB (Spyropoulos and Chadwick, 1975, 55, 69-71; Hooker, 1976, 105f.), information to which Greenhalgh could have had access before publishing his article. Even though the destruction of the palace at Thebes could have been slightly earlier in the LH IIIB period (i.e., c.1240 B.C. according to Symeonoglou, 1973, 5), this is still a good deal later than the LH IIIA date accepted by Greenhalgh. In any case there is no evidence that Thebes was unfortified in the LH IIIB period (Symeonoglou, 1973, 4f.), which tends to invalidate Greenhalgh's oft-repeated argument about the invaders attacking only fortified settlements in the northern sites.

A further piece of archaeological evidence that Greenhalgh could have used in support of his theory of invasion is the presence of handmade burnished pots found in a number of Mycenaean sites alongside LH IIIC pottery; these sites include Korakou, Lefkandi, Tiryns, Aigira, (Rutter 1975, 17-32; Deger-Jalkotzy, 1978, 33f., and verbally). Rutter takes these handmade pots as evidence of invaders from the north, who soon became absorbed into the mainstream of Mycenaean culture; on the other hand, this ware could have been locally made in

a period of break-down (Sandars, 1978, 192f.). But this possibly promising evidence is passed over in silence by Greenhalgh.

A DORIAN INVASION

We now turn to a Dorian invasion as the cause of the destruction at the end of LH IIIB. Here Greenhalgh returns to a view which has been challenged since the days of Beloch (1893), who rejected the tradition of a Dorian invasion out of hand. At the time Beloch did not get much support for his idea, but later many other scholars voiced their doubts about an invasion or series of invasions by Dorian Greeks, mainly because it has proved difficult, if not almost impossible to find any archaeological evidence for their presence in the period immediately following the fall of the palaces. The belief in a Dorian invasion is based for the most part on traditions reported by Herodotus, Thucydides, Pindar and Tyrtæus,¹ and on the fact that in the first millennium B.C., Dorian dialects were spoken in most of the Peloponnese, Crete, the southernmost of the Aegean islands and the southern part of Anatolia. Greenhalgh (p.27), arguing from the presence of Doric in the Peloponnese in the historical period (he speaks of “the Doric dialect” as if Peloponnesian Doric did not at that time embrace a number of related dialects), and seeing this situation as the result of Dorian migration, prefers to accept the traditional accounts about the Dorians to “miscellaneous ‘hit-and-run’ peoples about whom there is no tradition at all”. In his adherence to the accounts of Herodotus and Thucydides, Greenhalgh shows a certain amount of inconsistency. Their evidence is for the most part accepted, while Homer as an historical source is dismissed out of hand, except when it suits Greenhalgh; instead of the currently used “Mycenaean”² as a term to denote the culture, the people and language of the Late Helladic period, he prefers the term “Achaean”, which he takes straight from Homer; Homer is acceptable as evidence for the “Dorian question” because the Dorians are “totally ignored” in the Homeric catalogue of the Greeks who went to Troy.³ Such inconsistency in the use of traditional material as evidence does not inspire confidence. A further point to remember when dealing with a traditional account as reported, for example, by Herodotus, is that there is no guarantee that the Dorians Herodotus speaks of were in fact to be identified as speakers of a Doric dialect.⁴ The Dorian invaders, if invaders they were, need not even have been Greek-speakers; invaders of a different language or dialect group may within a few generations lose their native speech and take on the language of the country where they settle (c.f., e.g., the language of the French Huguenots who went to the Cape after the Edict of Nantes in 1685; within a few generations their descendants were no longer speaking French, and the only trace of their original language is found in personal names, place- or farm-names and a number of loan-words in Afrikaans).

Greenhalgh, perhaps rightly, denies any connexion between the Greeks and the “Land and Sea Peoples” of the Egyptian records of the time of Merneptah and Ramesses III. He also rejects Bouzek’s suggestion of migration into Greece

of European tribes from the Balkans (Bouzek, 1973, 173); in his discussion of Bouzek's suggestion, it again becomes apparent that he subtly misrepresents statements he is reporting. Bouzek nowhere specifically says that his presumed attackers from the Balkans became the "Land" part of the later "Land and Sea Peoples". Greenhalgh also changes the significance of what Bouzek says by adding the little word [thus] in the sentence "Only after the Mycenaean power had been broken [thus], could the Dorians enter the depopulated area".

The picture which Greenhalgh reconstructs on the basis of his examination of the pattern of destruction in the LH IIIB period, of the traditional accounts given by Herodotus and Thucydides, and of the distribution of the dialects in the first millennium, is of a "movement of peoples into areas already weakened by internecine warfare between the rival Achaean power-centres" (p.32). He mentions possible "southward migration of the Lausitz type into Macedonia and Epirus" (not realising, it seems, that the bringers of Lausitz ware into Macedonia were probably the very people he rejected in Bouzek's theory of migrants from Europe); this migration set off a chain reaction of movements of peoples: the Thessaloi from Thesprotia displacing the Boiotoi, who move into Boeotia; the Thessaloi themselves settling in East Thessaly and destroying the "Achaean" power-centres there (in passing, I may mention that the "power-centre" of Iolkos was probably not destroyed at the end of LH IIIB as Greenhalgh claims, but continued into IIIC; Hooker, 1976, 149). The Dorians he sees as the agents who had pushed the Thessalians out of Thesprotia, but, unable to move into Thessaly or Boeotia, as these parts were already occupied by Thessalians and Boeotians, they had "swept down towards the Peloponnese", leaving a remnant in the area later known as Doris. On arrival in the Peloponnese, they were driven forward by a desire for plunder and a desire for "Lebensraum", and swept southward, destroying as they went. Greenhalgh finds it necessary to explain the initial failure of the Dorians to settle in the Argolid by saying that they continued on southwards, where they settled. When the southern parts of the Peloponnese had been occupied to saturation, some of them returned to the Argolid, broke the last resistance of Mycenae and Tiryns and settled there in the latter part of the 12th century B.C. This picture, for all its dependence on Greek tradition and on a survey of destroyed sites, is no less hypothetical than the other theories propounded for the destructions of the LH IIIB period. Having travelled through Greece from Iolkos to the southern Peloponnese, I find it hard to visualise the kind of invasion Greenhalgh envisages, of bands of invaders sweeping across inhospitable and difficult country, destroying "power-centres" with such strong fortifications that it would seem almost impossible for an attacker to take them by storm. There is certainly need of firmer evidence for a Dorian invasion than Greenhalgh supplies.

THE DISTRIBUTION OF THE DIALECTS

I turn now to the use Greenhalgh makes of the distribution of the dialects in the 1st millennium B.C. to support his theory of a Dorian invasion; in doing so I shall also examine his rejection of Chadwick's theory of the presence of Dorians as the underdogs in the Mycenaean kingdoms (Chadwick, 1976a, 103–117). Greenhalgh tries to demolish Chadwick's theory by three main arguments. I quote from Greenhalgh (p.29): "These subject peoples, he (Chadwick) says, were the Dorians, and thus he solves at one blow the problems of where they could have come from in sufficient numbers to impose their dialect, why they apparently had no archaeologically distinct culture of their own, and why the 'Mycenaean' language of the Linear B tablets appears to show a demotic alongside a katharevousa Greek, of which the former is now labelled proto-Doric and Doric is argued to have been a reversal of the trend in the katharevousa to East Greek forms".

He takes the third point first and in his arguments shows that he has not grasped the basic principles of the development of the Greek dialects, and that he seems unaware of the tremendous amount of research that has gone into the establishment of a picture of the pattern of dialect distribution in the second millennium B.C.; furthermore, it seems that he has misunderstood or deliberately misrepresented Chadwick's views. Chadwick in his article nowhere uses the modern Greek parallel of katharevousa and demotic as a reasonable analogy to the state of affairs in Mycenaean Greek. The fault with the katharevousa/demotic parallel is that the modern katharevousa is highly conservative and archaic, whereas demotic is innovative; Doric is largely conservative, the futures in $-\sigma\acute{\epsilon}\omega$ being the major shared innovation in the Doric dialects, and Mycenaean is strongly innovative. That means that Mycenaean behaves like a superstrate, where the ruling class modifies the language of its subjects under the influence of a foreign idiom (as an example we can quote English as being Anglo-Saxon modified by speakers of Norman French).

Where Greenhalgh says that "Doric is argued to have been a reversal of the trend in the katharevousa to East Greek forms", he has not fully grasped what Chadwick meant when he said (p.105): "If therefore, Mycenae, Pylos and Knossos have between the Mycenaean age and the historic period changed from East to West Greek, it follows that a substantial number of Dorians must have immigrated into these areas to produce the observed linguistic effects. No minor movement of a few ruling families could possibly cause such a widespread, but fundamental, change seeing that it is a reversal of a trend of development." This I take to mean not that $-si-$ reverted to $-ti-$ in the speech of the Mycenaean inhabitants of these centres, but rather that a group of speakers who were already using $-si-$ (retained, as we know, in the East Greek dialects) was largely replaced by speakers who continued to use the inherited $-ti-$. There was, of course, no reversal of trend, except perhaps in this sense. The speech of the palaces disappeared from the Argolid, leaving virtually no trace. While Greenhalgh admits that it would be possible to dispense with a migration of $-ti-$

speakers to account for the change in the pattern of dialect distribution, he holds to his view that the same result could equally well have come from the invasion of *-ti*-speakers who came from the north-west. He considers that in this way he has sufficiently refuted Chadwick's theory of proto-Doric in the Linear B tablets. The answers he gives to the other two points are equally inconclusive.

He answers the argument about the absence of any archaeological traces of Dorians by using the parallel of the invasion of the Slavs, as I have mentioned above, a useful analogy but no more than an analogy, and with regard to the large number of Doric speakers needed to effect the change, he merely points to our general lack of knowledge of conditions in the north-west in the period under discussion.

In view of Greenhalgh's obvious misapprehensions about the development of the Greek dialects, it would be appropriate, at the risk of repeating what has often been said elsewhere, to sketch briefly the way our knowledge of the pre-history of the Greek language has been changed and broadened since the decipherment of Linear B. Until the 1950's the 'wave' theory of Kretschmer (1909) had held ground, and there are still some scholars who hold to his view, which is that the Greeks had entered the Balkan peninsula in a series of waves of invasion, each group bringing with it its own dialect of Greek, the last group to come being the Dorians, who established themselves in areas previously occupied by "Achaean" speakers. This theory implies the existence of "Greek" before the Greeks entered Greece. That this is unnecessary, has been shown by the decipherment of Linear B, and the application of modern methods of dialect geography to the study of the Greek dialects makes it appear unlikely. The decipherment shows a much simpler picture of the distribution of dialects in the Mycenaean than in the historic period, with certainly two dialects implied from the fact that Linear B already shows the development from *-ti-* to *-si-*, while *-ti*-speakers are guaranteed by the occurrence of some *-ti-* forms in Linear B, and by the continued existence of the inherited *-ti-* in West Greek. Whether a third dialect, the ancestor of the Aeolic group, already existed as a separate dialect at this time, is still unclear. The Thebes tablets have thus far not shown any features which differ from Linear B as it was written in Pylos and Mycenae at the same time (Chadwick in Spyropoulos and Chadwick, 1975, 97f.). The application of the methods of dialect geography has shown that many of the features which distinguished the dialects from one another in the historic period are late innovations (see in particular Porzig, 1954; Risch, 1955; Chadwick, 1956, 1963; Bartoněk, 1973; Coleman, 1963; López Eire, 1970, 1972-3, 1974; García-Ramón, 1975).

When the decipherment was made known, and showed an archaic form of Greek, scholars tried to establish the relationship of the Mycenaean dialect as it came to be called, to the dialects as they were known from the historic period. The most obvious similarities were with the Arcado-Cypriot group (e.g. $-\tau\omicron\iota$ as the primary ending of the third person singular of the middle voice), which seemed to support the view that an Arcadian dialect had been the language of the

Peloponnese in Mycenaean times, and that after the fall of the palaces it continued in existence in Arcadia, while other speakers of the dialect settled in Cyprus, where the dialect developed, but not to such an extent that the close relationship of the two dialects could be denied. In this respect Greenhalgh's reconstruction of events at the end of the LH IIIB period holds water; there is certainly evidence for a movement of Mycenaean speakers to Cyprus, whether their first arrival preceded the attacks of the "Sea Peoples" on Cyprus (Schachermeyr, 1976, 283ff.) or came later (Sandars, 1978, 183 and table III). But it does not necessarily follow that the events were the result of a Dorian invasion. Further, Greenhalgh's assertion (p.22) that the Cypriot syllabary which remained in use in Cyprus till the 3rd century B.C. is a "hybrid form" of Linear B is false; it has been shown that it is more likely to have had a Minoan origin (Chadwick, 1979, 139-143).

As research into the nature of the Mycenaean dialect continued, it became clear that Mycenaean was not identical with the Arcado-Cypriot reconstructed as the parent of the two classical dialects. In addition to Arcado-Cypriot traits, Mycenaean shows some archaic features, such as the retention of the labio-velars and *w*; it has also been said to have some Aeolic features, such as patronymics in *-toç* (see, e.g. Tovar, 1964, 144), though this and other so-called Aeolic features could be accounted for as archaic features in Mycenaean, which survived in the Aeolic dialects only. Heubeck (1960/61, 163-171) has also pointed to some features where the Mycenaean form has not been continued in the later dialects. These features led him to state that the Mycenaean dialect had no direct descendants in the historic period, a view which is feasible if, as seems likely, the "superstrate" language disappeared with the fall of the palaces.

The Mycenaean dialect is shown to be relatively uniform at all the centres where it is found, and there are few traces of dialectal variants. Some of these, i.e. *i* for *e* in the dative of consonant stems, *a* instead of *o* as the reflex of η (e.g. *pe-ma* instead of *pe-mo* < **spermn*) and the *e/i* variation in e.g. *te-mi-ti-ja/ti-mi-ti-ja*, led Risch (1966) to postulate alongside the 'mycénien normal' of the official script of the palace administration a 'mycénien spécial' where a particular scribe, distinguished by his handwriting, reverted to the dialect of his everyday speech and used the variant forms of each of the examples quoted above, which in fact were the forms continued in most of the later dialects. Bartoněk (1966) at the same time elaborated on the idea of a Mycenaean *koine*, the official language of the palace records which fell into disuse after the fall of the palaces, when the need for the script was past and gone; the dialects of the historic period would then have developed from the spoken language of the ruled, not the rulers.

It is as a deduction from the observations of Risch and Bartoněk that Chadwick's theory of the sub-standard language of the Mycenaean as "proto-Doric" was devised.⁵ It is a hypothesis which has the virtue of "economy". One of the problems which had faced researchers into the history of the Greek language was the isolation of the speakers of proto-Doric, if they had been sitting

in the north-west, in the wings, as it were, of the Mycenaean stage, throughout the period since Indo-European speakers entered the Balkan peninsula c.2000 B.C., where their language was to develop into Greek in the succeeding centuries. That their West Greek speech would still have been intelligible to the inhabitants of the Mycenaean centres after a separation of c.800 years, is hardly likely (Risch, 1955, 76; Chadwick, 1976a, 107; Hooker, 1976, 171). But if they were already there, as the subject people of the Mycenaean palaces, this difficulty disappears.

A further virtue of Chadwick's hypothesis is the light it throws on the genesis of Ionic. Chadwick points out that a fairly uniform culture existed in the 12th century B.C. in the area extending from the east coast of Attica to the Dodecanese and sees in this area the "cradle of the Ionic dialect" (Chadwick, 1976a, 115; 1976b, 195f.). The Ionic dialects are directly descended from neither the Mycenaean *koine*, nor the spoken language of the Mycenaean people, though they show features of both of these, together with other distinctive elements. The *-si*-forms of Ionic (e.g. ἔχουσι) reflect the forms of the Mycenaean *koine* (e.g. *e-ko-si*), ἀπό instead of ἀπύ is from the spoken language, while the change to η from original long α is an innovation. It is interesting to note that except for this and other recent innovations, the features in which Ionic differs from Arcadian, are precisely those in which it agrees with West Greek, e.g. the treatment of the labio-velars + ε (Ionic and Doric τε-, Arc.-Cypr. τζε-, πε-), the reflex of vocalic *r* (Ionic and Doric ρα/αρ, Arc.-Cypr. ορ), the third person singular, middle primary ending -ται (Arc.-Cypr. -τοι). (For -ται as an innovation, see Ruipérez, 1952, 8–31).

Chadwick sees as the source of the West Greek influence "the lower classes of Mycenaean Attica and the islands, who were absorbed by and mixed freely with their overlords" (p. 115). The separation of Attic from the rest of the Ionic dialects he sees as a secondary development, possibly under the influence of Aeolic-speakers from Boeotia (Chadwick, 1969, 91–93). The interpretation of events at the end of the LH IIIB period, in the light of the distribution of the dialects, to a great extent depends on the question of where Greek was formed; if outside Greece, as the "wave" theory of Kretschmer assumes (and Greenhalgh appears to subscribe to this view) one needs a Dorian invasion; but if, as the research of the past thirty years into the genesis and development of the Greek language tends to show, Greek was formed inside the Balkan peninsula in the course of the second millennium B.C., we not only do not need a Dorian invasion, it becomes a positive embarrassment.

The presence of the Doric dialect in Crete in the first millennium B.C., and certain differences in the Cretan Doric as compared with Peloponnesian Doric (e.g. οί, αἱ as plural of the definite article, τοί, ταί in the other Doric dialects) can also be satisfactorily explained, if, as Chadwick suggests (1976a, 114), the Mycenaean who established their rule at Knossos in the 15th century B.C. brought with them large numbers of the lower classes, speakers of "sub-standard" Mycenaean. These people would have settled in Crete, and may well

have been the cause of the fall of Knossos early in the 14th century B.C.

In other respects, too, Chadwick's theory has the virtue of economy. As Carol Thomas (1977, 207–218) points out, there were a number of puzzling features connected with the rise of the *polis* in the Greek world during the Dark Age. There were features in both early Ionic and Doric *poleis* which looked like Mycenaean survivals, such as the rise in importance of the βασιλεύς, the position of the δῆμος, and various festivals. While of course it is possible that there could have been influences operating from one type of *polis* on another after the fall of the Mycenaean world, it seems more economical to trace these shared features back to the Mycenaean period, and if Dorian Greeks were present in the Mycenaean world, these features in their *poleis* could have been part of a common inheritance rather than a borrowing.

If space and time permitted, I could discuss many other points in Greenhalgh's article in which he seems to me to be painting a false picture of events in the LH IIIB period, but I think I have said enough to reinforce Elaine Ball's criticisms, and to show that Greenhalgh's article must be approached with caution.

If, in conclusion, I were to try to give a brief account of what seems to me to have happened at the time of the fall of the palaces, I should stress the fact that it was clearly a period of extreme unrest throughout the Eastern Mediterranean, and that it is against this background of unrest, of movements of peoples and the fall of empires (such as the Hittite Empire) that we must see the end of the Mycenaean palaces in Greece. Even if there was no attack by "Sea Peoples" or by invaders from the north, there may well have been a weakening of the Mycenaean centres, perhaps through disruption of sea traffic as a result of the movements of the "Sea Peoples", perhaps through Mycenaean participation in warlike enterprises overseas (of which the story of the Trojan War may be a dim recollection), perhaps through economic factors, as Betancourt suggests (1976, 40–47) or even through a combination of all these. Against such a background of unrest, with or without attack from outside, it would not be unlikely that the subjects in the Mycenaean kingdoms, taking advantage of the weakening of the fabric of these kingdoms, would rise against their overlords without having to breach the fortresses which would have been impregnable to an enemy attacking from outside; the presence of a "Doric fifth-column" in the palaces must certainly have been a contributory factor to the disaster.

NOTES

1. E.g. Herodotus I, 56, VII, 129, VIII, 31, 43, IX, 26; Thucydides I, 9, 12, 107; Tyrtaeus fr.2; Pindar, *Pythians* I, 65. For a recent discussion of the early literary evidence see Carol Thomas, "A Dorian Invasion? The early literary evidence", *SMEA* 19 (1978) pp.77–87.

2. Greenhalgh (p.2) objects to the use of the term 'Mycenaean' on the grounds that, under the influence of Homer (and of Schliemann) it implies the political leadership of Mycenae over the whole of Greece in the Late Bronze Age. I continue to use the term, as most scholars do, to cover the culture and language of Greece in the Late Helladic period without implying any such thing.

3. Greenhalgh, *op. cit.* pp.30, 35; it is true that the Dorians are not mentioned by name in the *Iliad*, but it has been suggested by more than one scholar, most recently by E. M. Craik, *The Dorian Aegean*, (Routledge and Kegan Paul, 1979) pp.26-30, that in reference to the contingent from Rhodes under Tlepolemus in *Iliad* 2, 653-680, we can see a reference to Dorians (a tripartite Rhodes, with ships in multiples of three, reflecting the tri-partite tribal system of Dorian communities in historic times). Greenhalgh does not mention the reference to the Dorians in the *Odyssey* (19.177); this places them in Crete, which ties in well with Chadwick's theory that there was a substantial Doric-speaking population in Crete before the fall of Knossos (Chadwick, 1976a, 114, 116).

4. Cf. the discussion by Chadwick on the Ionian name in *Greece and the Eastern Mediterranean in Ancient History and Prehistory*, studies presented to Fritz Schachermeyr on the Occasion of his Eightieth Birthday, ed. K. H. Kinzl, 1977, pp.106-109.

5. Greenhalgh's implication that Chadwick has simply followed Risch is unjustified, since Risch has not yet endorsed Chadwick's theory of 'mycénien spécial' as proto-Doric.

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