

THE LUVIAN INVASIONS OF GREECE

IN his stimulating and vigorously written book on the *Mycenaeans and Minoans*, Professor Leonard R. Palmer, rejecting the generally accepted equation of the first Greek speaking Indo-Europeans to appear in the Mainland of Greece with the Middle Helladic people, maintains that "Greece and Crete were twice invaded by Indo-European people during the second millennium B.C." For the Mainland he states: "first came the Luvians, causing the Middle Helladic revolution; they were followed by the Greeks, who caused the less violent archaeological break at the beginning of Late Helladic." For Crete he maintains that first came the Luvians *ca.* 1700 B.C. and then the Greek-speaking Achaeans at the end of Late Minoan II, *ca.* 1400 B.C.¹

I think that archaeologists will do well to study with care Professor Palmer's provocative theory. In it there is much that is of value, but there are also many important flaws that will require careful consideration. Before we examine them, we may consider briefly a point that has remained unchallenged too long. The "deciphering" of the Linear Script B by Ventris naturally caused a great deal of enthusiasm which perhaps is responsible for the exaggerations apparent in statements made following the discovery. But the stressing of the surprise experienced by some, mainly non-excavators, at the discovery that the language of Linear Script B was Greek or that "Ventris's proof that the masters of Knossos spoke Greek came as an electrifying shock to almost all who had studied the question" or that "the impact" of that fact "was devastating,"² seem to be out of place now that the first impression of the great achievement is passed. For the record, it should be remembered that besides those to whom this development came as a surprise, there were others, mostly field workers of long experience, who believed that the Linear Script B was invented to be used for the writing of Greek long before Ventris' monumental achievement proved the point.

As early as 1930, the late Professor Axel Persson maintained that the linear writing on the rim of the bowl of Asine recorded a statement in Greek. This was emended by Lindquist in 1931,³ the way a number of readings of the tablets are being revised now from time to time.

¹ L. R. Palmer, *Mycenaeans and Minoans*, London, 1961, pp. 26 and 211 ff. He was prompted to publish his theory by Professor Sterling Dow's paper, "The Greeks in the Bronze Age," presented at the Eleventh International Congress of Historical Sciences held in Stockholm in August, 1960. In that paper Professor Dow gave "clear and cogent expression" to the accepted views regarding the coming of the "Greeks" into the Mainland.

² Palmer, *op. cit.*, p. 164.

³ A. W. Persson, "Die spätmykenisch Inschrift aus Asine," *Corolla Archaeologia*, 1932, pp. 208-215; *Schrift und Sprache in Alt Kreta*, 1930, see especially p. 17 where a comparative table of signs is given. I. Lindquist, "A propos d'une inscription de la fin de la période mycénienne," *Bulletin des Lettres de Lund*, 1930-1931, pp. 111-128.

Professor J. Penrose Harland, in an article published in 1934, not only maintained that the inscribed documents from the Mainland were written in Greek and that consequently the language of the Linear Script B was Greek, but even suggested that the Asine inscription may belong to the "Arcadian-Cyprian" dialect-group, "the Theban inscriptions may be in an (other) Aeolic dialect, and it is possible that the Tirynthian dialect at this time was Doric."⁴

Before 1939, the late Professor A. J. B. Wace, as reported by Pendlebury,⁵ taught that "in LM II the Mainland was strong enough to establish control over Crete. . . . In that case the destruction of the Cretan cities (at the end of that period) was due to a national revolt against the foreign 'harmosts.'" Naturally, these "harmosts" from the Mainland of Greece, for whose benefit the tablets of Linear Script B were compiled, spoke Greek. We may recall that Wace had a good many followers who did not accept Evans' views on the matter. Certainly, the proof that the masters of Knossos spoke Greek did not come "as an electrifying shock" to Wace and his followers.

In 1936, in discussing the inscribed stirrup amphora found at Eleusis,⁶ I maintained that the Linear Script B was developed from Script A for the expressed purpose of writing Greek and that the language of the script is Greek. As a matter of fact, I suggested that perhaps the Cypriot syllabary was imported to the island from the Mainland of Greece, a suggestion that the late Stanley Casson found most appropriate.

In the fall of 1951, at the American School of Classical Studies in Athens, during a brief but memorable visit of Ventris accompanied by Ktistopoulos, I suggested to Ventris that he use the Greek language in his readings.⁷ There was therefore no devastating impact, not even great surprise, but vindication of the views and theories of a number of archaeologists, who do not adhere to the principle of following the leader and the established view and who were not under the influence of Sir Arthur Evans' brilliant discoveries and Knossocentric theories, when Ventris' proof came. It is true that Ventris' brilliant achievement opened new vistas of possibilities and it is natural for scholars to see new relations and unsuspected similarities with other

⁴ J. P. Harland, "Scripta Helladica," *A.J.A.*, XXXVIII, 1934, pp. 83-92; cf. p. 86.

⁵ J. D. S. Pendlebury, *The Archaeology of Crete*, p. 229.

⁶ G. E. Mylonas, "Ὁ ἐν ἐπιγραφῇ ἀμφορεύς τῆς Ἐλευσίνος καὶ ἡ Ἑλλαδικὴ γραφή," *Ἀρχ. Ἐφ.*, 1936, pp. 61-100. See also *Archaeology*, IX, 1956, p. 274.

⁷ *Archaeology*, IX, 1956, p. 275. Shortly after the appearance of Ventris' and Chadwick's "Evidence for Greek Dialect in the Mycenaean Archives," *J.H.S.*, LXXIII, 1953, pp. 84-103, a comprehensive survey of the Minoan writing was published by Professor Sterling Dow (1954). In that survey, already in proof when the "Evidence, etc." was circulated, were given "the reasonings which in spring 1953 forced themselves upon" Dow "as decisive in favor of the then unproved hypothesis that Linear B was Greek." Furthermore, Miss Henle in her dissertation *A Study in Word Structure in Minoan Linear B*, 1953, maintained that "the language of Class B is probably Greek." Her work also was independent of Ventris' efforts.

cultures, but a voice of caution must be raised against over enthusiasm that leads to exaggeration.

The break in the continuity of the Bronze Age culture that occurred at the end of the Early Helladic period imposed the conclusion that *ca.* 1900 B.C. a new wave of people invaded the Mainland of Greece and established themselves there. This conclusion is accepted generally. The majority of scholars believe that the invaders were the first Greek-speaking Indo-Europeans that came to Greece. Professor Palmer maintains that the invaders were Luvians, an Indo-European but not a Greek-speaking people. He bases his conclusion on the "Minyan" ware found in Beycesultan and on the name of Mount Parnassos, which in Luvian means "(place) of the temple"; consequently he suggests that other place names in SSOS and TTOS are Luvian.⁸

To begin with, can we be sure that the Beycesultan people of the "Minyan ware" were Luvians? It seems that a good deal of doubt exists on the matter. Leaving the answer to the excavators of the site, let us turn to the available archaeological evidence. It is universally accepted that the Gray Minyan ware is a characteristic element of the Middle Helladic culture. Lately, James Mellaart has projected the view that it was derived from initial stages developed in Asia Minor towards the end of its Early Bronze Age. In Asia Minor we find the district inhabited by the Luvians and we are assured that "closely related types of Minyan" were found at Beycesultan, a site within the Luvian district. Those "closely related types" are taken to prove that the producers of the Gray Minyan were Luvians. But are those "closely related types" sufficient to prove such an important relationship? Mellaart cautiously pointed out that "calling the Beycesultan vessels 'Minyan' requires some qualification and one might prefer 'protominyan.'" He further stressed the fact established by the excavations that in levels IV to XII, that is, "during the later half and well into the Middle Bronze Age, from 2250-1650 B.C." vessels of Minyan shapes are evident.⁹ I must also stress the facts that 1) these "Minyan shapes" are limited to cups and bowls, 2) that all are red-polished or buff ware, and 3) that these "protominyan" form only a percentage of the vessels found.

We may admit that the Beycesultan cups and bowls possess the "high flung handles, small ring bases, and metallic profiles" exhibited by some Gray Minyan bowls. But we can equally well point out that the characteristic gray color of the Middle Helladic ware, its soapy feel, and its peculiar biscuit cannot be compared to the red-polished "protominyan" of Beycesultan. It is maintained, of course, that

⁸ See Carl W. Blegen and J. B. Haley, "The Coming of the Greeks," *A.J.A.*, XXXII, 1928, pp. 141-154; J. P. Harland, *Prehistoric Aegina*, pp. 111-112; Mylonas, *Ἀρχ. Ἐφ.*, 1930, pp. 6-20. Palmer, *op. cit.*, pp. 26, 211 ff.

⁹ J. Mellaart, *A.J.A.*, LXII, 1958, pp. 9-33, especially 18; *Anatolian Studies*, VI, 1956, pp. 125-126. Mr. Mellaart took an active part in the excavation of the site, and to him we are indebted for the study of its pottery.

shapes are more important than color, or texture, or technique and that the shapes alone should form our criterion. I do not know whether or not such a contention is true, but assuming that it is, I may point out that the carinated profiles, ring bases, etc., are the result of the efforts of the potters to imitate metallic prototypes. One could very well believe that the bowls of the Mainland developed from such vessels in metal and not from clay copies of metallic prototypes. Early Helladic sauceboats in gold are known from the Mainland of Greece, indicating the existence there of vessels in metal in the period that preceded the Gray Minyan invasion. In our case then, one could maintain that both the Beycesultan "protominyan" and the Gray Minyan may have been influenced independently by vessels made of metal, of silver especially for the Gray Minyan. Consequently direct relationship of the two wares could be excluded. Furthermore, Mellaart, in listing the vessels found in Beycesultan Level VII, points out that the "Minyan" shapes in local ware (actually two-handled cups) "undoubtedly developed from the contemporary one-handled cups." Comparable if not similar one-handled cups, however, I found in the Early Helladic cemetery and settlement of Aghios Kosmas.¹⁰ And if the "Minyan" shapes of the Luvian district of Beycesultan were developed from cups, the Gray Minyan bowls with high-swinging handles could have developed from the Aghios Kosmas cups. It seems evident that the same reasoning which is used to account for the genesis of the "protominyan" shapes of Beycesultan can also be used to indicate a Mainland Greek source for the Gray Minyan shapes.

People need vessels other than bowls and cups, and the potters naturally will provide what is needed. "Minyan shapes," i.e. bowls and cups, "in local ware," are reported to be more common in Beycesultan Level VI, the transitional level dating from 2000-1900 B.C. from the years immediately preceding the Mainland invasion, than is the case in Level VII. But even in Level VI these "Minyan shapes . . . are vastly outnumbered by a variety of typical Middle Bronze Age Shapes."¹¹ And we may wonder, if we are tempted to accept the hypothesis that the Middle Helladic invaders were Luvians because of the "bowls and cups," how is it that they not only changed the color of their characteristic ware, but also failed to produce forms of pottery that were common and most usual in the assumed area of their provenience? Beaked jugs and all kinds of juglets, as they are called, were very popular in Beycesultan in the periods of levels VI and VII, but are completely lacking in the Middle Helladic area where the invaders established themselves. This lack of the popular Beycesultan-Luvian shapes becomes most serious when we recall that the less common "Minyan shapes," composing the "related types," are cups and bowls that could have developed from the one-handled cups to be found in Early Bronze Age contexts not only in Anatolia but also in the Mainland of Greece.

¹⁰ Mylonas, *Aghios Kosmas*, figs. 140-156 and drawings fig. 57.

¹¹ Mellaart, *Anat. Studies*, VI, 1956, p. 126.

One of the Middle Helladic vase-shapes that is as characteristic of the invaders' culture as are the bowls and cups is the goblet on a high, very often grooved stem or foot. It has been found in all the excavated sites in Greece and it is common especially in the early sites.¹² According to Mellaart, this "pedestalled goblet" was used in the river valleys of the Hermos and the lower Maeander, but it disappeared after "the conquest of the region by the Westerners," who introduced the red-polished ware including "protominyan" shapes.¹³ That characteristic Middle Helladic shape, in other words, disappeared in the Luvian district at the time when the "proto-Minyan" shapes came into being! And one wonders how Luvian invaders could have introduced into Greece a shape that was unknown in their own homeland. The disappearance of the goblet is considered very puzzling and, to circumvent the difficulties it raises, it was conjectured that goblets of metal took the place of the clay vessels. We can hardly believe that such a complete substitution was possible, and the total eclipse of the shape remains puzzling to say the least.

I believe that by now it has become apparent that the "protominyan" bowls and cups of Beycesultan are not sufficient to prove or even indicate an invasion of the Mainland by Luvians *ca.* 1900 B.C. The common pottery found at that site in the appropriate levels contrasts very sharply with the characteristic Middle Helladic ware and proves that the theory of the Luvian invasion is not substantiated by the archaeological evidence.

It seems to me that much stress has been laid on one element of culture, on the Gray Minyan pottery. Some emphasize its technique and find parallels, others the shapes of the pots and develop them from prototypes common in certain districts.^{13a} It is a commonplace to repeat that other elements of culture of equal if not greater importance do exist and should at least be considered in any attempt to establish relations and affinities. In the remains of the Middle Helladic invaders of the Mainland we have clear evidence of their architecture and their burial customs besides that afforded by their pottery. The introduction of Luvians into Crete is based on the character of the palaces of the Minoan "new era" (*ca.* 1700 B.C.) which "are brought by the excavations of Beycesultan into connection with the finds of the Luvian area." The report of the excavators states that the Beycesultan palace, destroyed in the second

¹² C. W. Blegen, *Korakou*, fig. 20. H. Goldman, *Eutresis*, figs. 183-185. Mylonas, *Προϊστορικὴ Ἑλเลนίς*, figs. 38-40, etc.

¹³ Mellaart, *A.J.A.*, LXII, 1958, p. 18. See *Anat. Studies*, VIII, 1958, p. 118 where among the shapes of vases from Levels XIII and XIV of the "late Early Bronze Age," are included high stemmed goblets of this general type (fig. 2, Nos. 8 and 11).

^{13a} F. Schachermeyer, *R.E.*, XXII, pp. 1468-1469, maintains that Gray Minyan was developed somewhere in the Aegean territory after the invasion; consequently it could not be a good indicator of the exact provenience of the Middle Helladic people. I offered a similar suggestion in *Ἀρχ. Ἐφ.*, 1930, pp. 7-15. Cf. W. A. Heurtley, *Prehistoric Macedonia*, p. 128.

half of the eighteenth century B.C., was founded at least a century and a half before 1700, in other words *ca.* 1850 B.C.¹⁴ The first wave of Luvians is assumed to have invaded Greece *ca.* 1900 B.C. It is natural to believe that those Luvians, besides their knowledge of the technique of Gray Minyan ware, would have taken with them the knowledge of building palaces, if not of the Beycesultan's advanced form at least of palaces in an earlier stage of development. It is a well established fact that such architectural achievements as the palace of Beycesultan do not appear ready-made but are the result of a step by step gradual development. Already in its Level X appear "megara." In Levels IX and VIII, dating from *ca.* 2200-2100 B.C., are found large structures that "foreshadow the Greek Megaron in its more developed state." They have round hearths, columns, plastered brick benches, wooden thresholds, deep porches with antae faced by "wooden plates set in the brick work evidently for the purpose of fixing a vertical fascia."¹⁵ The early architectural remains of the Middle Helladic invaders—the detached small two-room houses often with an apsidal end—that characterize their efforts cannot be conceived as reflecting megara or a palace architecture, however incipient. Furthermore, the mud-brick construction strengthened by a framework of wooden beams and resting on a foundation of wooden beams typical of Beycesultan is unknown in Middle Helladic Greece, where we find mud brick walls standing on a low and narrow foundation of rubble masonry.

The Middle Helladic people exhibit definite burial customs, peculiar to them, and a very definite type of grave. The interment of one body in a contracted position without gifts in small cist graves, made of four vertically set slabs, is typical of the beginning of the period. This type of burial, under outside influences, possibly from Crete, developed gradually and in the course of some three centuries. We can follow this development especially in the remains of the West Cemetery of Eleusis.¹⁶ The small-sized cists gradually increase in dimensions and their long sides are built with small, flattish stones; more than one person was buried in the grave, and furnishings or *kterismata* were placed in the sepulcher. To make it possible for more people to be buried in the same grave the practice of pushing into a corner the bones of the previously buried was introduced and established. One wonders where in the Luvian district cist graves of the type usual in early Middle Helladic times have been found? Furthermore, in the Mainland intramural burial seems to be characteristic of those times. Now we learn that intramural burial is the rule in the eastern culture group of Asia Minor, extramural burial in cemeteries in the western group. In this is found

¹⁴ *Anat. Studies*, VI, 1956, p. 123. Palmer, *op. cit.*, p. 240.

¹⁵ *Anat. Studies*, VIII, 1958, pp. 98-100.

¹⁶ Blegen and Wace, "Middle Helladic Tombs," *Symbolae Osloenses*, IX, 1930, p. 28-37. Mylonas, "The Cult of the Dead in Helladic Times," *Studies Presented to David M. Robinson*, I, pp. 68 ff. Mylonas, *Πρακτικά*, 1952, pp. 58-72; 1953, pp. 77-87; 1954, pp. 50-65; 1955, pp. 67-77; 1956, pp. 58-62.

an important cultural difference between the two groups.^{16a} The Luvian territory is in the western group.

The second wave of Luvians that is assumed to have invaded Crete *ca.* 1700 B.C. would have influenced the burial customs of the people among whom they settled, in the way they are supposed to have influenced their architecture. If the Middle Minoan invaders were Luvians, we would expect to find in Crete graves of the type introduced by the assumed Luvian invaders of an earlier era in the Mainland and current there throughout the duration of the Middle Bronze Age. Not a single cist grave has yet been reported from Crete. In the island we seem to have a continuation of the burial customs usual in Middle Minoan II times. Could we assume that the Luvians of the first wave introduced into the Mainland of Greece burial customs that were not current in the Luvian district and as a result the Luvians of the second wave were not conversant with them? Or shall we assume that the Luvians of the second wave abandoned their ancestral burial customs, as illustrated by those of the Middle Helladic invaders, also their technique of the ancestral pottery, the Gray Minyan, also some of the most common types of pottery, and were only able to influence the palace architecture of the people they subdued or among whom they were established? But even the palace architecture of the Minoan "New Era" has its roots and seems to form a natural development of the architecture that started in the island with the Middle Minoan I period and was developed in Middle Minoan II times.¹⁷

One of the interesting features of the palace at Beycesultan is a type of sanctuary "consisting of a pair of shrines, one of a 'male' and the other of a 'female' character and associated with it are two upright clay *stelae*, originally perhaps five feet high." The claimed resemblance of "the gap," emphasized in front by clay "horns," to the Minoan "horns of consecration" is still to be proved. Let us not forget that Seager discovered at Mochlos in an Early Minoan II-III votive deposit a clay object that is nearer to the Minoan "horns of consecration" than the Beycesultan "upward curvings." The character of the "male" and "female" shrines, however, is proved by figurines of the "mother-goddess" type found in the latter and by a "stout wooden post or pillar" set in the former.¹⁸ And one wonders where in Crete were found shrines reminiscent of the type of the sanctuary in Beycesultan that "had existed from

^{16a} Mellaart, *A.J.A.*, LXII, 1958, p. 13.

¹⁷ Cf. Pendlebury, *op. cit.*, figs. 13, 20.

¹⁸ Palmer, *op. cit.*, p. 239. R. Seager, *Explorations in the Island of Mochlos*, fig. 48, No. 31. Sir A. Evans, *Palace of Minos*, I, p. 57, fig. 16c. Cf. also Mallowan, *Iraq*, IX, p. 184 for oriental parallels. It should be remarked that the shrine illustrated by Palmer is a reconstructed drawing of the shrine found in Level II which dates from the very end of the Bronze Age (*Anat. Studies*, VIII, 1958, p. 111, fig. 7.) The so-called "horns" are to be seen in Levels XIV-XI, in the earlier period, and their function has still to be determined. For marble figurines from the shrine of Level XVII see pl. XXVIII, 6, *Anat. Studies*, VIII, 1958, op. p. 101.

the Early down to the Later Bronze Age on this site," or even in the Middle Helladic territory of the Mainland. And what happened to the male divinity that at Beycesultan seems to have been as prominent as the female? Did the invaders of Crete forget their male God, or where are we to see his traces in early MM III contexts? And where in Crete, or even in the Mainland, do we find "clay stelae" some five feet tall that characterize the Beycesultan sanctuary?

Mellaart suggests that perhaps "some of the innovations" he recognizes in the Middle Minoan III period should be attributed "to cultural influence from South-western Anatolia and the arrival of small bodies of aristocratic warriors may have led to a more efficient reorganization of the Minoan Kingdoms."¹⁹ Such an infiltration of course is short of the invasion suggested by Palmer. One may wonder, however, whether "some of the innovations" did not actually occur as a result of peaceful commercial relations between the two areas. Students of the Greek prehistoric world are familiar with the changes in the Mycenaean culture brought about by Minoan influences that infiltrated into the Mainland of Greece through commercial and other peaceful exchanges.

It seems to me that a good many questions would have to be answered before Luvian invasions could be accepted. At present we can only point out the contrast existing between the cultural elements of the Middle Helladic era and the Middle Minoan II and III periods, a contrast that would lead us into a difficult dilemma if we maintained Luvian invasions. If we project a Luvian invasion of Crete *ca.* 1700 B.C., we shall have to accept that the Middle Helladic invaders were not Luvians, for the culture of the latter is not reflected in any way in what would be conceived as Minoan-Luvian achievement in Middle Minoan III times. If we maintain that the Middle Helladic invaders were Luvians, then, for the same reason, we have to admit that the island of Crete was not invaded by Luvians *ca.* 1700 B.C. Fortunately, the dilemma does not exist in actuality because, as we have seen, a Luvian invasion of the Mainland at *ca.* 1900 B.C. is not indicated by the existing archaeological remains. What about the linguistic evidence, does it substantiate the claims for Luvian invasions?

I leave it to the specialized philologists and to the students of the Linear Scripts to determine whether or not the Luvian elements recognized on the Cretan Linear A

¹⁹ *A.J.A.*, LXII, 1958, pp. 27-28. The innovations, in his words, are: "not only is there a great rebuilding of the palaces on a more unified plan, but there is a change from the painted pottery to wheel-made plain wares and an influx of new and more advanced metal types" and then Linear A "spreads all over the island." It should be noted that painted pottery was not abandoned but continued to be produced along side the plain, and that vessels influenced by metallic prototypes were common in MM II. Whether these rather generalized innovations are sufficient to prove an invasion remains to be decided by individual predelections. As far as I am concerned, I doubt that they are sufficient to prove even an intrusion.

tablets are sufficient to indicate the establishment in the island of a few Luvian aristocrats or of a crowd that would constitute an invasion. One, of course, could also maintain that they were the result of commercial relations and mutual influences.^{19a} But I shall have to examine the evidence that was brought forth for the Mainland of Greece because it depends upon archaeological data as well. Before I do that, however, I cannot help but point out that sometimes the methods employed in the interpretation of the texts of the tablets, the way conclusions are reached, and the tendency of reading into the texts all kinds of non-existent information, leave the mere archaeologist in a state of bewilderment.

As an example we may take the far-reaching conclusions regarding the identity of the Queens-Goddesses mentioned in the Pylos tablets.²⁰ On Pylos Fr 1219 we find the text *wa-na-so-i po-se-da-o-ne*, Oil etc. On Pylos Fr 1227 we have the text: *wa-na-ka-te wa-na-so-i*, Oil etc. Professor Palmer interprets the former as "The Two Queens (and) Poseidon," and the latter "To the King (and) the two Queens." But Dr. Chadwick states that *wa-na-so-i* does not mean two Queens, but is "the name of a place or building" and this view seems to have been accepted by Professor Bennett.²¹ The non-specialist is entitled to wonder what is the real meaning of the *wa-na-so-i*. But let us not presume the task of proving the correctness of the one or the other interpretation; let us assume, for the sake of the illustration undertaken, that it means "two Queens." Since the "two Queens" are the recipients of offerings and are connected with Poseidon on Fr 1219, we may admit that the "two Queens" are divine, that they are "two Goddesses" and that the Wanax associated with them on Fr 1227 is also divine, a God who received offerings. After establishing this much, Professor Palmer proceeds to make the "God" a "Young God" and further on to equate him with the infant of the well-known ivory group of Mycenae, thus transforming him into an "infant God." And the non-specialist is entitled to ask, what is there in the "*wa-na-ka-te*" that will indicate that the Wanax-God of the text is a "Young God"?

Towards the end of his book we read: "the religious texts from Pylos had established with reasonable certainty that Wanassa Queen was the Mycenaean cult title of a Goddess corresponding to the Earth Mother of Western Asia and the confirmation had come for our further deduction that Wanax 'King, Ruler' was in all probability the title of the 'Young God,' who is the son and consort of the Mother Goddess." I have failed to find out where in the "religious texts" of Pylos evidence is preserved that will establish "with reasonable certainty" the cult title claimed.

^{19a} The importance of the element of borrowing in the consideration of linguistic affinities is recognized by Professor Palmer. I could provide a number of modern Greek parallels to stress the point.

²⁰ For the data used in the discussion see Palmer, *op. cit.*, pp. 123-125, fig. 13 and p. 232.

²¹ J. Chadwick, *Minutes of the London Minoan Linear B Seminar*, 7 November, 1956, p. 5. E. M. Bennett, *The Olive Oil Tablets, Minos*, Suppl. No. 2, 1958, p. 50.

In those texts, we have two Queens—two Wanasoi, who perhaps are two Goddesses, but whether Wanassa is the cult title of the one of them (which one?) who can be proved to be the Mycenaean Goddess corresponding to the Earth Mother of Asia is nowhere indicated. Now, however, the God has become not only a “Young God,” but also the “son and consort of the Mother Goddess.” And we may ask again, what is there in the *wa-na-ka-te* of the text that would indicate that he is the son and consort of the Mother Goddess? The “Wanax” of the Pylos texts is associated with the *wa-na-so-i*, which may mean the “two Queens.” Even in the ivory from Mycenae we have two female figures. But the “Young God” is the “son and consort” of *one* Mother Goddess. Which one of the two *wa-na-so-i* is that Mother Goddess and why? The “Two Queens” were identified with “the Mistresses” of Arcadia. Is there anything in the religious tradition of Arcadia that indicates the existence of a “Young God” the son and consort of one of the Mistresses? If the “Wanassa-Queen was a Mycenaean cult title of a Goddess,” then it is reasonable to assume that the “Wanax-King was “a Mycenaean cult title” of a God. And since Poseidon is the great God of the Pylians and we find him associated with the Wanassoi-Queens on Fr 1219, could we not conclude that the Wanax is the Mycenaean cult title of Poseidon and that the *Wa-na-ka-te* of Fr 1227 refers to him? That the *wa-na-ka-te*, consequently, has nothing to do with the “Young God” the son and consort of the Wanasoi? The Arcadian traditions will favor such a relation of the Wanax-Poseidon with the Wanassoi-Mistresses. However, “Potnia” also is called the Mistress-Lady; is the Potnia one of the two Queens? Again another divinity mentioned in the texts of the tablets, the mysterious *di-u-za*, is thought to be the “divine Mother,” the “Magna Mater” of Asia Minor. Is one of the “two Queens” to be equated with the *di-u-za*, i.e. the Mother Goddess equated with the Divine Mother? In one of the olive oil tablets from Pylos we find another goddess that may vie for the same title: *ma-te-ze te-i-ja*, possibly the *Mētēr Theōn*. What is the relation of that Goddess to the Wanasoi? ²²

This bewildering array of equations, assumptions, and question-marks becomes more bewildering when it is claimed that a “welcome harmony” exists between the straightforward philological interpretation of the texts and the contemporary evidence from the Mycenaean world on the one hand and that of later Greek religion on the other.” ²³ I think that our discussion indicates anything else but a straight-forward philological interpretation and a harmony between our various sources. I leave it to

²² Cf. Ventris and Chadwick, *Documents*, p. 125; Bennett, *op. cit.*, p. 42. Chadwick suggested that in *ma-te-re- te-i-ja* we may have the Goddess Potnia under another name: *Mimos*, V, 1957, p. 125. Bennett, *op. cit.*, p. 27, states that “the parallelism” of *wa-na-ka-te* “with *po-ti-ni-ja* in 1225 (1235?) suggests that here, whether or not elsewhere, he might conceivably be a masculine counterpart or colleague of Potnia.” In that case, is he still a young God, the son and consort of Wanasoi?

²³ Palmer, *op. cit.*, p. 125.

the students of religion to straighten out the equations of "the *two* Queens" with the *one* Mother Goddess of Asia Minor. For the contemporary evidence from the Mycenaean world, however, I must point out that before we can accept that the ivory group from Mycenae represents the "Wanax" and the "Wanatoi" we have first to prove its religious character. What evidence is there to prove or even to indicate that the group represents divinities? Was it found in a verified shrine with other cult objects? I have repeatedly stressed the fact that there is not a single shred of evidence associating the group with a shrine or with the religious activities of the Mycenaeans.²⁴ As for the child, Professor M. P. Nilsson pointed out a few years ago that it could not be Iacchos as was originally suggested.²⁵ Now it is identified as Triptolemos. But there is no place for Triptolemos the God in the Eleusinian tradition until the fifth century B.C. and then he was not a child brought up by the Goddesses of Eleusis. He is one of the mortal leaders of the Eleusinians in the Homeric Hymn to Demeter that seems to embody the local traditions of the Sanctuary. As for the multiple terracotta figurines, showing two women joined together like Siamese twins with a child seated on their shoulders that are assumed to represent twin Goddesses and a child-God, I know of no evidence that would indicate that interpretation.²⁶ As a matter of fact the multiple figurines were not found in shrines but in graves; they are of the Φ type that has been interpreted by some archaeologists as *ushebtis*, representing slaves and even concubins placed in the graves for the needs of the departed!²⁷ The harmony claimed is non-existent and the example we cite is a fair warning against exaggeration and interpretation that is exciting and provocative, but that at the same time goes beyond the textual evidence uncovered thus far.

With these remarks of caution in mind, let us turn to the linguistic evidence assumed to favor a Luvian origin of the Middle Helladic invaders. It seems that it is limited to the name of Mount Parnassos. That name is recognized as Luvian, meaning the "(place) of the temple." "It is applied to the mountain," writes Professor Palmer in his convincing way, "which dominates the most important religious centre of Greece. . . . Parnassos was named by Luvian speakers who lived there and worshipped at a shrine important enough to survive a new ethnic invasion and to remain the religious centre of the Greeks."²⁸

This is a fascinating theory that leaves spellbound the student of Early Greece.

²⁴ See among other references Mylonas, *Ancient Mycenae*, pp. 62 ff.

²⁵ M. P. Nilsson, *Minoan-Mycenaean Religion*, 2nd edit., p. XXIV. Wace originally claimed that the child was Iacchos, *J.H.S.*, LIX, 1939, p. 210; *Mycenae*, pp. 83-84, figs. 101-102.

²⁶ Mylonas, "Seated and Multiple Mycenaean Figurines," *The Aegean and the Near East*, pp. 120-121; *Yearbook School of Philosophy University of Athens*, 1954-55, pp. 139-152; *Ancient Mycenae*, pp. 80-82.

²⁷ Cf. for example, A. Persson, *The Royal Tombs of Dendra*, p. 89; *New Tombs at Dendra*, p. 33. Nilsson, *op. cit.*, pp. 307-308.

²⁸ Palmer, *op. cit.*, p. 242.

It carries with it the prestige of an established fact, the fact of the continuity of cult places in Greece.²⁹ But does the theory correspond to the facts as established by archaeology? How can we prove *when* the name was given to the mountain? If the 'temple' established on it was so important as to give its name to the mountain, it must have been the center of a prospering, at least of a large, community. Palmer himself states that Luvian speakers "lived there and worshipped at a shrine important enough to be called 'the temple.'" The question naturally arises: do we have the remains of a Middle Helladic settlement, characterized by Gray Minyan or by Luvian pottery, in the area or around the area of the temple of Apollo at Delphi which is assumed to have been the descendant of the shrine established by "the Luvians who lived there?"

The sanctuary of Apollo at Delphi has been almost completely cleared by our French colleagues. In its lower strata were found prehistoric remains. These represent the Neolithic period, possibly the Early Bronze Age, and the Late Mycenaean period, especially the Late Helladic III era. Perdrizet, who published the remains found in the area of the temple, states specifically that the painted pottery recovered "appartient toute à la série lustrée . . . aux deux derniers styles" of Furtwängler and Löschcke. He reported no Gray Minyan sherds from the area.³⁰ Demangel, who published the remains from the Marmaria district, found no pottery of the Middle Helladic age, and the painted ware and figurines he illustrates belong to the Late Helladic III period.³¹ In the summer of 1934, Lerat explored a prehistoric settlement to the north of the temple of Apollo. The figurines and the pottery found place it in the Late Helladic III period.³² In the entire excavated area of Delphi not a single Gray Minyan sherd was found and Gray Minyan is supposed to be the characteristic pottery of the Luvian invaders; not a single cist grave, not a foundation attributable to the Middle Helladic invaders. The assumed great Luvian shrine, the center where people that lived there worshipped, could not have existed in a place where Luvians are proved not to have lived in Middle Helladic times.³³ Consequently the name of the mountain could not have been derived from a temple that seems never to have existed at Delphi. The linguistic evidence for the Luvian origin of the Middle Helladic invaders is not corroborated by archaeology.

The name of the mountain, however, is there and we are assured that its meaning

²⁹ Nilsson, *op. cit.*, pp. 457 ff.

³⁰ D. Perdrizet, *Fouilles de Delphes*, V, pp. 1-5.

³¹ R. Demangel, *Fouilles de Delphes*, II, fasc. 5, pp. 6-13.

³² *B.C.H.*, LIX, 1935, pp. 275, 329-375.

³³ In a brief note, *ibid.*, p. 522, Lerat states that two deposits of plain pottery belonging to the Early and Middle Helladic periods were located to the east of the Kassotis, and that some vases were found below the Lesche of the Knidians. But he adds that this pottery presents analogies to that from West Greece and not to the pottery from Central Greece or the Peloponnesos. Cf. also *B.C.H.*, LXXXV, 1961, p. 366. One wonders whether the Luvians themselves could not have inherited the term from the people who lived in Asia Minor before the arrival of any Indo-European tribes.

in Luvian is definite. If that meaning is acceptable, how can we account for it? Of course the archaeologist cannot be held responsible for the lack of archaeological evidence to prove a linguistic point. He is required to state whether or not there exists the archaeological evidence claimed for the corroboration of a linguistic conclusion. This we have done and we have stated definitely that such evidence does not exist. But for the sake of thoroughness, we may pursue the point. If we accept Professor Palmer's theory of the Luvian invasion of Crete and his interpretation of the name Parnassos, we could figure out the following possibility. The homeric Hymn to Apollo states that Cretans established the shrine of Apollo at Delphi. If Luvians were established in Crete *ca.* 1700 B.C., could we not believe that the Cretans who built the first shrine at Delphi were Luvians, or at least had in their vocabulary Luvian terms and words, and that they were responsible for the name? This possibility, drawn from Professor Palmer's Luvian theory, if true, will strike out the linguistic evidence from his arguments in support of the Luvian origin of the Middle Helladic invaders. However, how then shall we explain the names of mountains, such as Parnes, Parnon, which seem to stem from the same root "Parn" and which have no relation, legendary or cultural, with Crete?

Let us now go back to the invaders of Greece and point out that no tangible archaeological evidence has been brought forth to justify the Luvian invasion of Crete, *ca.* 1700 B.C. It is otherwise with the case of the Middle Helladic invaders of the Mainland of Greece. The archaeological evidence brought forth in that case consists of one element, "the related types" to Minyan ware. But our discussion (above, pp. 286-288) of that element has proved, I believe, its inadequacy to indicate that the Middle Helladic invaders were Luvians. On the other hand, other known cultural elements of the period, including the real Gray Minyan, seem not to be found in the Luvian territory of Beycesultan. Furthermore architectural elements extant in Beycesultan are not to be found in the Mainland of Greece. The archaeological evidence, as far as it exists now, excludes a Luvian provenience for the Middle Helladic invaders of the Mainland.

If, however, we accept the view that the Middle Helladic new-comers were Luvians, we have to bring into the Mainland of Greece Greek-speaking Indo-Europeans sometime before the Late Helladic III period, when, according to the Linear Script B inscriptions, Greek was spoken by the Mycenaeans. Professor Palmer, as we have seen, brings the "Greeks" at the beginning of the Late Helladic I period, or *ca.* 1600 B.C., on the strength of the "new types" of tombs, the tholos and the chamber tombs used in the course of that period. To strengthen his conclusion he calls on the expressed opinions of the late Professor V. G. Childe and of Professor Martin P. Nilsson.⁸⁴

⁸⁴ Palmer, *op. cit.*, p. 247. It is interesting to remark that Professor Palmer is using the principle of the "archaeological break in the continuity of a culture" to detect intrusion of a new people, the

Childe's statement, made "in a discussion in 1955," is quoted to the effect that there exists a break between Middle Helladic and Late Helladic, "deduced" "from the sharp contrast in burial rites—individual burials in cists or jars within the settlement in MH, whereas in LH we have collective burials in cemeteries or tholos tombs and rock-cut chambers. These family vaults would surely indicate the arrival of a new stratum." We must stress the fact that these statements do not agree with the archaeological evidence as known to date.

The custom of intramural burial does not end with the Middle Helladic period, but continues into the Late Helladic. Examples of such burials are known from a number of sites. I shall quote only examples found in excavations I had the privilege to direct. At Eleusis in Late Helladic contexts intramural burials of children were found and published.³⁵ At Aghios Kosmas intramural burials of Late Helladic date were found in the settlement on the headland.³⁶ These prove that the custom continued into Late Helladic times. On the other hand, the excavations of Mycenae (Wace), Prosymna (Blegen), and Eleusis (Mylonas) proved that at least in the second half of the Middle Helladic period cemeteries outside the settlements were in use.³⁷ In addition, the graves cleared at the cemetery of Eleusis prove that gradually the dimensions of the cists were increased and, before the end of the period, more than one person were buried in each grave; that before the end of the period, the graves were transformed from individual into family graves.

The "family vaults" illustrated by the tholos and chamber tombs would not indicate "the arrival of a new stratum" because the principle of collective burial which they exhibit was already practised in the family graves of the late Middle Helladic period and in the shaft graves. We do not know how many people were buried in the oldest tholos tomb found at Mycenae, in the so-called "Cyclopean Tomb," because it was emptied of its contents long before the era of excavations. But in the tholos tomb of Dendra Persson found only three complete skeletons and some disturbed bones,³⁸ and Professor Marinatos reports six or seven burials from the

principle he maintains is discredited. He does correctly use it, since he brings no proof to show why the principle is discredited. Professor Schachermeyer's "stylistic phases" do not cover the facts and few if any excavators will be satisfied only with stylistic analyses and suppositions. Professor Matz's statement: "the discovery that the Linear Script B tablets of Knossos were written in Greek is surprising because in the archaeological finds from this period (LM II) of Crete there is an unbroken continuity . . ." does not discredit the principle, if Professor Palmer's thesis is correct that those tablets do not belong to LM II period. Sir Arthur's opinion that "there was no break" in LM II and consequently no intrusion of new people, which brought about the claim for discrediting the principle, is vindicated, as Palmer himself states (p. 24), by placing the tablets in LM III times.

³⁵ *A.J.A.*, XL, 1936, p. 424, figs. 11, 12. *Πρακτικά*, 1952, p. 58. See also *Eleusiniaka*, I, p. 573.

³⁶ Mylonas, *Aghios Kosmas*, pp. 60-63.

³⁷ C. W. Blegen, *Prosymna*, pp. 30 ff.; A. J. B. Wace, *Mycenae*, p. 51. Mylonas, *Πρακτικά*, 1952, pp. 58-72; 1953, pp. 77-87; 1954, pp. 60-65; 1955, pp. 67-77.

³⁸ A. Persson, *The Royal Tombs of Dendra*, pp. 13-18.

unrifled tomb of Routsis.³⁹ The tholos tomb of Menidi seems to have yielded only six burials.⁴⁰ The remains of five persons were found by Schliemann in each of the shaft graves IV and V and Dr. J. Papademetriou and I cleared four skeletons in Grave Gamma of Circle B.⁴¹ In the cemetery of Eleusis bones belonging to four and five persons were found in graves of the closing years of the Middle Helladic period. The relatively small number of persons found buried in the Shaft graves and in the sepulchers of Eleusis compares favorably with the equally small number of burials found in the unpilfered tholos tombs. Indeed we can now trace the development of the habit of collective burial from its inception in Middle Helladic times to its culmination in the Late Helladic era. This development does not present either a break or an innovation indicating "the arrival of a new stratum" in Late Helladic I times. The late Professor Childe's statement does not correspond to the facts as established by the latest excavations.

Professor Nilsson's statement is not as categorical as that of Childe's.⁴² His hesitation to accept the Middle Helladic invaders as Greeks stems from his belief that a break in culture exists between the Middle Helladic and Late Helladic periods, but he agrees that the burial customs indicate that an "early Greek element must have been established" in the Mainland of Greece in Middle Helladic times. He is willing to accept the opinion of the field workers.

Perhaps the fact should be stressed now, that there is no sharp and clear line of demarcation between the Middle and Late Helladic I periods. We do not have evidence to prove that the Middle Helladic period ended at a particular year, or even a particular decade, and then the Late Helladic period began. As a rule the end of the former and the beginning of the latter is placed around 1580 B.C. Furthermore, the shaft graves of Circle A are considered the earliest manifestation of Late Helladic I activity. It is acknowledged, however, that Shaft Grave VI went back into the closing years of the Middle Helladic period. Besides the graves of Circle A, we now have the sepulchers of Grave Circle B and there can be little doubt that at least some of them belong to the closing years of the Middle Helladic period. In the course of the excavations of 1961 at Mycenae, we revealed the old wall located by Tsountas below that retaining the parapet of Grave Circle A. An incision into the old wall yielded twenty-two small sherds, all of which belonged to the latter half of the Middle Helladic period. These sherds prove that the original wall that enclosed the shaft graves explored by Schliemann was built in late Middle Helladic times. Thus we find that the shaft graves are not the peculiar cultural feature of a period, of the Late Helladic I, but a link that

³⁹ S. Marinatos, *Illustrated London News*, April 6, 1957. A. K. Orlandos, *Tò Ἔργον τῆς Ἀρχαιολογικῆς Ἑταιρείας*, 1956, pp. 93-95.

⁴⁰ H. Lolling, *Das Kupfelgrab bei Menidi*, 1880.

⁴¹ J. Papademetriou, *Πρακτικά*, p. 443. Mylonas, *Ancient Mycenae*, pp. 106, 137.

⁴² Nilsson, *op. cit.*, pp. 11-12.

binds together two periods, a link whose beginning goes back into the earlier while its developed form belongs to the later period. Sometime ago Wace pointed out that the shaft graves are "elaborate or royal versions of the ordinary Middle Helladic grave." The shaft graves, therefore, in spite of their rich ktermata and of the Minoan influence they exhibit, cannot be considered as constituting proof of a break between two periods.

The consideration of the tholos and the chamber tombs will lead us to similar conclusions. It is generally assumed that they follow the shaft graves. Wace, sometime ago, laid the foundation for this assumption when he maintained that "the earliest tholos tombs should follow the last Shaft Grave."⁴³ This assumption was perhaps prompted by Tsountas' conclusion that "there is but one theory on which these facts," (that is the difference in the types of graves and, as he maintains, in the burial customs), "can be fully explained. It is that of a change in the ruling race and dynasty and it clears up the whole history of Mycenae."⁴⁴ The assumption was given new prestige when it was taken over by Wace and was developed into the theory of the "Shaft Grave Dynasty" and "the Tholos Tomb Dynasty," dynasties that succeeded each other.⁴⁵ On this assumption is based the notion that the tholos and chamber tombs are a distinct break in the continuity of the Middle to Late Helladic culture and on that notion in turn is based the introduction of new people, of the Greek-speaking Indo-Europeans, into the Mainland of Greece in Late Helladic I times. There is no evidence whatsoever to justify the distinction into dynasties of the rulers of Mycenae and this commonly held conjecture has to be abandoned.

The assumption centering around the chronological relation of the tholos and chamber tombs on the one hand and the shaft grave on the other is not based on fact. Since Wace's earlier excavations at Mycenae, the evidence uncovered has proved two things. *One*. Construction of the chamber and tholos tombs does not begin when that of the shaft graves ends. For some time shaft graves, tholos and chamber tombs were in use concurrently. As a matter of fact, the furnishings of the tholos tomb of Traghana, excavated by the late K. Kourouniotes and published recently by C. W. Blegen, prove that tholos tomb construction was current long before 1510 B.C., the date calculated by Wace to mark the end of the building of shaft graves. In the first excavated chamber tomb of the cemetery of Volimidia, near Chora of Triphyllia, I found Late Helladic I sherds packed with the bones of the early burials placed in the grave. Vases from the same period were found by Professor Marinatos in other chamber tombs of the district, (he even reports a vase from Grave No. 3 identical to

⁴³ Wace, *B.S.A.*, XXV, 1921-1923, p. 391. For his statement regarding the development of the shaft graves from the MH cists see pp. 120-121.

⁴⁴ Ch. Tsountas and J. I. Mannatt, *The Mycenaean Age*, 1897, p. 345. The suggestion was made previously by Adler and was accepted by Perrot, *Journal des Savants*, 1892, p. 449.

⁴⁵ Wace, *B.S.A.*, XXV, 1921-1923, pp. 119-120; *Mycenae*, p. 22.

one found in Shaft Grave III). Wace illustrates Late Helladic I vases found in the chamber tombs of Mycenae and some of the chambered sepulchers of Prosymna were in use in Late Helladic I times.⁴⁶ And it is widely accepted that most of the shaft graves of Circle A were in use during the Late Helladic I period. The fact that the three types of graves were used concurrently for some time will invalidate the division of dynasties and will prove that the distinction into "Shaft Grave Dynasty" and "Tholos Tomb Dynasty" is not indicated by the available evidence. It would take us too far afield to demonstrate the point further; I have dealt with the question fully in a previous study in which the interested scholar will find a full argumentation.⁴⁷

Two. It is assumed that the shaft graves on the one hand and the tholos and chamber tombs on the other exhibit different burial customs. This assumption, too, does not correspond to the facts. The only difference existing between the two is the apparent difference in the type of the sepulchers; the tholos and chamber tombs could not have developed from the shaft graves. This difference alone does not prove difference in burial customs, especially since all the other details they exhibit are identical to each other. Again I have examined this assumed difference in my previous study and I believe that there I have demonstrated that the type of grave is different, but the burial customs are the same.⁴⁸

Professor Palmer brings the "Greeks" into the Mainland of Greece *ca.* 1600 B.C. If we actually assume their arrival on that date, we shall have to equate their activity with the shaft graves. But then we shall have to account for the fact that on their establishment, or even before that, they were able to amass so much gold, certainly introduced from the outside and possibly from Egypt, that they were able to absorb at once the Minoan artistic ways, that they were capable of establishing overseas connections with Egypt or the Eastern Mediterranean territories indicated by objects such as the ostrich eggs, the inlaid daggers, the embalmed body found in Grave V, etc. This I believe would prove a most difficult if not impossible task.

⁴⁶ Blegen, "An Early Tholos Tomb in Western Messenia," *Hesperia*, XXIII, 1954, pp. 158-182. The pottery places that tholos tomb to the very beginning of Late Helladic I if not in the closing years of the Middle Helladic Period. Marinatos, *Πρακτικά*, 1952, pp. 474 ff. Wace, *Chamber Tombs*, pls. I-IV. Blegen, *Prosymna*, pp. 230-231.

⁴⁷ Mylonas, *Ἡ ἀκρόπολις τῶν Μυκηνῶν*, Ἀρχ. Ἐφ., 1958, pp. 169-171.

⁴⁸ *Ibid.*, pp. 162-169. The identity of the burial customs is proved by the following similarities. Both in the shaft graves and in the tholos and chamber tombs we find: 1) People were inhumed and cremation was not practised. 2) Kterismata were carefully placed around the body at the time of burial, but were carelessly removed, often were broken and sometimes were thrown out of the grave after the flesh had decomposed. 3) The bones of the previously buried were often uncereimoniously swept to the corners. Points 2 and 3 seem to indicate identity of the notions as to what happened to a person after death. 4) Funeral meals were held after burial. 5) Libations seem to have been made after the graves were sealed. 6) Mounds of earth were poured over the shaft graves and the tholos tombs. 7) Stelai were erected over the graves. 8) Tending of graves after burial was not practised.

It may be remarked that the coming of the new people was not associated with the shaft graves by Palmer, but with the tholos and chamber tombs. However, it was placed at *ca.* 1600 B.C. and that date "belongs" to the shaft graves. But perhaps it may be suggested that the intrusion could be dated a little later, its date might be changed by a quarter or half a century, so that it could be connected with the tholos and chamber tombs. This change will not help matters. We have seen above that the tholos and chamber tombs do not follow the shaft graves in a clear-cut order, that the earliest tholos tomb does not follow the last shaft grave, but that the different types of graves co-existed for some time. Because of this fact, if we associated new people with the tholos and chamber tombs we would be forced to assume that the intruders in control of the territory allowed the princes, whose rule they had usurped, to bury their dead with so much gold and with so many valuable objects as are contained for example in Shaft Grave V, the grave that seems to be one of the latest in date. This seems unbelievable.

Again we have seen how the shaft graves and the tholos and chamber tombs exhibit identical burial customs and how they differ only in the form of the sepulcher. An intrusive people that is assumed to have introduced different types of graves, types that are as elaborate as the tholos and chamber tombs, would have had their own characteristic burial customs. It would have been a coincidence miraculous indeed if those customs proved to be identical to those of the different people whom they conquered; equally fantastic would be to assume that the conquerors with the elaborate tholos tombs would have abandoned their own ancestral habits of burial and adopted *in toto* the burial customs of the people they conquered. We have admitted that the tholos and chamber tombs could not have evolved from the cists and the shaft graves of the Middle Helladic period. But we should also stress the fact that they could not have been introduced by Greek-speaking invaders, whether we bring them from the north, from the northeast or the southeast, from Central Europe, the Balkans, or Asia Minor, because similar graves are not to be found in those districts. On the other hand the chamber tombs could have been inspired by the rock-hewn graves of the Middle Kingdom of Egypt, with which the mainlanders seem to have come in contact towards the end of the Middle Helladic period. The tholos tomb could be conceived as an elaborate, royal version of the chamber tombs devised and produced in Greece, that is, it could be conceived as a local, mainland development. In a similar manner the Middle Helladic inhabitants developed from the rock-cut cists the more elaborate shaft graves for the use of royalty. Thus the different types of graves could be accounted for and there remains the bulk of burial customs to prove the normal and continuous development of the one from the other period. By now, I hope, it has become evident that there is no archaeological evidence that will indicate, let alone prove, the arrival in the Mainland of Greece of a new wave of people either

around 1600 B.C. or at the time when the tholos and chamber tombs were introduced in the life of the Bronze Age.

The invasion of Crete by the Mycenaean-Greeks, or the Achaeans, will have to await, I am afraid, additional verified evidence. Professor Palmer's challenge of the date accepted for the Knossian tablets in Linear Script B seems to be well taken and will require study and further scrutiny.⁴⁹ The appearance of Linear Script B at a later than the assumed date seems to be indicated by the evidence obtained in the Mainland.⁵⁰ The later date will certainly eliminate some difficulties, but this relief should not alter facts. At present facts seem to be hard to establish. Let us hope that additional evidence will be obtained from future excavations in Crete. An invasion and conquest of Crete at the end of the Late Minoan II period, in the course of which the palaces were destroyed, seems possible. That event would have given to the mainlanders the freedom of action indicated by their expansion to Rhodes and the East.⁵¹

There is one point, however, that has been connected with the destruction of the Late Minoan II palace at Knossos that has to be straightened out; that is the date of the destruction of the palace at Thebes. The date of that destruction was suggested originally by its excavator, the late Professor A. Keramopoulos, and it was based upon some fragments of pottery and frescoes.⁵² Rodenwaldt maintained that the fragments of frescoes found in the palace of Thebes were contemporary with the frieze from the palace of Mycenae.⁵³ That frieze belongs to Late Helladic III B times, to later times, that is, than *ca.* 1400, the date suggested for the Theban destruction. The sherds, illustrated by Keramopoulos, that form his ceramic evidence belong to Late Helladic III B advanced times. Furthermore in "corridor D," as he called it, among the debris of the destroyed palace he discovered twenty-eight inscribed stirrup amphorae. These certainly must date the destruction. Elsewhere, I have tried to prove

⁴⁹ Palmer, *op. cit.*, pp. 156 ff.

⁵⁰ The inscribed stirrup amphorae from the Mainland were dated too high, *ca.* 1360 B.C., by Ventris and Chadwick (*Documents*, p. 38.) Sir Arthur Evans maintained that their date "should not be brought down to a later date than the close of the Fourteenth Century B.C." (*Palace of Minos*, IV, p. 755.) They cannot be proved as closing the gap that exists between the end of LM II and the Pylos tablets. Placed in the thirteenth century, they would come into reasonable relationship with the tablets of Pylos and with the proposed later date of the Knossian tablets.

⁵¹ The emphasis placed on the strategic importance of the island of Crete in Mycenaean times should be modified. In the present era of the airplane, the submarine, and the missile, the location of the island is of paramount importance. But in the days when the open sea was avoided by mariners, when coastal sailing was almost the rule, when the sea distances had to be negotiated by sail boats, an island located in the midst of a broad sea could not very well control the sea lanes the way it does today.

⁵² A. Keramopoulos, *Ἀρχ. Ἐφ.*, 1909, pp. 105 ff.

⁵³ Rodenwaldt, *Tiryns*, II, pp. 96 ff., 188 ff. and chronological table on p. 201; *Jahr.*, XXXIV, 1919, p. 99, n. 2, cf. Keramopoulos, *Ἀρχ. Ἐφ.*, 1909, pp. 57 ff., 1910, pp. 177 ff. *Δελτ.*, 1917, p. 339, fig. 193.

that those stirrup jars belong to the thirteenth century.⁵⁴ Professor Palmer, concurring with this opinion, states that the inscribed stirrup amphorae from the Mainland are almost contemporary with the tablets of Pylos and consequently places them to the very end of the thirteenth century. This has to be reconciled with the date he seems to accept for the destruction of the palace. Either he has to admit that the palace was destroyed not shortly after the destruction of the Late Minoan II palace at Knossos, i.e. shortly after 1400 B.C., but long afterwards, sometime towards the end of the thirteenth century, or he has to date the inscribed stirrup vases, found in its debris, before 1400 B.C. I believe that he will agree with my placing the destruction of the Theban palace in the thirteenth century B.C., possibly in the second half of that century, and see in it a reflection of the story of the wars against that city by "the Seven" and their descendants, who, according to tradition, in the years that preceded the Trojan expedition destroyed the famous city founded by Kadmos.

Perhaps a note of caution should be sounded regarding the interpretation of the lack of change in the signs of Linear Script B as illustrated by the tablets of Knossos on the one hand and by the documents of the Mainland on the other. The same lack of change is noticed in the ideogram representing the corselets of the period, and this is also taken to indicate a late date for the Knossos tablets. "If the two archives," it is stated (i.e. the Pylos and the Knossos archives), "are dated respectively to the beginning and the end of the Mycenaean power curve, then we must conclude that throughout these two eventful centuries of military enterprise protective armour remained virtually unchanged. The record of man's inventiveness in time of war, and in particular the development of the Mycenaean sword, would lead us to expect considerable differences between the corselets of the fifteenth and the twelfth centuries B.C." Even before this statement was published, in May, 1960, the ephor of the Argolid Dr. N. M. Verdelis and the Director of the Swedish School of Archaeology in Athens Dr. P. Astrom discovered in a grave at Dendra a bronze corselet that is identical to the type represented by the ideogram. As a matter of fact the find has yielded an explanation for the *qe-ro*₂ of the tablets. The corselet was found with pottery that will date it with accuracy. According to the discoverers, the pottery dates from LH II B-LH III A: 1; it belongs in other words to the second half of the fifteenth century.^{54a} If the scribes were as conservative as the corselet makers, no great change in their product could be assumed with any degree of certainty.

The question of the general picture of Crete after 1400 B.C., that is, in the Late Minoan III period, will be revived by Professor Palmer's belief that in that period the island "was no backwater, no place of ghosts and mouldering staircases. It was

⁵⁴ Mylonas, *Ἀρχ. Ἐφ.*, 1936, pp. 64-70 and *A.J.A.*, XL, 1936, pp. 426-427.

^{54a} Palmer, *op. cit.*, p. 179. N. M. Verdelis, *Ἀρχ. Ἐφ.*, 1957 (circulated in 1961), *Παραρτ.*, pp. 15-18.

a flourishing Achaean power, as Homer depicts it, a centre of arts and crafts, exporting metal-work to the mainland and doubtless much else," especially perfumed oil for religious purposes.⁵⁵ The concept of the export of metal work is apparently based on the four tripod cauldrons of Cretan workmanship mentioned in the Pylos tablets TA 641 and TA 709.⁵⁶ One wonders whether four cauldrons, which could have been brought in by marauders or travelers, are sufficient to prove that Crete "was a flourishing centre of arts and crafts" in Late Minoan III times, that its people exported metal work or that the "dynasty of Knossos . . . could vie with the lords of Mycenaean Greece at the height of their powers." It will be remarked, however, that besides the cauldrons we have the industry of the perfumed oil to back the assertion. It is claimed that Crete enjoyed "virtual monopoly in the export of that commodity." Can this be proved?

The export of perfumed oil from Crete does not seem to be what it is claimed, nor the monopoly of the island. The claim is based on the assumption that all the inscribed stirrup amphorae found in the Mainland contained perfumed oil and were imported from Crete.⁵⁷ In a number of examples from Thebes are recognized "three words in a pattern." "1) a personal name, 2) a place name, and 3) a personal name in the genitive or an adjective meaning royal." This certainly is a full commercial documentation recalling the labels of perfume bottles from Paris, but rather doubtful for the Mycenaean age. But who is the person whose name appears on the label? Was he the "unguent boiler," as he is called on the Pylos tablets, or the owner of the factory? The specialists tell us that the administration of the Achaean state, as it is indicated by the tablets, controlled absolutely the commercial activity of its communities. We would expect therefore the *Wanax* of the Achaean state of Crete to control this very important enterprise, the export of perfumed oil. This may also be indicated by the "adjective meaning royal" that is recognized in some inscriptions. Is then the personal name on the amphorae that of the *Wanax*? But then how can we explain so many different names on contemporary jars found in the same place, and derived from the same source? If it is not the name of the *Wanax*, is it then that of his administrator, for it cannot be the name of the owner of the establishment since the factory presumably belonged to the State, to the *Wanax*. Is the custom of naming the administrator current in the Knossian tablets?

On the Pylos tablet Fr 1184 we find the text: "Kokalos gave so much oil to Eumedes" and both these individuals are known from other texts to have been "unguent-boilers." On Pylos Un 08 we read: "How Alxoitas gave to Thyestas the

⁵⁵ Palmer, *op. cit.*, p. 225.

⁵⁶ Ventris-Chadwick, *Documents*, Nos. 236 and 237, pp. 336-337.

⁵⁷ Palmer, *op. cit.*, p. 213. His discussion of the inscribed stirrup amphorae from Thebes, Eleusis, Tiryns, and Mycenae is contained in pp. 167-169. For the Cretan monopoly see p. 213.

unguent-boiler spices, for boiling in the unguent.”⁵⁸ Below this introductory statement we find the amounts of coriander, ginger-grass, wine and honey that were given to Thyestas. Could this Alxoitias be an “administrator” in charge of a perfumed oil establishment? If the *A-ko-so-ta* of the tablets stands for the name of Alxoitias, and if all references to that name belong to the same individual, then he must have been not only a very busy man, but one whose activities were not limited to the oil business. For we find him on a tour of inspection of various localities to record the grain he “saw,” or else “receiving” amounts of an unidentified nature, “giving” materials to an unguent-boiler, dealing with ivory, but above all prominent in the great cattle inventories.⁵⁹ The palace administrators seem to have been responsible for varied activities, one of which might have been the giving of “spices” to the unguent-boilers. But it seems that the latter were responsible for the making of the perfumed oil. Their names therefore would have been more appropriate for the label on the jar, since their achievement would have contributed to the reputation of the product.

The tablets found at Mycenae contain the amount of spices issued to a number of persons; the same tablets come from the “perfume factory” of that city. They do not indicate the existence of one general administrator of the establishment. Would the names of “unguent-boilers” and “stewards,” who may have been issuing the spices, be of sufficient importance to be placed on merchandise meant for export? Will it not be more appropriate to believe that the name of the *Wanax*, who controlled the enterprise, would be placed on objects for export, acting as a guarantee for the quality of the merchandise? But we have seen above the difficulties that result from such a supposition.

In “Corridor D” of the palace in Thebes were found twenty-eight stirrup amphorae belonging, apparently, to the same period, but bearing a variety of “place-names” and “personal-names.”⁶⁰ Were they shipped from different manufacturing centers in Crete? Yet other “place-names” are to be found on the examples from Eleusis, Tiryns, and Mycenae. Were there so many different places in Crete where the industry of the “dynasts” of Knossos was flourishing? But can we be sure of the “place-names” being what they are claimed to be? Can we be sure that they did not indicate something else? In modern times the “place-name” London is also the personal name of one of the leading artists of the Metropolitan opera. And one could find a legion of such “coincidences.” In the *wa-to* of Theban jars was recognized a Cretan “place-name.” But it is equally clear that on other Theban jars the syllables

⁵⁸ Ventris-Chadwick, *Documents*, p. 217. Bennett, *Olive Oil Tablets*, pp. 40-41. Palmer, *op. cit.*, pp. 70 and 107.

⁵⁹ Ventris-Chadwick, *Documents*, p. 200, p. 224, No. 103; No. 154, p. 268; No. 249, p. 348. *Archaeology*, IX, 1956, photograph on p. 276. Palmer, *op. cit.*, p. 101.

⁶⁰ Keramopoulos, *Παράρτημα*, 1921, pp. 32-34 and 1922-23, pp. 28-31; *Ἀρχ. Ἐφ.*, 1930, p. 33. Sir Arthur Evans, *Palace of Minos*, IV, pp. 739-744, figs. 724a and 724b.

wa-to form the ending of multisyllabled words. What is their meaning in the polysyllabic words? It could be surmised that in those instances the polysyllabic words end in two signs that are identical to those of the "place-name." It could equally well be surmised that when the two signs are found by themselves they do not indicate a place-name but they stand for the abbreviation of a polysyllabic word, the way the sign *wa* on the Eleusinian amphora is assumed to be the abbreviation of "royal." And if indeed these signs give us a place-name, how can we be sure that it is not on the Mainland as well as on Crete? Let us look more closely at one or two inscribed sherds that have been published. A sherd from Mycenae is brought forth as an establishing factor of Cretan place-names on the inscribed amphorae. "To cap it all," it is stated, "another painted sherd from Mycenae also exhibited a Knossian place-name (*E-ra*)." But is not the same word *E-ra* on the Pylos tablets taken to be the name of the Goddess Hera? And furthermore, is it not possible to assume that the two signs are the abbreviation for the word Olive Oil? ⁶¹ What is it that will make it necessary for us to accept that a Cretan place-name and not that of the Goddess or of olive oil was inscribed on the sherd?

On a stirrup amphora from Tiryns the word *Pa_s-ko-we* is read, a word that figures on a Linear Script B tablet from Knossos and is there taken to be a place-name. This place-name is assumed to be another indication of Cretan provenience. But on the Olive Oil tablets from Pylos we find another *pa-ko-we* which is used in connection with oil and is taken to mean "sage-scented" (oil). I submit that an inscription meaning "sage-scented" (oil) on an amphora that contained oil would make more sense than the name of a locality perhaps, standing by itself. I realize that the first sign in the two words *pa-ko-we* is different. The one is *pa_s* and the other is simply *pa*. They are, however, homophones and it would be understandable if the scribe by mistake used the one for the other. Are we sure of the role of these homophones in the Script? Furthermore, Bennett states that sometimes the spelling *e-ra-wo* was used for the commoner form *e-ra_s-wo* on the olive oil tablets of Pylos.⁶² In other words, the one homophone *ra* was used instead of the other *ra_s*. In the same way instead of *pa_s-ko-we* we could have *pa-ko-we*. And as I said above the "sage-scented" oil would make better sense on an oil jar.

In my study of the Eleusinian amphora,⁶³ I mentioned the fact that its clay seems to be local. The mica it contains is identical in form and color to that contained in coarse ware of the Middle Helladic period. Since we cannot possibly imagine that the latter was imported, the mica it contains will indicate the nature of the clay in which

⁶¹ Ventris-Chadwick, *Documents*, p. 286, No. 172 for Hera.

⁶² Bennett, *op. cit.*, p. 16.

⁶³ Mylonas, *A.J.A.*, XXXVIII, 1934, pp. 427, 431. Perhaps we should note that Palmer (pp. 168-169) wrongly states that the inscribed amphora was found "under the Hall of Initiation built by the Athenian tyrant Pisistratus." It was found below the "Lesser Propylaea."

similar mica is to be found. If the jar of Eleusis is of local clay, how can it have been imported from Crete? What of the clay of the Theban specimens? Is it Cretan?

Stirrup amphorae without inscriptions, as far as I know, have been found in Egypt and also in Troy, among other places. They presumably held perfumed oil. Some of them at least were exported from the Mainland of Greece.⁶⁴ Shall we assume that the mainlanders exported perfumed oil, while at the same time they imported a similar commodity from Crete? Fragments of inscribed stirrup amphorae were found in Mycenae and Tiryns. We shall have to assume that these too were imported from Crete. We may now recall that Professor Marinatos was the first to point out that the houses revealed by Wace beyond the walls of the citadel could not be the "House of the Oil Merchant," but an annex to the palace of the King of Mycenae where perfumed oil was manufactured. His opinion has now the weighty support of Professor Palmer who writes: ⁶⁵ "Professor Marinatos and I have independently studied the facts from different angles. We both come to the conclusion that Professor Wace was mistaken in believing this house to be the house of an 'oil merchant.' It seems most probable that this house just outside the walls was an annex of the Palace concerned with the making of unguents. That it was the unguent kitchen is consistent with the fact that Wace discovered an arrangement for heating jars in one corner. From ancient descriptions we know that the aromatic substances were gently heated in the oil." Furthermore, in his excavation of 1959 Dr. Verdelis seems to have discovered the "fire-place" where cauldrons could be heated whenever more heat was required. The establishment continued active to perhaps the end of the Late Helladic III B period. Is it possible to maintain that the ruler of Mycenae imported perfumed oil from Crete, as the fragments of Mycenae and its dependency Tiryns would imply, when he had his own, very large for the times establishment?

The tablets found at Mycenae "were concerned with the issue of 'spices' to a number of persons. The substances are coriander . . . red and white safflower, penny royal, ginger-grass, cumin, fennel, mint, sesame, and celery." The Olive Oil tablets from Pylos prove that an extensive enterprise concerned with the manufacturing of perfumed oil existed in that city. Five tablets recording condiments from Knossos were published by Ventriss and Chadwick (Nos. 98-102), but there is no indication that these were for the manufacture of perfumed oil. And nowhere in the island as yet have been found architectural remains that could be attributed to a "perfume factory." Perhaps this negative evidence, that may be supplemented by later discoveries, does not disprove the existence in Crete of the industry in perfumed oil, but the positive evidence from Mycenae and Pylos proves decidedly that Crete did not enjoy "a virtual monopoly in the export of" perfumed oil. The manufacture of per-

⁶⁴ Blegen, *Troy*, III, p. 74, figs. 330-331.

⁶⁵ Marinatos, *Πρακτικά 'Ακαδημίας*, 1958, pp. 161-173. Palmer, *op. cit.*, p. 108.

fumed oil at Mycenae and Pylos is definitely proved by actual remains and the commercial activities of the Mycenaeans in the Late Helladic III period are well attested. It would, indeed, be strange if the Mycenaean lords had taken no advantage of their industry and far-flung commercial activities to produce and export a commodity which proved so popular at the time. Certainly, they did not lack the primary commodity, the oil.

The general conclusion from this detailed analysis seems to be that at present we cannot be sure that Crete in Late Minoan III times was a "flourishing Achaean power . . . a centre of arts and crafts," a power that "enjoyed a virtual monopoly in the export of oil" or one that could "vie with the lords of Mycenaean Greece at the height of their powers." The picture of Crete that is available to date is rather vague and modest. More excavations and more remains in the island itself will perhaps clear the picture and will determine the exact place of Crete in the Mycenaean world of the closing centuries of the Bronze Age.

We may now summarize the results of our survey. I believe that I have indicated that a Luvian invasion and occupation of the Mainland of Greece at the close of the Early Helladic Period is not substantiated by the archaeological evidence available to date. The survey of our data indicates the impossibility of bringing the "Greeks," that is Greek-speaking Indo-Europeans, into the Mainland *ca.* 1600 B.C., and of attributing to them the tholos and chamber tombs of the Mycenaean world. It seems that no other new elements of culture appearing suddenly do exist that would indicate the arrival of the "Greeks" after that date. The establishment of Luvians in Crete *ca.* 1700 B.C. at present cannot be proved. More evidence, archaeological and linguistic, both from Crete and especially from Asia Minor, will be required before such an intrusion becomes acceptable. In the tablets of Linear Script A we have perhaps the source which might ultimately give us the proof one way or another. Let us hope that, in our times, the "decipherment" of that script will crown the efforts of the scholars working on it. An Achaean invasion of Crete *ca.* 1400 B.C. seems quite possible.

The available archaeological evidence definitely points to the conclusions generally accepted, conclusions that were repeated recently by Professor Dow and which can be summarized as follows:

- 1) At the end of the Early Bronze Age, at *ca.* 1900 B.C., the Mainland of Greece was invaded by a new wave of people who established themselves especially on its eastern half. Before we can establish the area of their provenience we have to obtain by excavation more information regarding the cultures that were developed in the Early Bronze Age in the regions adjacent to the Greek peninsula, especially in Asia Minor. Perhaps the fact that the Middle Helladic invaders seem to have been established first in the area of Boeotia-Phocis, in northern Greece that is, may indicate that they came from the northeast either by sea or by an overland route.

2) These Middle Helladic invaders seem to have been the first wave of Greek-speaking Indo-Europeans that came to the Greek peninsula.

3) We surmise that they were Greek speaking, because the culture they produced gradually, without "breaks," and under Minoan and other external influence, developed into that of the Mycenaean Age. To the last sub-period of that Age, to Late Helladic III, belong the documents inscribed in Linear Script B, whose language is proved to be Greek. And since there is no evidence to prove that another wave of invaders with a different culture established themselves in the Mainland of Greece in the period between Middle Helladic and Late Helladic III times, and since we have to bring in Greek-speaking Indo-Europeans before 1400 B.C., it is possible to conclude that the Middle Helladic invaders were the ancestors of the Greek-speaking Late Helladic III inhabitants of the Mainland.

4) The "Mycenaean"—"Achaean" rulers of the Mainland attained a high degree of culture and power and finally overthrew the Minoan State. They invaded Crete, destroyed its palaces, established themselves at Knossos at least, and exercised control over the entire island. The date when these "Achaeans" conquered Minoan Crete has still to be proved. The conception that the conquest of Knossos occurred at the end of the Late Minoan I period, *ca.* 1450 B.C., that was based on Sir Arthur Evans' stratigraphy, is now being challenged. To us it seems reasonable to accept Professor Palmer's interpretation, which in fact is Sir Arthur's conception, that the Achaean conquest occurred *ca.* 1400 B.C., that is at the end of the Late Minoan II period of Knossos. However, we must emphasize that new, verified evidence has to be obtained to establish the date of that conquest. Furthermore, we must emphasize the fact that the archaeological evidence available to date does not prove that the Achaean State of Crete of Late Minoan III times rose to such power and artistic and commercial influence as to rival that of the Mycenaean Lords of the Mainland. The picture might change if more evidence is revealed by excavation.

5) The end of the "Mycenaean" world came some time after the Fall of Troy, which I maintain occurred around the date indicated by the prevalent ancient tradition, i.e. after 1200 B.C.⁶⁶ The end of the Mycenaean world came after the middle of the twelfth century B.C. The reputed "Descent of the Herakleidai" seems to have given the "coup de grace" to the Mycenaean States, especially to the state of Mycenae, weakened by long wars and internal strife indicated by the feuds and killings within the royal family of Atreus and Agamemnon.

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⁶⁶ I regret that I cannot accept the date suggested by Blegen and his collaborators (*Troy*, IV, p. 6-12). For my reasons, see my study on the date of the Fall of Troy in the *Yearbook* of the School of Philosophy of the University of Athens, 1959-1960, pp. 408-469.