THE LINGUISTIC EVIDENCE: IS THERE ANY?

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1. For the place and period we are interested in, the Early Bronze Age in the Aegean, there is no direct linguistic evidence, in the sense that we have no contemporary written documents from any part of Greece, or indeed any part of the West Coast of Asia Minor. The only conceivable exception could be provided by some texts in Cretan Hieroglyphic and by the few fragments of Linear A or Proto-Linear A found at Phaistos, if the very early date suggested by Levi is accepted—and even so there is no way of going back earlier than MM I. However, a) the dating is always disputed (especially in the case of Linear A at Phaistos), and b) the texts are too fragmentary to be of any use. So much of course will come as no surprise to any one. Unfortunately, most of the content of this paper will also cause no surprise, since it merely provides a restatement of the obvious or, when the problems are controversial, a statement of my own opinion. I have taken as my brief to deal exclusively with linguistic evidence and to make no attempt to rely on data of an archaeological or mythological or historical nature. This is, of course, not a procedure which is likely to lead to any satisfactory conclusion. A conclusions, if any, will come from combining different types of evidence, but in the present semi-chaotic stage of the discussion, when hypotheses and counter-hypotheses are thrown in from all directions without much hope of a cogent demonstration, it may be wise to try to isolate the conclusions which each approach can reach on its own, before we try to adopt a more sophisticated procedure. This will, at least, prevent those perpetual misunderstandings which induce archaeologists and historians to believe that the linguist has the answer—and vice-versa. Even so, the linguist is often reduced to complete silence in this field, unless he can count on some extra-linguistic data. I am no exception to the rule, but when this occurs I shall try to make it explicit.

2. There is no direct linguistic evidence for the Early Bronze Age, but this does not imply that there is no linguistic inference that can be made. What we do have, of course, is linguistic evidence from a later period and we are well accustomed to reconstructing earlier linguistic situations from later evidence.

The first question then concerns the later evidence, its nature, its date, and its geographic distribution.
There are at least three different types of evidence: 1) written evidence which we can read and understand (such as the Greek texts of the first millennium BC); 2) written evidence which we can read (in the sense that we can assign a phonetic value to the signs of the script) but we do not understand (this is the case, e.g., of the so-called Eteocretan texts of the first millennium); 3) written evidence which we cannot read and cannot understand (an example is provided by the Cretan Hieroglyphic texts of the second millennium). In listing these data I shall limit myself to the classical and pre-classical periods and to what can be loosely called Greek territory. I shall not try, at this stage, to discuss the evidence from Asia Minor, simply because it is too complicated to allow a brief survey; later on I shall have to consider Near Eastern evidence.

2.1. My first type of evidence mainly includes Greek, though this language is attested at different phases of its development and in different scripts. Starting from the eighth century BC we have evidence for alphabetic Greek on the mainland and in the islands. From the same period we have evidence for Greek written in a syllabic script in Cyprus. Very recent findings, though fragmentary, show that the Cyprian syllabary was used as early as the eleventh century BC (Karageorghis 1980: 134-36). At a date, which is not entirely clear, but which is normally put between the fourteenth and the twelfth centuries BC, we have evidence for Greek written in Linear B in Crete and in the mainland.

In the first millennium we also have evidence in Cyprus for Phoenician written in the Phoenician alphabet, but this fact adds little to our knowledge and we shall not mention it again. Similarly we do not need to consider isolated imports of Oriental origin, such as the second millennium cuneiform and hieroglyphic cylinders from Thebes (cf. Touloupa 1964; 1965: 290-301; Falkenstein 1964; Lambert 1964; and now Porada 1982).

2.2. The second type includes peripheral evidence. Nothing comes from the mainland, since I shall ignore the Carian "bilingual" from Athens and the odd miscellaneous objects of oriental origin: in all cases we deal with isolated imports which bear witness to commercial relations, without even giving any idea of their magnitude.

There are a number of other features which are particularly significant in helping us to understand the linguistic evidence: the development of the language itself, the nature of the scripts, the nature of the inscriptions, and the distribution of the evidence. These are all issues which will be discussed in detail in the next section.
BC and so far they remain incomprehensible, though a few infrastructural elements have been identified.3

2.3. My third category includes documents which we cannot read and cannot understand. Here too, the data come from peripheral areas: Crete, Cyprus and some of the islands. There is no clear evidence from the mainland; a few odd signs of Linear A appearance, mason marks, etc. have been found but it is difficult to know if they must be treated as writing, and in some instances they may belong to imported objects.5

In Crete, in addition to Linear B, there is evidence for at least three different scripts, all dating from the second millennium BC.7

I shall just mention the Phaistos disc: the origin of this curious object, which is normally dated to a period between MM II and MM III B and was found together with a Linear A tablet, is still obscure and there is no agreement whether it was imported or local. It is unique, and—in spite of its too numerous decipherments—is unlikely to provide us with much help.6 A better localised script is the so-called Hieroglyphic Cretan. Olivier (1976: 17) has correctly pointed out that we know practically nothing about this script except that it is not hieroglyphic and is not pictographic. It is attested—in a scanty manner—in various parts of Crete in the period which goes from MM I to MM III or perhaps even LM I, and, at Mallia and Phaistos at least, seems to overlap, in a somewhat evolved form, with Linear A texts. The few remains that we have do not certainly allow a decipherment, nor do they allow us to define the connection with Linear A or Linear B. The chances are that there is indeed a graphic connection between Cretan Hieroglyphic and Linear A, but this (a) is not proved, and (b) even if it were proved, would not tell us anything


4 Grünbach (1962: 85) drew attention to the discovery of two Linear A signs on a copper vessel from Shaft Grave IV at Mycenae (cf. Hooker 1979: 46). The object may be imported from Crete so that, even if the signs may be treated as a form of writing, this does not contradict the statement made above.

5 Hooker (1979: 16) recognizes “no fewer than seven Minoan writing systems” and mentions (in addition to Pictographic, Linear A, Linear B, and the Phaistos disc) the Askalochori axe, a clay tablet from Tyllirissos (TV 2), and a fragmentary clay tablet from Knossos, all of which show signs which cannot be identified with those of Pictographic or Linear A. The evidence is very small and cannot be assayed with any confidence, so that I feel entitled to ignore the last three objects mentioned.

6 We should remember that the date of the disc is far from certain. An up-to-date introduction to the problems of the Phaistos disc is provided by Dubosc (1977, with the earlier literature). For a photographic edition cf. Olivier 1975 (1), 1973. A case for the Cretan origin of the disc is made by Hooker (1979: 16-17). Cf. also Heubeck 1979: 9.

7 For a recent introduction to Cretan Hieroglyphic see Olivier 1976, and now Henckel 1979: 2. The richest edition of texts is still Evans 1909, but for the new findings of Mallia cf. Pourat et al. 1978.

8 Pope and Rainos (1978) give a very detailed and up-to-date account of Linear A and its problems (with ample bibliography). For a list of editions see Rainos and Pope 1977; cf. also Rainos and Pope 1980. GORILA, the new edition by Godart and Olivier, will eventually appear in five volumes but is not yet complete (Godart and Olivier 1976-). Also important as introductions are Olivier 1973 (2) and Godart 1976. Cf. also Heubeck 1979: 11.

9 The earliest Enkomi clay tablet dated at the end of the sixteenth century shows clear graphic similarities with Linear A (Mason 1965).


about the language thus written. If the two scripts overlapped geographically and chronologically, it is quite possible that they were used for different languages, but even this is not certain.8

Finally we have in Crete a great deal of evidence for Linear A. It is not often realised how greatly our evidence has increased in the last few years. Brice’s collection (1961) is being replaced by a five-volume edition by Godart and Olivier (1976--) which includes a much larger number of documents. Linear A texts span a period which more or less coincides with MM and LM I; it is obviously the normal form of writing for the second palaces. There is a certain amount of evidence which may indicate that the Linear A inscriptions, which now come from all parts of the island (Zakro in the South East, Ayia Triada in the centre and Khania in the North West, to give examples) are written in the same language. Certainly the word which means ‘total’ is used both at Zakro and at Ayia Triada.10

The next port of call is Cyprus. Here we have evidence for writing in the second millennium and there is a graphic connection between the writing of the second millennium and the syllabic script(s) of the first millennium. In Cyprus, E. Masson has found evidence for two related forms of script, which he assumes corresponded to two different languages. The two scripts are closely related to each other and (less closely) related to the Cretan scripts.11 The first script (Cypro-Minoan I, in her terminology) is attested between the sixteenth and eleventh centuries in various parts of Cyprus and on a number of different objects; the second script is attested in Enkomi only in the thirteenth-twelfth centuries. Finally, a third variety of Cypro-Minoan was in use at Ras Shamra-Ugarit. According to Masson, the second script, which is attested in large tablets, may represent the adaptation of the standard Cypro-Minoan script to the language of a group of newly-arrived peoples, whom he tentatively identifies with the Hurrians.12

In the Islands we have in the second millennium a few very fragmentary bits of evidence for Linear A at Thera (Marinatos 1971: 64), Keos (Caskey 1970), Melos...
to different phases of the same language. Similarly we do not know if Eteocretan is at all related to any of these languages.

Also in the second millennium Cyprus may provide evidence for two different languages, if we follow Mason; what these languages were it is not possible to say with confidence. In the other islands of the Aegean Linear A was occasionally used and it is possible that it was used for the same language as in Crete. Given the nature of our evidence no linguistic proof is available nor do we know that the texts were produced locally.

3. What conclusions do we draw from this summary of the evidence? First, we know that Greek is Indo-European, while we are, at the moment at least, entirely ignorant of the linguistic affiliation of the other language(s)—obviously so for those written in a script which we cannot read, unfortunately so for those written in a script which we can read. Secondly—and here I move away from a strictly linguistic approach—it seems likely that Indo-European, or rather that form of Indo-European which became Greek, did not arise in Greece itself, nor reached Greece strictly through diffusion without any movement of peoples. In other words, most linguists still wish to assume that the Indo-European languages were brought into their Mediterranean places at a relatively late stage of their history and that the Urheimat of the Indo-Europeans was not in the Mediterranean area. Objections have been raised against the expression "the coming of the Greeks" because it makes little sense to refer to Greek speakers before the period when the language acquired its definitive features, and this no doubt happened in Greece itself. While accepting the point, it is possible to argue that it makes sense to speak of the arrival of Indo-European-speaking groups, which in the course of time altered their form of speech and in Greece started speaking something which we would recognize as Greek.

Given all this, and given the areas of allophony in Greek territory in the first millennium, we may now ask whether Eteocretan and Eteocyprian (I shall leave aside the question of Lemnos) are to be treated as due to the superimposition of foreign speakers on Greek or vice-versa. In other words, is the situation similar to that which led entire sections of New York or Philadelphia to be italophone, or is it comparable to that which confronts a tourist in the United States, who is lucky enough to enter an Indian reservation and to find someone who speaks an Indian language? This is largely a matter for archaeologists and historians to decide; I
should instinctively prefer to assume that Eteocretan and Eteocyprian represent pockets of a language which retreated in front of Greek rather than vice-versa. If this is so, they may continue languages spoken in the islands in the second millennium. However, as I mentioned above, it does not follow that they continue the languages for which we have evidence written in Linear A or Cretan Hieroglyphic or Cypro-Minoan 1 or Cypro-Minoan 2—though this is possible. In addition to this, the linguist cannot have anything to say about the time when the speakers of these languages reached their historical places. Something could be said, if we understood the texts and recognized the genetic affiliations of the language—but this is just what we cannot do (for Lowian see below 4.4.4.).

4. I now turn back to Greek and to the inferences which we can make from the features of this language as we have it in the second and first millennia BC. There are a number of ways in which the problem can be approached.

4.1. The language is Indo-European and, as stated above, this implies, in my opinion, that there was a group of Indo-European speakers which at some stage reached Greece from the Unetnait—though obviously nothing proves or even suggests that migratory groups moved in a straight line from the Unetnait to Greece. If we could identify the Indo-Europeans with a particular culture which has left archaeological remains of one or the other type and is, for instance, associated with a particular type of pottery or a particular type of burial, it would be possible to say something about the arrival of the Indo-Europeans in Greece and its chronology. Yet in this linguistics does not help. Hypotheses have been made about the archaeological aspect of the Indo-Europeans, but there is no agreement about them and I find it difficult to see how, in the absence of any written evidence contemporary with the relevant period, a purely linguistic concept can be confidently associated with archaeological data: in other words I share some of the misgivings expressed by Schmitt (1974). The problem concerns more the archaeologist than the linguist and, in view of the disclaimer made above (1.), I cannot tackle it here; any solution is fraught with difficulties and by its very nature is such that the linguist cannot use it as the foundation of his edifice.14

14 A useful collection of papers on the subject was edited by Scherrer (1968); for a historiographical introduction to the Indo-European problem see Mallory 1973; for a bibliography and a recent reappraisal of the situation, Crosland 1971 (1), 1972 and 1973. Schmitt’s sceptical remarks called for a reply by Gimbutas (1974); cf. also Gimbutas 1970 and, in general, most of the essays collected in Gardnaa et al. 1970. Wyatt (1970) has argued that the linguistic evidence shows that the Indo-Europeans arrived in Greece ca. 1600. I suspect that his data could be explained in a different way.

4.2. Another approach also starts from Greek but in a more modest way. If we accept Ventris’s decipherment of Linear B it follows that there were Greeks who wrote Linear B in Greece in the last part of the second millennium BC. It is well known that in the first millennium Greek is fractioned in dialects and that in some instances their geographic distribution can only be explained through a number of historical assumptions. The similarities between Ionic and Attic, on the one hand, and between Lesbian, Boeotian and Thessalian, on the other hand, can be accounted for if we assume that groups of people moved from the mainland to the coast of Asia Minor sometime during the period of the Greek colonial expansion.15 This is a clear-cut instance, but there are more complex problems. We wonder why Arcadian is so closely related to Cyprian, though it is so isolated in the centre of Peloponnesse, surrounded as it is by Doric and West Greek speakers. We can guess that the ancestors of the Arcadians occupied large parts of the South and West Peloponnesse and perhaps extended their sphere of influence as far as Cyprus. This hypothesis can be made on purely linguistic grounds; the close connection between the Mycenaean language, as attested in the Linear B texts, and Arcado-Cyprian now confirms this view. The example is relevant because it shows that an analysis of dialects and their distribution may lead to sound historical conclusions. Is it possible to explain in a similar manner the existence and distribution of the main dialect groups of Greek: Aeolic, Ionic-Attic, Doric? According to the traditional explanation, which is based on such an assumption, the tribes which brought Indo-European to Greece arrived in waves separated in each case by a number of years. The three main dialect groups represent each a different wave of invaders. If that were so, and if we could assume that the authors of our Mycenaean texts were the last-but-one wave of invading tribes (preceding the Dorians only), it should be possible to date the first wave to a relatively early period, possibly as early as the end of EH or the beginning of MH. Presumably a linguist could produce this date only as a terminus ante quem and not as a terminus post quem, since the whole process, including the arrival of the future Mycenaean speakers, could have taken place at any time, albeit in the staggered manner described above. The conclusion would be that some speakers of the Greek-to-be language, if not all, ought to have been there at the beginning of MH. Unfortunately this hypothesis rests on what I called the traditional view of the distribution of the Greek dialects. In recent years it has become clear that, in spite of the parallels mentioned, which associate the presence of given

15 On purely linguistic grounds we could probably postulate an original unity of Attic and Ionic, but I doubt that we would be able to establish the direction of the migration which caused the separation.
dialects in certain areas with extensive migrations or colonizations, nothing obliges us to assume that the same happened in the case of the pre-Doric dialects. In other words, it is conceivable that the Indo-European speakers arrived in Greece as a group or in scattered tribes which moved very closely to each other, and that the dialect distinctions arose in Greece itself at any period before that of our first evidence. If this is so, the dialect distinctions of Greece do not allow us to make any historical inference about the first part of the second millennium B.C.

4.3. The first two approaches, as we have seen, are not very fruitful. We must now examine a rather different way of considering the problem, which is based on a closer analysis of Greek as we know it. Let us remember first that part of our purpose in examining the linguistic evidence provided by non-Greek documents of the first and second millennia was to see whether we could establish what language or languages existed in Greece at an earlier time and to try to reconstruct through a study of its or their affinities some part of the history of the original speakers. The written evidence we have proved unsatisfactory, but it could be argued that better results could be obtained through a close examination of Greek itself and of the influence of other languages on it.

Suppose, to invent a paradoxical example, that Greek was shown to have been influenced by Finnish (or rather by an ancestor of Finnish) as closely as English has been influenced by Latin; this would automatically call for some explanation. Since the influence is unlikely to have occurred in the historical period (we would know the circumstances) it ought to be prehistoric. In a period in which writing is not a normal part of life, linguistic influence, if it exists at all, implies close contacts and perhaps a certain amount of bilingualism. Where did the contacts occur, how could we bring together the predecessors of the Greeks and the predecessors of the Finns? The example is paradoxical (to my knowledge Greek shows no trace of Finnish influence), but it may make clear what sort of influences and contacts we are looking for, and it may also show how, if these were identified, they could lead to some sort of historical conclusion.

4.3.1. The first question concerns the way in which we ought to proceed. In obvious cases, such as that of the Latin influence on English, we know Latin and this simplifies the problem. Assuming we did not know Latin, would we still recognize the words which English borrowed from Latin as in some way “non-English”? The question becomes more general and concerns the way in which we identify loan-words, or at any rate foreign elements in a language. Morphological elements are both easier and more difficult to recognize but are less common. There are a number of criteria which can be used to recognize loan words: I shall list a few here.

First, loan words or forms may patently break the phonological or morphological rules of the language. This is the case of some pronunciations of a word like garage in British English [ˈɡɛərɪ], or of some names like Ανδρός which appear in late Greek under Biblical influence.

Secondly, loan words or forms do not have an obvious etymology in terms of the parent language, even in languages which are transparent enough to allow an etymology of most lexical items. In English, a word like “zero” cannot be explained in terms of Proto-Germanic.

Under this heading we may distinguish a number of different cases.

(a) The language of origin may be known and the word in question attested in it: “zero” was taken from French into English and from Arabic into French.

(b) The language of origin may not be identifiable, but the word belongs to a group of words or forms which (1) cannot be etymologized, but (2) share some semantic or grammatical features. Even if we did not know Italian, we could see, for instance, that a large part of the English musical terminology is both non-etymologizable in Old English terms and shares some phonological and morphological features such as the frequent vocalic endings (piano, lento, etc.), -issimo suffix (pianissimo, lentoissimo), etc. A combination of these features with the fact that all the words in question are concerned with music makes it likely that we are dealing with an instance of foreign influence on English and that all words come from the same language.

It is also clear that the absence of an entirely convincing etymology is not in itself sufficient to guarantee that a word is of foreign origin (the problem may be different for morphological elements); it simply spurred us to look round and see if there is any other reason (such as those mentioned under (a) and (b) above) to assume that the word is foreign.

Thirdly, we may find that some words or forms (which we shall want to call loan words or forms) can intuitively be compared with words or forms of the parent language (i.e. can intuitively be etymologized in terms of words or forms attributed to, or reconstructed for, the parent language), but do not show the normal phonetic development. The English dentist can be intuitively compared with the Indo-

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14 The traditional view is expressed in a number of articles by Kretschmer (e.g. 1909). For modern introductions to the problem (with the earlier literature) see Chadwick 1975; Schmitz 1977: 124-53. Cf. also Palmer 1980: 57-92.

17 Notice, however, that we are able to observe this fact only if we have at our disposal spoken rather than written English—a condition which can hardly be satisfied for ancient Greek.
European word for ‘tooth’, *denti-, but we expect an Indo-European *d to be continued as τ in English, and an Indo-European *t to be continued as θ (as in tooth < *denti*). We happen to know that dentist is of French and, ultimately, Latin origin. If we did not know it, we would be interested in collecting other words (decimal vs. ten, duodecimal vs. two, paternal vs. father, fraternal vs. brother, etc.) where the same phenomenon occurs: on the one hand there is a similarity, intuitively established, between the English words and the Indo-European equivalents (*deēp* ‘10’, *dudh* ‘2’, *pētīt* ‘father’, *bhrīter* ‘brother’, etc.); on the other hand there are numerous ‘irregular’ sound treatments, which may even lead us to establish a new set of ‘regular’ rules. In other words, though we decided earlier that Indo-European *d* and *t* were continued by English *i* and *θ* respectively, we now find a series of words where in similar environments they are continued by English *d* and *t*. It must be stressed that the observation is important only if it applies to a series of words. If our first example (*dentist*) were unique, it could not take us very far, because we should never know whether the similarity in meaning and form of *dentist* and IE *denti-* is accidental or not. On the other hand, if a series of words is available, we must seriously ask whether English shows the influence of another Indo-European language. The nature of the words in question can also tell us something about the character of this influence. If the words belong to a literate society and are predominantly learned words, we may think of literary and cultural influence and we do not need to postulate contacts of peoples (at this stage at least)—though obviously a certain amount of bilingualism is the necessary precondition for any instance of large scale borrowing. In the case of words which indicate plants, animals, etc. or are, at any rate, tied to a particular geographic or climatic condition, we may think of large scale borrowing from a substratum language, the language originally spoken in the area.

Finally, a few words must be said about personal and geographic names, since they occupy a very special position. It is commonplace to notice that place-names in particular often remain tied to given places, even though the language of the local inhabitants may change. The United States has a number of towns with Indian names, though no American Indian language is spoken there. Similarly, I doubt that much Celtic is spoken in Dover in England, though the town has a Celtic name. The general tendency is to assume that place-names preserve traces of the language of the first inhabitants of a given place. Indeed, the Celtic place-names scattered through England document a Celtic occupation of the country for which we have other evidence too. Yet, a warning is necessary—and has often been given—against the danger of forgetting that place-names can also be carried round or acquired by people who do not speak the language to which the name belongs. The existence of

Philadelphia in the United States does not document a large colonization of the country by Greeks and the Cam- element present in the name of Cambridge, Mass. does not tell us anything about the presence of Celts in Massachusetts. More seriously, we seem to have evidence in Brazil for the spreading of place-names which originally belonged to the local Tupi language through the activities of a number of Portuguese speakers who, in their hunt for gold, gave Tupi names to forests, mountains, valleys and rivers, so that we have Tupi names in regions where there never were any Tupi speakers.

4.3.2. This was a long digression, but we now come back to Greek. We can look for possible ‘non-Greek’ words in Greek following the criteria indicated above. In early Greek we have no clear-cut evidence for words of the first category. Most words seem to be well integrated in the phonological and morphological system of the language—with the possible exception of Linear B names and lexical items, for which, however, the spelling is such that we cannot venture any hypothesis about the pronunciation.

The second and third categories are, of course, relevant. A cursory examination of 578 words listed as lemmata in ten sequences of ten pages each taken from Chantraine’s Dictionnaire Etymollogique de la Langue Grecque (1968) shows that ca. 52% of the words have an *etymology obscure*. Of the remaining 47.8% almost one sixth (i.e. ca. 8% of the total) consists of clear or relatively clear borrowings for which the language of origin can often be specified. This leaves us with less than 40% of words which have an Indo-European etymology. Admittedly, the figures are biased because of the particular arrangement of Chantraine’s dictionary where e.g. all derivatives and cognates of μυγνάρες are listed under a single heading so that in my reckoning they count as one word only, just as the obscure μυγνάρια, which is a gloss with no known derivatives or cognates within Greek. Even so the figures are remarkable.

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15 Cf. e.g. Chadwick 1969: 84-95. Patterns of preservation and replacement may vary from place to place and from period to period. Hainsworth (1972) points out that in England big cities and rivers tend to preserve Celtic names while minor villages do not: on the other hand Finkbeiner (1961) argues that in Turkey the few place-names of Turkish origin belong to large urban centres only—though in a sense the Turks are the newcomers on the Anatolian scene. Finally Robert and Robert (1977: 18) observe a trop of Asatanzia that ‘La présence de toponymes anciens est un fait très important ... Il s’applique ... ainsi bien à la ville importante et sans éclipses ... qu’à l’arrière des villages’

16 Cf. e.g. Chaves de Melo 1975: 41.

17 My calculations are based on the words listed in Chantraine 1968: 100-110 (θεῖο - -θρόο, 200-210 (θέα - -θήρ), 300-311 (p. 507 is blank) (δάκο - -δάκιο), 400-410 (δαυ - -δαν), 500-510 (σγατύλας - σκατύλας), 600-612 (pp. 607-8 are blank) (σκοτιας - δακτιλιας), 700-710 (μπρου - μπρω), 800-810 (δαύ - -δαίω), 900-910 (μπρου - -πρω), 1000-110 (σγατύλας - σκατύλας).
We have seen that c. 8% of our sample is composed of words treated as possible borrowings from a number of languages. According to my earlier classification most of these words would belong to the (a) section of my second category. Also, most of the words in question were introduced into Greek at a late stage and do not interest us here. Yet, it may be worthwhile to remember that the Mycenaean texts have shown that Greek borrowed a number of Semitic words before our first texts, i.e. that there were close contacts with Semitic peoples long before the Dark Ages. Most of the words borrowed refer to herbs or plants, perfumes, textiles and precious objects, which may have been introduced through trade relations. It is possible that at some stage a few abstract words were also borrowed but, even so, though some bilingualism was obviously called for, it is not easy to argue that this bilingualism was due to the presence of a Semitic substratum or superstratum in Greece.\footnote{Anatolian borrowings are also attested in Greek, as recent studies have shown. In most cases the date of introduction of the words is not clear, and here I shall only mention that there may be recognizable Anatolian loan words in the Linear B texts: ἐλασίς 'ivory' and σκόπος 'kyanos' are the main examples.\footnote{Here too, neither these words, nor the supposed later borrowings clarify the nature of the contacts between Greeks and Anatolians. In the first millennium the Greeks came frequently across the Anatolians in Asia Minor, but for the early period all that we have to go by are A few words have been ignored even though they occur as lemmata in the pages listed above; they are either proper names or glosses whose existence and meaning are entirely uncertain. Of the remaining 570 words considered, 301 have no known etymology (I have included in this list among those one unassimilated words, Ladinian, etc.), 229 are built on roots which seem to have equivalents in other Indo-European languages, 48 are treated as borrowings from a specified language or language group. Chaniotini is extremely cautious in committing himself to an Indo-European etymology; sometimes, albeit rarely, I felt it was right to ignore some of his cautionary remarks. Within the sample the proportion between my three categories varies; hence the need to choose at random a number of ten-page sequences from the dictionary. The maximum contrast is between the figures of pp. 409-410 (22 'obscure' words, 28 'Indo-European' words, 3 'borrowings') and those of pp. 1000-1010 (36 'obscure' words, 6 'Indo-European' words, 5 'borrowings').}

We should not forget, however, that Gordon has argued that the Cretan scripts were used for a Semitic language (e.g. Gordon 1966) and that Astour (1967) has tried to point out a number of connections between Greek and Semitic (especially in the mythology and the proper names). A list of acknowledged borrowings from Semitic is available in Manoë 1967 (with the earlier literature). More recent bibliography and some new suggestions in Sienerfey 1974: 145-47.\footnote{Cf. especially Neumann 1961; Hübner 1961; for further suggestions see Sienerfey 1974, 152-55. Loarce (1965) pointed out that Hittite láğš must have been the source of Greek ἐλασίσ, Mycenaean ἐ-λάς; but see also Laroche (1968: 777) who wonders whether ἐλασίς and lágš derived independently from the same source (Syrian?). Hittite láagš means 'copper', but if precipitated by the NA there is no certainty. The exact referent of Mycenaean ἐ-λάς is not certain (see Haller 1969) but it seems very likely that ἐ-λάς and láagš have the same origin (cf. Laroche 1966 (2): 180; Hasewitz ten Kate 1973: 143) is perhaps too cautious.}

4.3.3. The words of unknown etymology which cannot be treated as borrowings from any known linguistic group are very numerous. Do they belong to any special semantic or grammatical sub-group? The answer cannot be firm. It is certainly true that a large number of these words refer to concrete objects, fruits, plants, animals, birds, fish, etc. It is also true that some of the words show consonantal and vocalic alternations which are not normally found in words of clear Indo-European origin: cf., for instance, γάιος/γάιθος 'fishing basket', and ἔξωθος/ἐξώθος 'the name of a plant, a blue pimpernel' (Arist. and Theophr.). On the other hand it has also been said (Furnée 1972: 104-05) that in most cases we have no perfect doubles but only words which are similar (and may be related), but show different suffixes or different vocalism, so that the alternations cannot be taken as a certain indication of an 'abnormal' phonological structure.

Scholars are divided in their approach to the words of unknown etymology. For a long time linguists have argued that those words which had meanings connected with particular climatic and geographic conditions and no known etymology were survivals of a substratum language, which was spoken in Greece before the arrival of the Indo-Europeans. It has also been suggested that some of these words—and in particular those which emerge in an intuitively similar form in Latin (vinum, mense, flus, rosa, litium, etc.)—represent a wider 'Mediterranean' substratum, i.e. a language which was originally spoken both in Greece and in other Mediterranean countries (cf. Hester 1968: 223-24; Katić 1976: 1, 56 with references).

Here it is not possible to go into the details, but two points may be made. First, it is entirely probable that some of the words of unknown etymology represent survivals of one or more pre-Greek languages, and that the words which indicate typical Mediterranean plants, animals, etc. belonged to a language or to languages which were more 'local' than Indo-European was. Yet this can only be stated on general grounds without a specific demonstration. Secondly, it is also true that if this were so, and if we were able, for instance, to reconstruct some features of the language or languages in question, no historical conclusions could be drawn from this. We would have no way of dating or localizing the language(s). Any such language, if it existed, could have left its traces on Greek, both if it was its immediate predecessor and if it was the predecessor once or twice or thrice removed. Moreover, the language could have been spoken in one part of Greece only or in all of it: nothing would allow us to decide. In other words, both if we assume and if we do not assume the existence of a Mediterranean substratum in Greece, this tells us nothing about the few words of Linear B date quoted above (for other possible contacts with Anatolia, see below 4.4.4.).}
what was going on in the area in the Early Bronze Age. Nor are we really in a position to decide if we are speaking of one or more languages.23

4.3.4. Other linguists have isolated a group of words of unknown etymology, and tried to show that the words can in fact be provided with an Indo-European etymology, if we assume that they show a phonetic treatment of the sounds of the parent language which differs from that attested in the normal Greek words. The phenomenon would be parallel to that which I have illustrated for English with the example of tooth and dentist. The starting point is provided by some Greek words of certain Indo-European etymology, besides which we also have apparent doublets with similar or identical meaning but a different phonetic aspect: cf. ἐκλίαω 'anoint' and ἱππος 'a fat' with a θ / π alternation; ἄθροα 'crest of a hill, rim of a cup' and ἄθυρα 'nave' with a β / θ alternation; and θυρος and θυρος 'tombs'. From a few examples of this type it is possible to extract some general rules of derivation (e.g. IE *tbhθ > bθ rather than pb in Greek; IE *p > p and not p as in Greek, IE *e > a and not e, etc.); having accepted the new rules, we can then hunt for Indo-European etymologies even for words for which no doublet is available. Thus it can be suggested that the word θρήνος, 'wooden image of a god', which Chantraine (1968; s.v.) labels as 'termes méditerranéens sans étymologie', can be compared with the Indo-European root *θrēν- 'to cut', which yields German 'brech- and German 'brecht 'plank, table'. In this way we multiply the number of Indo-European words in Greek, and we argue that the new phonetic rules apply to an Indo-European language which preceded Greek in Greece. The names given to this hypothesized language differ; the most commonly used is "Pelagian" and from now on I shall use this denomination in a purely conventional manner.24

Before entering into the merits of the Pelagian controversy, we may mention why it is relevant here. The point is often made that if we were able to prove that in addition to Greek we have evidence in Greece for another Indo-European language, this would be likely to be the language spoken in the area before the arrival of the future Greeks. In other words, our chronology ought to allow for the arrival of two groups of Indo-European speakers, who had developed two remarkably different forms of speech. If so, the existence or nonexistence of the Pelagians would have historical importance. However, it can be argued that even if Pelagian existed, all that we

23 For a general introduction to these problems and their bibliography see Hester 1968. Very good surveys can also be found in Quenelle 1972; Sakkoulas 1977; Kardel 1976.

24 For Pelagian see, in addition to the surveys quoted in n. 23, Hester 1965. Cf. also Birnbaum 1974. I have not mentioned in the text the diverging, though not too dissimilar, theories of e.g. Merlengo, Haas, van Wisselink, but details will be found (together with the relevant bibliography) in the bibliography quoted above. My examples are all taken from Georgiev 1969.

would really know about it would be that it had great influence on Greek. Influence in this context implies contacts and bilingualism. But, and this is the important point, nothing would imply that the contacts happened in Greece. It would be conceivable, for instance, that the Greeks-to-be and the 'Pelagians' met during the course of their migrations in 'Pelagian' territory and that this territory was not in Greece. My conclusion is that even if the existence of the Pelagian language was established on linguistic grounds this would not be in itself sufficient proof of the existence of an Indo-European substratum in Greece. Yet, in my opinion the existence of a Pelagian language is also doubtful. The doublets quoted above are at first sight impressive, but either their semantic identity is not certain (cf. ἄγνωστος and ἀγνώστος) or they can be explained in Greek terms, postulating e.g. different Indo-European 'suffixes' or 'enlargements' (cf. ἀλφα and ἰαστός), or they are problematic both for the Hellenist and the 'Pelagianist' (how do we explain ἦσος near ἥσαν). The supporting etymologies are also doubtful and sometimes contradictory (from a root *khrēn- we should expect *khrēnus and not θρήνος, according to the 'Pelagian' sound laws). Obviously it is impossible to demonstrate that Pelagian did not exist, but it is possible to say that its existence is not demonstrated.

4.4 Where do we go from there? A recurrent problem, which I have already mentioned more than once, concerns localisation and dating. Even if we could establish that the Greeks were influenced by a 'Mediterranean' or a 'Pelagian' language, how could we date the time at which these languages were spoken or identify the areas where they were spoken? I shall now consider some methods which would allow us to do just that for 'Mediterranean' or 'Pelagian', if they existed, or for any other pre-Greek language.

First, we could identify the language in question with one of the non-Greek languages attested in the Greek territory: Eteocretan, Etocyprian, the language of Linear A, the language of the Cretan Hieroglyphs, Cypro-Minoan 1 and 2, the language of the Lemnos stele.

Secondly, we could collect the references made by the Greeks to the peoples they thought had existed in Greece before them or together with them, examine their names, and decide to what linguistic group they belonged on the strength of their etymologies—and, of course, of the assumption that the Greeks used the indigenous names for these peoples.

Thirdly, it should be possible to analyse the known place-names of Greek for which a Greek origin seems excluded and to connect them with one of the reconstructed or attested pre-Greek languages.

The second and third method suffer of an obvious disadvantage. They are based on the etymology of ethnics or place-names. Etymology in its turn is based on com-
parison and comparison must be tested on semantic and phonetic grounds: two words are related when the meanings are similar and the sounds correspond according to given rules. In the case of an ethnic like Πελαγιαν we do not know what meaning to look for so that the semantic text is not available. It is not necessarily useful to look for a meaning which fits the cultural or anthropological features of a people, since the name could have been given long before those features were developed or could have been inherited from previous populations which did not share those characters. Consequently, I do not feel inclined to pursue my second method, though I am aware that it has been extensively used. Etymologizing place-names is also dangerous, though we may find meanings which correspond to some unchangeable geographic characteristics ('steep', 'rocky', 'salty', etc.). This does not reckon with the movement of place-names which can be transported from one place to another and may be re-used for emotive reasons, even when they are no longer understood.

As for the first method, it can only be applied to languages written in a script which can be read (Cypro-Minoan and Hieroglyphic Cretan are excluded); even when this condition is satisfied, the method cannot be easily exploited when the evidence is minimal or incomprehensible (as for Eteocretan, Eteocyprian, the language of the island of Lemnos etc.).

4.4.1. In spite of these objections, suggestions have been made aimed at drawing firm historical conclusions; in what follows I shall discuss three different views which have been held in the recent past or are being held at the moment.

In all three theories place-names play a considerable part. Obviously, typically Greek names of the Νέας Πόλεως or Λευκέρας type are not very informative; the main area of interest is formed by the 'unetymologizable' place-names, which traditionally have been taken as Pre-Greek: on the one hand, forms like Ἀθῆναι and Θῆβαι, on the other hand forms distinguished by typical suffixes such as -αις, -οςος or -οςαξ, -οςος εξ -οςος, -οςος ας -οςος, -οςος αις, etc. We may repeat here that there is no reason to suppose that all non-Greek names have the same linguistic background; nothing tells us that Αθηναι and Κορωνοι originally belonged to the same language. However, Hester (1957) has argued that all pre-Greek names formed with the suffixes -ος, -οις, -οις, -οις, -οις (possibly -οις), and perhaps -ας and -ας, show a marked preference for open syllables in the root, beyond a level which can be attributed to chance. This may

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28 Subtelnyk (1977: 101) derives the names of the Pelasgians from that of a god Pelasgos and makes a case for a Pelasgian etymology of the god's name, which would derive from *mēl-o-γáμος 'name of branches and flowers'. I am not convinced that there is sufficient evidence to prove that the name of the Pelasgians derives from that of Pelasgos and Pelagon has the meaning it is supposed to have.

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29 The examples come from Georgiev 1966, but cf. also e.g. Georgiev 1961 and 1973.
plants, fruits, birds and very concrete objects (cf. ἰἀσώνιζος 'chick-pea', διανόθις 'wild fig', πλάτονος 'brick', etc.), this seemed to speak in favour of a suffix typical of the Argoaic area (but cf. Szemerényi 1974: 132). At the same time the observation that a number of the place-names under discussion was attested not only in Greek territory but also in Asia Minor favoured the view that they belonged to a wide substratum which existed not only in parts of Greece, but also in parts of Asia Minor. The attempt to define this substratum in archaeological terms and to anchor it in time was made by Blegen and Haley in two articles published in 1928. Here it is enough to remember that Haley (1928) tried to identify and to put on a map all place-names of pre-Greek character which were attested both in Greece and in Asia Minor. He dedicated a first list to those names which occurred in Greece and in Asia Minor in the same form (Θέμβος, Πέτραων, Κάρυσσος, Λάδισα, Λάδινα). A second list dealt with names which were not attested in both areas but were formed with suffixes typical of both areas: -άκης, -ος/τος, -ός/νιου, -άς, -άς, -ός, -ής, -ής (Αλίκωνιζος, Αμίλων, Αργαλλίκης, Αίμασα, Εύακης, Δήτξα). The third list included names built on roots which appeared in both areas. When the names were put on the map of Greece it emerged that they covered a well defined area, which left blank a large part of western Greece and northern Greece, but included East and central Crete, the Peloponnesae (except for Arcadia and Elis, which had only a few examples), and Achaia, Attica, Boeotia, Phocis and Thessaly. Blegen (1928(1)) compared the distribution of the place-names with that of early Greek sites. He excluded the possibility that the onomastic layer went back to the Neolithic period, because the names were attested in the Cyclades, where there did not seem to be any archaeological evidence for the Neolithic period. He also excluded all the Middle Bronze Age because there were wide differences in culture between Crete and the Greek mainland, while the names covered both areas. His conclusion was that the origin of these place-names was to be found in the Early Bronze period—a map of the excavated Early Bronze sites appeared to cover an area similar to that of the map of names and locations. Blegen also assumed (1928 (1): 140) that "if we can identify the pre-Greek layer, it naturally follows that we can recognise as Hellene the succeeding layer and we can thus determine exactly where in the archaeological stratification the Hellenes first appear". He concluded that the Greeks arrived at the end of the Early Bronze Age.

27 Histories of the question can be found in most of the literature quoted in n. 23 and n. 24; cf. especially Kastrić 1976: 1, 40 and see also Lopez Eire 1967. For the earlier period the standard book is Krencker 1896. More recently see especially Schachermeyr 1954: 1511; 1955: 239; 1967: 12; 1972; for the sub-formation see Quarternio Morosini 1984.

98 The same problem also arises when we consider the much larger list of Greek place-names compiled by Gindis (1967); cf. also Kastrić 1976: 1, 91; Birdwood 1974: 375. He aims at identifying roots (rather than suffixes) which can be compared with Anatolian roots in order to establish the existence of an Anatolian substratum in Greece. Here the missing link could be found if it were true that all roots considered are formally similar not merely to Anatolian roots but, more specifically, to Indo-European roots of Anatolian; yet, this does not seem to be the case. Moreover, one cannot forget that the roots are compared on purely formal grounds, since no meanings are available, and that the Anatolian material largely consists of names extracted from cuneiform sources where in most cases the spelling does not indicate distinctions (such as that between voiced and voiceless consonants) which are basic in Greek.
the lists produced by Haley and Blegen are not coherent; they may all represent pre-Greek place-names, but nothing guarantees that they are pre-Greek place-names which belong to the same language and that their presence may be explained in the same way for all of them. If so, the conclusion may also be unacceptable. The problem would be different if Blegen and Haley had been concerned with the \(-\lambda \nu h\) and \(-\sigma o s\) place-names only, or at any rate with place-names linked by some formal factor, such as well defined suffixes or an obvious preference for some syllabic pattern, such as that defined by Hester for the \(-\nu t\), \(-\sigma r\), \(-\mu \nu\) and \(-\omega \nu\) names. My own view is that Blegen’s conclusions do not rest on any definitive proof.

4.4.4. More recently, attention has concentrated on the place-names formed with the \(-\nu t h\) and \(-\sigma o s\) suffixes. Two reasons have contributed to it. On the one hand the need to deal with a coherent set of names rather than with the mixed bag considered by Haley and Blegen, on the other hand the increased awareness of the link between Greek \(-\nu t h\) and Anatolian \(-n\)a, Greek \(-\sigma o s\) and Anatolian \(-\eta\)a. In their turn the two Anatolian suffixes have come in for closer analysis, and in a series of articles Laroche has argued that (a) in the second millennium \(-\eta\)a, \(-\eta\)a\(n\)a and \(-\alpha\)\(a\)\(n\)\(a\) place-names occur in the whole of the Hittite Empire, and (b) there is no reason to assume that these suffixes were borrowed from a non-Indo-European substratum since they are perfectly at home in the Indo-European languages of Anatolia. \(-\eta\)a may be taken as the neutron plural of the genetival suffix \(-\eta a\) or \(-\nu t a\), Luvian \(-\nu r\) (perhaps from IE *\(-\eta r\)p); \(-\eta\)\(a\)\(n\)a and \(-\nu t a\) are connected with IE *\(-n\)t (and the omnipresent \(-n\)t suffix of Hittite and Luvian) and with IE *\(-n\)\(e\)nt.

In his list of the relevant place-names, Laroche stressed that although the suffixes are at home in the Indo-European of Anatolia, they are added indifferently to Indo-European and non-Indo-European roots, and that it is not possible to distinguish between Hittite and Luvian place-names when these suffixes are involved. In one of his early articles (1961) he also concluded that if the \(-\eta \)\(a\)\(n\)a and \(-\nu \)\(t\)\(a\) place-names were to be connected with the \(-\nu t h\) and \(-\sigma o s\) forms of Greek, this would give some support to the suggestion that in Greek these were preceded by another Indo-European language. Yet, at no stage did Laroche state that the theory of a pre-Greek Indo-European substratum could be actually demonstrated, and most recently (1977: 217) his tone seems to be far more sceptical.19

Other scholars, but not Laroche, have gone a step further and argued that there was a pre-Greek Indo-European substratum in Greece and that this was a Luvian substratum. The suggestion is based on linguistic, archaeological and mythological data, and has been both violently defended and violently attacked. I shall be concerned only with the linguistic evidence; this calls for an analysis of the data collected by Palmer, who has gone more deeply than the others in the linguistic side of the problem.20 Palmer’s argument (or rather the linguistic part of it) is founded on (a) the study of the place-names and their suffixes, (b) the analysis of another Greek suffix which he takes to be of Luvian origin, and (c) the suggestion that there are clear Luvian elements in the Linear A texts, if we read them attributing to the signs the Linear B values. I shall discuss these points one after the other before I turn to some more general considerations about Luvian and Hittite. First, the place-names. Palmer has pointed out that the connections between Greek and Anatolian place-names go beyond the identity of suffixes and extend to cover one place-name which is fully analysable in Anatolian terms. The Greek \(\Pi \rho \alpha \rho \omega \sigma \tau \sigma \rho \varsigma \sigma\) matches the Anatolian \(\Pi \rho \alpha \rho n \varsigma\), which is built with the \(-\eta \)\(a\)\(n\) suffix on the root \(\rho \alpha n\) of the Hittite and Luvian word for “house”. Palmer argues that if \(\Pi \rho \alpha n \varsigma\) ‘makes sense’ both in Hittite and in Luvian, and if it can be shown, as Laroche has demonstrated, that \(\rho \alpha n\) is a typically Anatolian root, Greek \(\Pi \rho \alpha \rho \omega \sigma \tau \sigma \rho \varsigma \sigma\) must be taken from Anatolian. What is true of \(\Pi \rho \alpha \rho \omega \sigma \tau \sigma \rho \varsigma \sigma\) must also be true of the other \(-\nu t h\) names. A priori the Anatolian and Greek connections could be explained either by assuming a migration from Anatolia to Greece, or by assuming a migration from Greece to Anatolia, or by assuming a migration from a third place moved both to Greece and to Anatolia. The existence of \(\Pi \rho \alpha \rho \omega \sigma \tau \sigma \rho \varsigma \sigma\) speaks for the first hypothesis.

Secondly, Palmer stresses the importance of the names \(\Delta \nu \epsilon \upsilon \nu \nu \) and \(\Delta \kappa \lambda \rho \rho \varsigma\). As others have done before him, he connects \(\Delta \nu \epsilon \upsilon \nu \nu \) with the name of \(\Delta \kappa \lambda \rho \rho \varsigma\) (now attested in the Mycenaean \(\delta \iota \kappa \lambda \rho \rho \varsigma\) and \(-\)\(\nu \)\(a\)\(n\) or \(-\)\(\nu \)\(t\)\(a\) which, in this form, is a typical Luvian suffix used to form ethnics. In Luvian \(-\)\(\nu \)\(a\)\(n\)\(i\) may be "simplified" to \(-\)\(a\)\(n\)\(i\) and it is this form which Palmer compares with the \(-\)\(\nu \)\(a\)\(n\)\(i\) of \(\Delta \nu \epsilon \upsilon \nu \nu \) and the \(-\)\(\nu \)\(a\)\(n\)\(i\) of \(\Delta \kappa \lambda \rho \rho \varsigma\) (formed on \(\Delta \kappa \lambda \rho \rho \varsigma\)). The point is meant to show that these suffixes of Luvian origin were productive in Greek territory, i.e. that they did not only exist in words which could be borrowed as a whole or imported without understanding their meaning.

Thirdly, Linear A Palmer suggests that a recurrent series of signs in what has come to be called the libation formula (which now occurs in a number of different texts in Crete), must be read as \(\alpha \alpha -\kappa -\epsilon -\nu -\sigma -\tau -\nu -\mu -\alpha\), \(\alpha \alpha -\kappa -\epsilon -\nu -\sigma -\tau -\nu -\mu -\alpha\), \(\alpha -\kappa -\epsilon -\nu -\lambda -\rho -\mu -\alpha\), \(\alpha -\kappa -\epsilon -\nu -\lambda -\rho -\mu -\alpha\) and interpreted as ‘Ma-donna’, ‘to my (male) lady (asassera)’. The Hittite word for

‘lady’ is iththi- and in correspondence with Hittite i we often find Luwian a. In one of the ‘libation formulae’ Palmer also finds an Anatolian verb *tanos*, a third person singular ending -*t* (cf. Luwian -*t*), a negative as (cf. Hieroglyphic Luwian *ne*), a verbal form *de*- or *da-woq ‘take’ (cf. *de-* ‘take’ in Hittite and Luwian), a noun *du-wa-na* (cf. the verb *tawo- ‘put’ in Cuneiform and Hieroglyphic Luwian) and a pronoun element *i-je* (cf. Hieroglyphic Luwian *i- ‘this’). He concludes that the connections with Luwian are too many to be due to chance. Linear A was used for a language similar to Luwian.

I shall neglect here the archaeological, historical and mythological evidence adduced and I shall just mention Palmer’s conclusions. At some stage at the end of the Early Bronze Age some Luwian tribes migrated from Anatolia and settled in Greece. From the mainland they moved to Crete. Finally at a later stage (perhaps at the break between MH and LH) new Indo-European tribes arrived into Greece and replaced Luwian with their language, i.e. the language which at a later stage we call Greek.

4.4.4.1. So much for the theory and the linguistic evidence. I cannot discuss the whole problem, but something may be said about the linguistic data and their background.

It is now clear that in the second millennium Anatolia knew three closely related Indo-European languages: Palaic, Hittite and Luwian. They are first attested indirectly in names quoted in the Assyrian texts of Kültepe, probably to be dated to the nineteenth century BC. Yet it is extremely uncertain whether these texts contain names which can be defined as Luwian, as contrasted with Hittite. The most likely conclusion is that the texts contain names which match later Hittite and Luwian names, but this does not prove that at that time the distinction was meaningful. There are lexical elements which seem to be more definitely Hittite. In the case of Luwian, Larroche (1966 i: 315) concludes quite correctly that ‘waasin type launiim ne correspond à un morphème spécifiquement louvien’; he had previously explained (ibid.): ‘le mot ‘louvié’ n’est plus aujourd’hui l’étiquette commode d’il y a trente ans, recouvrant la somme de nos ignorances.’ In other words, the Kültepe texts tell us little about Luwian. The first Hittite texts go back to the eighteenth century at the earliest, the Luwian texts belong to the second part of the second millennium. In that period we think of speakers of Luwian as settled in the southern part of Anatolia. In the first millennium we have a dialect closely related to Luwian written in the so-called Hieroglyphic script in Syria, Cappadocia and Cilicia; the bulk of the texts dates from the tenth to the eighth century. A third language, Lycian, written alphabetically, also belongs to the Luwian group, but the main documents belong to the middle or second half of the first millennium BC and continue until the fourth century or later. Luwian is a linguistic concept and we ought to stress that there is no clear evidence for Luwian as such before the second part of the second millennium. Melkaart (1971 i: 468; 1971 ii: 703) wants to find Luwians at Kültepe, but we have seen that the evidence is not clear. He also speaks of a Hieroglyphic Luwian seal from level VI (ca. 2000-1900 BC) at Beycesultan, but this is an old idea of Bossert which must be rejected. It is not clear that the seal bears the signs of any script, or that the script, if it is there, is Hieroglyphic, or that Hieroglyphic at that time was used for Luwian. Similarly, Melkaart’s statement (1971 i: 468) that a Luwian is mentioned in an obelisk set up at Byblos not later than the twenty-first century and written in Egyptian hieroglyphs, cannot be accepted without reservations. It is based on the fact that the name Kukun is similar to a much later Luwian name, Kukunni—but there is nothing specifically Luwian in either Kukun or Kukunni (the presence of the name David in a document would not prove the presence of Englishmen in the area where the document was found).

The conclusions is that we have no data which bear on Luwian in the Early Bronze Age. Languages are not created from nothing and, if Luwian existed in the second part of the second millennium, it must also have had an earlier phase, but we have no direct evidence for it—which means that we cannot localize it or define it linguistically. On general grounds the presumption is that early Luwian was closer to Hittite than the later form of the language.32

4.4.4.2. I now come back to the arguments for the presence of Luwians in Greece. From a purely linguistic point of view it seems to me that none of them, with the possible exception of the Linear A interpretations mentioned above, speaks for the presence of Luwians rather than of Anatolians in general in Greece. If we want to segment an *-wan*-/*-en* suffix in *Δαλιάτις* and *Αλιάλιος* and compare it with Luwian *-wan(y)/*-en(y)/*en(y)*, we ought to remember that the Luwian form belongs to the end of the second millennium and not to the beginning of it, i.e. to the date of the supposed Luwian invasion of Greece. At that date presumably the suffix was closer to *-wan*—the Common Anatolian form (cf. Larroche 1960: 170-71) than to *-wan(y)*. It would be possible to derive *-wan*-/*-en* from a ‘helleneized’ form of *-wan*-/*en*- but, if so, the suffix does not point to Luwian but to Anatolian in general. Indeed, those who want
to make use of the same Anatolian suffix to explain both *-ωνις/-ωνις and the *-ωνας place-names (Δαριάωνας, etc.) could probably argue that the original *-ωνας was differently treated in different parts of Greece or different dialects; it would be easier to do so than to argue that *-ωνας is Hittite and *-ωνας/-ωνις Luwian.

If this is so and if the rest of the evidence points to Anatolian, rather than to Luwian, is it Linear A that induces us to think of Luwian? Even this is doubtful. Palmer has analysed only one text, a libation table from Knossos (I 6 Brisc; see now Godart and Olivier 1976- vol. 4: KN Za10), which he reads (1968: 348) ta-na-se-ri ja-ta-se-ri-ma
na da-wn-ri j frustration j-jafr

[This altar X] sets up for My Lady
do not take the offerings from this (here) ...

If we assume for a moment that this interpretation is correct (cf. for a slightly different version Palmer 1965: 334-39), what is Hittite and what is Luwian? The -sara-suffix is Hittite; Cuneiform Luwian has *sara-ji and Hieroglyphic Luwian -sara*-ja-rri with the usual -ri- thematization. If this postdates the Early Bronze period, we could say that at an early stage -sara- was both Luwian and Hittite.

Palmer compares -sara- or ja-sara with Hittite ilhes-‘lord’ and argues that the Luwian form would have been *ilhes-. However, a) neither *ilhes- nor ilhes are attested in Luwian, b) not all Hittite i’s correspond to Luwian a, and c) I do not see what favours this interpretation over that of Carruba’s (1968: 934), who compares the word with the attested kalayas-‘Queen’, which is both Hittite and Luwian (except for the final vowel). Some phonetic assumptions are necessary for both interpretations.

We have evidence in Hittite for an anodic possessive pronoun -mil; we have no such evidence for Hieroglyphic Luwian, while it is possible, but unlikely, that Cuneiform Luwian had an anodic possessive pronoun of third person. It is conceivable that these anodic forms were common Anatolian.

The reading te-ri-ri is very disputed. Raison and Pope now read ta-na-se-ri. Luwian has indeed a -ti ending, while Hittite has -zi, but -zi derives from -ti, so that if -ti were attested in Crete it could represent an Anatolian archaisms preserved in a lateral area; it does not speak for Luwian.

The identification of a monosyllabic na element at the beginning of the second line is disputed (cf. Pope 1961; Palmer 1968: 348). If it is acceptable, we should notice that na as a factual negative is presupposed both by Hittite natis and Cuneiform Luwian names; na ‘not’ is attested in Hieroglyphic Luwian, but it seems incongruous to use linguistic material of the first millennium when we have Luwian data for the second millennium in the cuneiform texts. More important, we now know (Hawkins 1975) that in Cuneiform Luwian the prohibitive negative is ni and in Hieroglyphic Luwian ni, and not na as used to be thought.

ta- ‘take’ is common to all Indo-European languages of Anatolia. ta-mas ‘put’ belongs to Cuneiform Luwian, Hieroglyphic Luwian and Lycian. It is the most specifically Luwian form identified by Palmer, but Hart (1969: 167) has pointed out that the word may well have been *[j]-da-wa-pa, which would make the identification impossible.

jafr can no longer be taken as a Luwian demonstrative on the basis of Hieroglyphic Luwian, since this has been shown to have a ja- demonstrative like Cuneiform Luwian (i-th was an earlier interpretation).

The conclusion is that, even if we accept most of Palmer’s interpretations of the words in the text, this has no specifically Luwian forms except for [j]-da-wa-pa, and the reading of this sequence is doubtful. The bulk of the evidence, if accepted, would point towards Anatolian rather than towards Luwian.

4.4.4.3. How acceptable is the evidence as a whole? Let us say from the start that some of the points made about place-names are impressive. I am convinced that Laroche is right in attributing to Anatolian *-ρας and -νδα place-names and is also probably right (I am less certain about this) in assuming that the suffixes are of Indo-European origin. The complications arise à propos of the problem which Laroche has never discussed in detail: the identification of these names with the Greek names in -σως and -σας. In a few cases (Parma/ρας) the identification is difficult to resist but we should remember Laroche’s dictum (1977: 217): “la ressemblance du hittite Parazana et du grec Paros nous suffit pas pour affirmer la présence de Lourdes à Delphes”. Also, we must realise that, as has often been pointed out, the Greek category of -σας names is not universal. There are names which end in accented -σας (Ἰλαράος/Λάραος) and names which end in unaccented -σας (Κρήσιμος/Κρήσιμος), there are names which end in -σως and names which end in -σας (though it is not always easy to distinguish the two categories), there are -σας names which have -ταος equivalents in Attic (Ὑπατος) and -σας names which do not (Δαυδος). This is well known, but the result is that we gain the impression (confirmed by some of Hester’s findings on syllabic patterns) that we cannot compare the -σας names in toto with the -ταος names of Anatolia; the comparison is justified for a few names only. If so, there may be more than one explanation and we do not need to assume the existence of Anatolian settlements in Greece on the strength of the sheer immensity of the data. Moreover there is the perennial question of the -σως/ταος alternation. This points to an earlier form of the *-σας or *-ταος type; a simple *-ταος or *-ταως would be more
likely to yield -ος (?) or -υς in the whole of Greek. Yet, the Anatolian form, if Indo-European, must have been either -ια or -ιε; why should it have been borrowed with an affix?23 If we exclude all the -ος/-υς names from our list, the comparison with Anatolian becomes less and less impressive. Once more we ask whether the normal process of borrowing and diffusion would not be sufficient to explain what is left.

What about -φως? Here there is again a phonetic problem. Why would Greek have absorbed an Anatolian -φω-ιν-? in the form -φω-τε? Borrowings are notoriously unpredictable in their phonetic behaviour—all the more so if we know little about the phonologies of the giving and receiving languages. Yet, we could reasonably expect -φω- or -φυ- in Greek, especially if we have, as we seem to, an early Anatolian borrowing into Greek, the work Πλατανός, Δέλφιανας 'ivory', where, if Laroche (1965) is right, -φω- reflects an Anatolian -φι- extension, i.e. the same suffix which Laroche finds in the place-names.

Two words like Δέλφιας and Πλατανός do not provide much in the way of evidence for a suffix -φω-ιν- to be compared with Anatolian. Also, even if we allow the comparison to stand, as we have seen this leads us back to a Common Anatolian *-ιν-, which is at the origin of the Hittite and Luwian forms. But there is no way of showing that this suffix is both Anatolian and Indo-European. If so, its presence in Greek, even if accepted, does not necessarily point to Anatolian influence. We cannot exclude a common pre-Indo-European substratum.

I finally come again to Linear A. It has often been said—and it may be repeated here—that one single text does not offer much evidence. Let us also remember that the readings are unreliable for two reasons: a) it is far from certain that in all cases we can use Linear B values for Linear A signs, and b) the readings of this particular text are doubtful, since a number of signs are scarcely recognisable. It can be added that the first word, which Palmer reads ta-su-q-ta, begins with two signs which also appear in a word of the parallel dedication formula written on a silver pin from Platanos recently published (Alexiou and Brice 1976; Godart and Olivier 1976-, vol. 4: PL 27f). If transcribed according to the same criteria the Platanos word reads *ta-nu-ki-na, which does not sound like a recognisable verbal or nominal form of Anatolian. Here too the conclusion is 'non ligent'.

To sum up: from a purely linguistic point of view I can only say two things. First, I see no linguistic reason for formulating a 'Luwian' hypothesis, as contrasted with an 'Anatolian' hypothesis. Secondly, however formulated, the Anatolian hypothesis is not yet supported by sufficient evidence. It cannot be excluded, but cannot be demonstrated.

5. My general conclusion is that, although there is in Greek a great deal of linguistic material which is not Indo-European or at least is not directly descended from Indo-European through the normal sound changes, we are still incapable of identifying the sources of this material and, above all, we are not in a position to say when and how it was acquired. If so, linguistics has little to contribute to the historical problems of the Early Bronze Age. The answer to the question which appears in the title of this paper is almost entirely negative.

5.1. Yet, I do not want to conclude on an entirely negative note. If no old problem can be solved, new ones can be created. What follows may complicate the question even further.

In 1967 Ruijgh (1967: 238-62), after an analysis of Mycenaean word-formation, concluded that in addition to the normal suffixes which Greek had inherited from Indo-European, Mycenaean had a suffix which, in spite of its deceptive appearance, was not likely to be Indo-European. The suffix appears in the form εις-, εις (feminine), and perhaps εις-ιο; it is excluded that it can be compared with Greek forms like Σωκράτης, Ἀχιλλεύς etc., since in Mycenaean these would appear as (regular) εις-ιο (< *αιας) and ειςιος (< *αιος) forms. On the other hand, the suffix cannot be compared with Indo-European *-ης, which was used to form adjectives of material and was continued e.g. in Greek φυσικός, φυσικός. The meaning of Mycenaean εις-, εις is that of a possessive suffix and is used to form adjectives with a possessive meaning; often it is added to -α stems, but the final -α of the stem is deleted before the suffix. Ruijgh concluded that the suffix was likely to be pre-Greek. His suggestion did not meet with general approval, but some recently published evidence from Thebes and Tiryns seems to show that the suffix is indeed a feature of Mycenaean and behaves in such a way that it cannot be identified with any Greek suffix of Indo-European origin.24 This is not particularly exciting in itself, since it simply adds another element to our list of Greek forms without an Indo-European etymology. More exciting is the observation that perhaps we can localize the source of this suffix. Earlier on in this paper I have expressed some doubts about

23 For the different types of -ους names see e.g. Hainsworth 1969; 1973; Chadwick 1969: 86-93. For general references on Greek place-names in the light of the recent evidence cf. Hainsworth 1972 and Schachermayer 1972. The point about -ους and -υς has often been made and was discussed in detail by Wyatt (1968, with the earlier references).

24 For the form and function of some of these εις- derivatives, especially in Thebes, see Källen 1979: 176; Morpugo Davies 1979: 100-01, n. 32. Källen (ibid.) points out that one of the new terms from Tiryns has added yet another -εις- adjective to the list. The same conclusion about the εις- suffix and the Linear A evidence is reached independently by Källen 1983: especially 91. This article also offers the most detailed discussion of the Linear B suffix.
the wisdom of using Linear B values for Linear A signs. In some instances, however, this may be justifiable, especially if Linear A has sequences of signs which occur in the same order in Linear B and which in Linear B can be identified with words (such as Cretan place-names) which are also likely to occur in Linear A texts. Now, in Linear B we have evidence for a Cretan place-name, mentioned in the Knossos tablets: *sa-ki-ni-ja*. The same sign-sequence also occurs in a Linear A seal from Phaistos (PH 37) and the sequence is long enough to make us think that the coincidence is not accidental. The first three signs are found in a graffito on a pithos from Ayia Triada (H 7 Brice; Godart and Olivier 1978-), vol. 4: HT Zh 1586) where they are followed by three other signs. The last three signs can be read because they occur in other equivalent sign-groups of Linear B and Linear A. If so, our second-word can be read (in Linear A) *su-ki-ni-te-i-j* and compared with the place-name *su-ki-ni-te* of Linear B and Linear A. Godart (1976: 42-43), who argues for this, points out that the contrast *su-ki-ni-te*, *su-ki-ni-te-i-j* gives us the first clear indication of a Linear A suffix. Perhaps we can go further. One of the characteristics of the -je, -je suffix recognised as pre-Greek by Ruijgh is that it is often added to -ir stems, but the -ir is deleted (Linear B *pei-ni-go-ja* / *pe-ni-go-ja*, *pe-i-re-xe*, *pe-i-re-xe*, *pe-i-re-xe*, etc.). Ruijgh has also argued that perhaps there are traces of this suffix in some Greek endonyms (Miwos, river names ('Αλεπικά), etc.) At this stage it may seem reasonable, in view of the similarity of form, function, and usage, to compare the Linear B suffix and the -te-ja suffix of Linear A, even if we have only one piece of evidence for the latter. Obviously it is impossible to make a conclusive case for the identification of the two suffixes on the evidence available; a definitive proof must wait for further evidence from Linear A.

We are allowed to speculate. If the identification were correct would this mean that Mycenaean influenced the Cretan language? This seems unlikely, all the more so because in Linear A we would be dealing with a Cretan place-name. Presumably the language of Linear A influenced Greek. But we must remember that the suffix is found in Linear B outside Crete too; it occurs in Thebes, for instance. Presumably the suffix somehow came to belong to the Mycenaean language, and from there may have been transmitted to later Greek. We do expect to find lexical borrowings from Minoan into Greek; the borrowing of suffixes is a less common event. At this stage I have only a series of questions to ask and I shall not try to provide any answer. Presumably if the mainland and Crete spoke the same language, before this was 'replaced' by Greek, the existence (if real) of a common suffix in the language of Linear A and in Mycenaean Greek would cause no problem. The suffix could have been borrowed anywhere and at any stage after the arrival of the Indo-Europeans into Greece. On the other hand, it is often argued that it is unlikely that Crete and

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[The remaining text is not relevant to the main argument and is not included.]
BIBLIOGRAPHY

Abbreviations used are those listed in AJA 82 (1978) 3-8 and 84 (1980) 3-4, with the following additions:

JIES: Journal of Indo-European Studies.
TPS: Transactions of the Philological Society.

In transliterating Greek names we have followed what is familiar rather than what is always consistent.

Alexakis, V. 1979. 'Τέχνη και Αρχαιότητα στη Μινωϊκή Κρητή. ('Ο κάμπος της Μινωϊκής 'Εργασίας.)' Krísiología, 8: 41-56.
Arslan, J. L. 1971. Leses, II: The People. Princeton. (Published also as The People of Leses. Washington.)