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MYCENAEAN AND GREEK LANGUAGE

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Most scholars would agree that the greatest single result of the decipherment of Linear B is the demonstration that Greek was in use in Crete, in the Peloponnese, and, as we now know, in Central Greece, well in the Second Millennium B.C.* The discovery is important for historical reasons (it shows that the Mycenaean was Greek) but for linguistic reasons too. It is now trivial to notice, since it has been done so many times already, that we have for Greek the longest stretch of continuous attested history known for any Indo-European language. Because of this the nature of the Mycenaean language has been discussed with an intensity which is almost unparalleled for any other Mycenaean or Greek problem. No meeting of Mycenaeanists has been without a paper or papers on the Mycenaean language — the Dublin meeting is no exception (1).

These earlier papers are mostly concerned with minute or less minute questions of phonology, morphology, lexicography, etymology and occasionally syntax. In addition, a recurrent dispute concerns the position of Mycenaean among the Greek dialects. It is generally agreed that the Greek of the tablets, however early, cannot be identified with Common Greek, the hypothetical pre-dialectal language which we reconstruct in order to accommodate the linguistic features which are common to all forms of Greek and distinguish Greek from Indo-European.

attested in Greek; this would be the dual ending of Indo-European which Szemerényi also finds in $*d(u)woi$, the form he reconstructs for the feminine and neuter nom. of the number 'two'. If so, our views about the nature of the a -inflection would have to be altered: $-a$ would not be present in all forms. This is a typical instance in which certainty cannot be reached, but at the moment I feel inclined to accept the traditional view (7).

We are equally uncertain about the relevance to our Indo-European reconstructions of the numerous Mycenaean sentences which start with the syllable $jo-$ or $o-$ always joined in writing to the word which follows (normally a verb). The particle has been taken as a sentence connective parallel to those which appear with unfailing regularity at the beginning of the classical Hittite (or Anatolian) sentences (Watkins 1963:19-21). If so Mycenaean would reveal a notable archaism of a sort which later Greek has not preserved. On the other hand a more "modernistic" interpretation of $jo-/o-$ as a subordinating conjunction is also available and perhaps preferable, so that no definitive conclusion can be reached (8).

The almost complete absence of the augment from the Linear B texts causes similar problems. How is it to be explained? It is now likely that we must reject Wackernagel's theory about the optional omission of the augment as a characteristic feature of poetic language in Indo-European (cf. Hoenigswald 1964); in this sense our data contribute to our understanding of the parent language — beyond this, it is difficult to go, given the multitude of hypotheses and counterhypotheses and the absence of any agreed solution (9).

A second approach to Mycenaean concentrates on innovations and developments, but is not very different in technique from the approach I have just exemplified. The emphasis here is on the way in which Mycenaean has enlightened the prehistory of Greek in a wealth of individual points (10). A first

important result concerns the relative and absolute chronology of the main changes — primarily phonetic changes, but morphological changes too. It is now possible to distinguish between changes which occurred before (possibly long before) the date of the Linear B texts, changes which occurred shortly before the date of our tablets or were in progress at the time, and changes which occurred later than the texts (11). Some of our earlier views are confirmed: among the pre-Mycenaean changes are the shift from voiced to voiceless aspirates, the change of $*s$ to h in intervocalic and initial prevocalic positions, the development of labiovelars to velars before and after u , and the split of initial prevocalic $*y-$ in what will later appear as h or ζ ; remarkably all these changes are shared by all Greek dialects. There are surprises. For Gr. $\alpha\chi\upsilon\eta$ a pre-form $*\alpha\acute{\iota}\kappa\varsigma\mu\acute{\alpha}$ had been reconstructed long before the decipherment of Linear B. The tablets have given us the expected $\alpha\acute{\iota}-\kappa\alpha-\varsigma\alpha-\mu\alpha$, i.e. $\alpha\acute{\iota}\kappa\varsigma\mu\alpha-$, but this shows that a) the shift $-\kappa\varsigma\mu- > -\kappa\kappa\mu-$, though common to the whole of Greek, is post-Mycenaean; b) the change of $-\varsigma-$ to $-k-$ in this position is chronologically divorced from the change of intervocalic $*-s-$ to $-h-$, something which in our concern for economical reconstructions we would not have been able to predict (12).

In general, and not only with reference to chronology, what the new evidence has provided is a more complex view of sound changes and their development than we had imagined. The shift of intervocalic and initial prevocalic y to h is in progress in the Mycenaean period at a later stage than we expected for a change which is once again shared by all dialects (13). The various consonantal clusters which include $-y-$ are also developing but our understanding of the details is impeded by a perennial disagreement about the value of the Linear B signs of the z -series: za , ze etc. Opposite views argue for clusters of the ky/gy type (or for palatalized k'/g') and for some form of affricate ($[ts]/[dz]$, $[tʃ]/[dʒ]$, or the like); the earlier and later developments of the clusters in question will be differently understood depending on the

view adopted. My present opinion is that an interpretation of e.g. za as [kya] or [gya] is impossible (14).

Similarly there is no agreement about the development of vocalic liquids and nasals in Mycenaean. Unless we adopt the more drastic view that the liquids at least are still preserved as vocalic *l* and *l'* indicated by various graphic devices, we must reckon with different vocalisms as the result of originally unitary phonemes: we have *pe-ma* : *sperma* vs. *pe-mo* : *spermo* both from *spermw*, "seed", and *qe-to-ro-po-pi*, "quadripeds" with *k'et*o- < **k'et(w)*o- vs. *po-da-ko*, i.e. *podatgos* < **pod(H₂)gos*. We are at liberty to explain the *a/o* alternations as due to analogical levellings of various sorts, or to different phonological developments in different environments, or to different dialects in Mycenaean itself, or even to a combination of all these factors. What we lose, however, is the somewhat naive faith in an early *ētaf de langue* in which all treatments are wonderfully simple and regular in contrast with the later intricacies (15).

A final point shows how the data from the tablets, firmly anchored in time as they are (no one would doubt that they belong to the Second Millennium), far from clarifying the chronology of some developments, may, if not obscure it, complicate it: the final result, one assumes, will be greater accuracy rather than increased confusion. During the last twenty years the date of Grassmann's law, viz. the law of dissimilation of aspirates (*ekhō* < *kekhō*, etc.), has moved in an incessant seesaw. The traditional view attributed the operation of the law to the Common Greek period; in recent years the law has been backdated to Indo-European itself, though admittedly the Mycenaean data have had little to do with this move. Yet it has also been argued, and with good arguments, that Grassmann's Law is post-Mycenaean; further discussion has spoken of a date as low as the fifth century B.C. Even the best arguments seem to me inconclusive and I prefer to suspend judgement, though I cannot see myself ever accepting an Indo-European or a post-Homeric Grassmann's Law (16).

Morphology may teach us similar lessons; it is often more instructive to find out what we disagree about and why we do it than to report on what we unequivocally learn from the language of the tablets. Yet here too we have had confirmation of earlier suppositions and clarification of earlier queries. The use of the *-pki* forms in Homer is explainable if looked at against an earlier background where *-pki* was a real case marker, as is in the Mycenaean tablets, where it has a definite function and is subject to clear morphological rules. Forms like the Homeric epitheton *ἐκτιμενος*, which contrast with *κτιζω*, the related present, reflect, as we had always known, an earlier athematic verb; this is now attested in the Myc. *ki-ti-je-si* : *ktiensi*. The numerous *-eus* names of Homeric heroes make sense if we look at the Mycenaean texts where *-eus* was a highly productive suffix both for personal names and common nouns. More interesting is the problem set by the Myc. *e-ke-u-te-ro-se* : **ēleutheōse*. We know that the *-ōō* verbs, which are rare in Homer, are supposed to be late; the expected denominative from a thematic adjective is an *-eō* verb, for which we have comparative parallels, and not an *-ōō* formation. Yet, if as seems now established, the deadjectival factitives in *-ōō* are the continuation of original factitives with a laryngeal suffix (cf. Hittite *nēwaḫḫ-*, Lat. *novāre*, Gr. *νέω*), what we expect at an early stage is exactly a remodelled aorist formation (e.g. **ēleutheōse* replacing a supposed **ēleutheōse*) on which later on the new present will be formed. In this perspective the rarity of the *-ōō* presents in Homer, if contrasted with the *-ōsa* aorists, makes sense, but so does the presence of an *-ōse* aorist in Mycenaean (17).

We have had to acknowledge that some of our earlier views were too simplistic. The assumption that the genitive sing. masc. in *-ao* was due to the analogy of the *-oo* genitives of the thematic inflection is now proved to be wrong; in Mycenaean *-ao* exists but the corresponding thematic genitive ends in *-o-fo*. We have to find an alternative explanation for *-ao* and numerous suggestions have been made (18). The Cyprian

$\lambda\alpha\mu\iota\epsilon\kappa\epsilon\omega\iota\gamma\alpha\kappa\epsilon$: $\tau\alpha\kappa$ ($\lambda\eta\lambda\epsilon\omega\mu\iota\gamma\alpha\kappa$) (IOS 217, 20) used to be understood as "priestess" on the assumption that the feminine of $\lambda\epsilon\sigma\upsilon\varsigma$ was formed with the normal $-\gamma\alpha$ suffix on the stem $\lambda\eta\lambda\epsilon\omega$. Mycenaean with its $-\epsilon\text{-}\gamma\alpha$ feminines ($\lambda\eta\text{-}\gamma\epsilon\kappa\epsilon\text{-}\gamma\alpha$, etc.) has altered our views (19). Perhaps the Cyprian word means "sanctuary" or the like and is closer in formation to Myc. $\gamma\alpha\text{-}\lambda\eta\kappa\epsilon\omega\iota\gamma\alpha$, "retinue (?) of the $\gamma\alpha\text{-}\lambda\eta\kappa\epsilon\text{-}\omega$: $\gamma^w\alpha\lambda\epsilon\upsilon\sigma$ " than to Myc. $\lambda\eta\text{-}\gamma\epsilon\kappa\epsilon\text{-}\gamma\alpha$. While we debate about this we also debate about the formation of $\lambda\eta\text{-}\gamma\epsilon\kappa\epsilon\text{-}\gamma\alpha$: is an original cluster $-\omega\gamma\text{-}$ already simplified into $-\gamma\gamma\text{-}$ or are we dealing with a stem different from that of $\lambda\epsilon\sigma\upsilon\varsigma$ (20) ?

The best example of a disagreement which has far-reaching consequences concerns the nominal inflection. Here the ambiguity of the writing system is largely at fault, but uncertainty reigns both about basic problems such as the number of nominal cases in Mycenaean and about more minute questions such as the identification of the specific case-forms. The shift from the Indo-European system with eight cases to the Greek system with five cases was determined by a number of syncretism processes. What is the Mycenaean stage ? According to some, Mycenaean still preserves a fully fledged instrumental case marked by special endings not only in the plural ($-\alpha\phi\kappa\iota$, $-\delta\iota\varsigma$, $-\phi\kappa\iota$) but also in the singular ($-\alpha$ (?), $-\delta$, $-\epsilon$). Other syncretism processes would have just started : there are traces of dative-locative syncretism and the ablative would have started to syncretise not with the genitive but with the instrumental. Others, more traditional in their approach, assume that the instrumental has partially syncretised with the locative and hesitate to attribute to it special forms for the singular, since the spelling is ambiguous ; they also assume that the syncretism between genitive and ablative had already taken place as in all likelihood had that between dative and locative. This view is compatible with the suggestion that the odd forms which end in $-\omega$ instead of $-\epsilon\omega$ and are functionally treated as genitives are ancient $-\delta$ (< $*-\delta\delta$) ablatives which formally survive and are used as alternative markers for the genitive singular. According to the other view these $-\omega$ forms

have to be differently explained. The discussion has little hope of reaching a generally accepted conclusion, in the absence of more data and a better graphic definition of the relevant endings ; it has been profitable inter alia because of its somewhat iconoclastic challenging of all received opinions (21). It is possible that in future a different reconstruction of the Indo-European system may alter the position (22).

This and other problems belong of right with the rewriting of Greek historical grammar which has had to take place after the decipherment of Linear B. A basic work such as Ruijgh's *Études sur la grammaire et le vocabulaire du grec mycénien* (1967) addresses itself partly to this task and is indispensable, but the new material has found its way into the standard textbooks ; we may quote Lejeune's *Phonétique historique du mycénien et du grec ancien* (1972), Chantraine's *Morphologie historique du grec* (1961), and most recently Rix's *Historische Grammatik des Griechischen* (1976).

So much for a bird's eye view of what we know and have learned and of what we do not know and may perhaps learn in future. There is yet another way of looking at Mycenaean and Greek. This time the aim is no longer that of tracing the development from Indo-European to Mycenaean or from Mycenaean to First Millennium Greek ; it is rather that of exploiting the Mycenaean data to reach a better understanding of later Greek. A comparison of two relatively similar *états de langue*, such as those represented in the tablets and in the First Millennium evidence, can highlight differences and similarities and show that apparently trivial facts are in reality more significant than it appeared at first. We may hypothesize a situation similar to that of Latin where obvious features of e.g. Plautine Latin are absent from the classical texts but reappear in the late phases of the language or in early Romance. To know what these features are is at least as instructive for the classical Latin scholar as is for those interested in archaic Latin or the prehistory of Romance.

Work in the direction I have mentioned has of course been done and numerous observations of importance can be found in the writings of Chadwick, Chantraine, Heubeck, Lejeune, Ruijgh, and others. Yet the scholar whom I mostly associate with this approach is Ernst Risch who has developed it in more than one article (23). In what follows I propose to put together a number of data, few of which are new, in order to pursue the inquiry which Risch has initiated. I want to extend the range of my survey to include not only linguistic facts but what I prefer to call metalinguistic data. In other words, I shall not be limited to a discussion of the Greek language as was effectively written and spoken at any given period, but I shall also be concerned with the various and varying attitudes to language and linguistic facts. In this context I shall feel free to discuss graphic facts as well as linguistic facts proper.

I start with a metalinguistic point which may seem either obvious or superficial or both, but which I believe to be important. We return to the dialect question I mentioned earlier. There are more than 4600 tablets (or fragments) which come from a number of different places: Crete, West and East Peloponnese (Pylos vs. Tiryns and Mycenae), Central Greece (Thebes). In spite of their number and of their chronological and geographic remoteness these tablets are strikingly similar in the form of writing adopted, in the language used, and, of course, in the general arrangement of the content; the differences, if they exist, are minute. This calls for an explanation. It is difficult to imagine that at any period and in any place geographical differences are not linked to dialect differences of some type; speech differences must have existed in the Second Millennium even if they were not the same as in the First Millennium. If so, the linguistic similarity of our texts may conceal real divergences in the spoken language of the regions in question (24). Scribal schools or a dominating aristocracy may have imposed everywhere the same or similar letter shapes, the same spelling criteria, the same linguistic

forms. The uniformity must be due to ethnolinguistic conditioning (writing may have been conceived of as unitary and not susceptible of adaptation to regional differences; there may have been no interest in marking ethnic differences by means of linguistic forms) and in its turn the ethnolinguistic conditioning may be linked with political or social factors. At any rate the contrast with the Greece of the First Millennium is noticeable. In the later period we find numerous and macroscopic differences between the forms of writing of the various regions but the differences are not limited to the letter shapes. From the beginning of the alphabetic evidence the inscriptions reveal a number of dialect differences geographically determined. At some stage we have the impression that the dialect is deliberately emphasized as some sort of ethnic marker. Neither the graphic-linguistic uniformity of the Mycenaean texts, nor the divergence of the First Millennium texts are in themselves surprising; what is remarkable is the contrast between the two. We shall obviously want to explain the later development as due to a number of factors; perhaps the increase in linguistic variation may have played a part, but the changes in the political set-up, the beginning of individualism, and the greater concern with ethnic distinctions must have been important too. The contrast, however, raises other problems of a more literary nature. In Attic tragedy we find characters from different parts of Greece who speak in the same form of Attic; we assume that this is due to a precise stylistic and literary choice. A degree of speech realism such as that found in comedy would not be suitable: extreme colloquialisms are avoided and so is the expression of regional variation in speech. We know that the choice was a choice, and that it was deliberate, for a number of reasons: first, the contrast with comedy; secondly, the fact that tragedy itself often refers to dialect differences; thirdly, and more generally, the prevailing of dialectally differentiated inscriptions in the fifth century and the frequent references in contemporary authors to the ethnic importance of linguistic variation. If we now turn to Homer, how do we assess his position? In Homer, as in Greek tragedy, every character speaks

in the same language (barring stylistic differences, of course) and this applies both to Greeks and non-Greeks (25). Yet in Homer, as contrasted with Greek tragedy, there is no reference to dialect variety within Greek, no equivalent of the passage in the Choephoroe (563 ff.) where Orestes says that he will speak Phocian because he does not want to be recognized (26). Given this, is that of Homer a choice similar to that of Greek tragedy? Or should we rather link the ethnolinguistic background of the epic language with that of the Mycenaean tablets? There may have been a period in which all forms of composition – and this would include the humble administrative writings of the Mycenaean scribes and the more glittering verses improvised by oral poets – automatically avoided expressing some types of linguistic variation and in particular the types tied to geographical distinctions. My suggestions are obviously vague and to a great extent unprovable; they may simply serve as a pointer to the new range of data (or at least of hints) that we may extract from the Linear B texts.

From metalanguage to language or rather from the attitudes to language to the structure itself. I start with a point of syntax which has literary reflexes. Homer uses both compound verbs of the ἐπιτέλλω type and verbs in tmesis, where the preverb is separated from the verb as in ἐνὶ ... ἔτελλε (Il. 1, 25 vs. 11, 840). We know from comparison that tmesis represents the earliest state of affairs, i.e. it is a survival from a period when the preverbs were treated as independent words and had not yet been univerbated, i.e. joined to the verb. Mycenaean, on the other hand, shows regular composition of preverb and verb and no traces of tmesis even if the odds are that the status of the prepositions / preverbs was somewhat different from that of classical Greek. Thus paradoxically Homeric syntax is linguistically more archaic than the syntax of the Mycenaean tablets. So much has been forcefully argued by G. Horrocks (1980 and 1981.148 ff.) but he did not explore the consequences of this point for our understanding of Greek literature. It is now obvious that when Kühner-Gerth

(1898-1904 1.530) state that "in Homer one cannot speak of tmesis in the strict sense of the word, because in him tmesis is fully natural, non-artificial, and is rooted in the structure of the contemporary language", they must be wrong, even if allowance is made for the different speed of development of the different dialects. It is likely that from the Dark Ages onwards, i.e. in our terms through the whole of Greek literature, the audience of epic poetry was exposed to a syntactic feature, tmesis, which was alien to the contemporary language. Previously we might have supposed that with respect to tmesis the reactions of e.g. Plato and Alcman were different but we must now acknowledge that in all likelihood they were identical. Tmesis was a poetic feature equally remote from spoken language in the seventh and in the fourth centuries B.C. No doubt when tmesis appears in post-epic poetry it is due to epic influence rather than to the independent survival of an archaic speech pattern. If so, it is no longer difficult to explain why tmesis seems to be more frequent in tragedy (especially Euripides) and in Pindar than in e.g. Sappho (27). Tmesis is now a clear pointer to the stylistic choices made by the individual poets. In this connection its quasi-absence from Greek verse inscriptions (Wickkey 1981.64 note 66) is significant: such an explicit level of syntactical epic imitation is avoided by these humbler poets.

The evidence of Mycenaean here rectifies some of our earlier assumptions and contributes to our understanding of some post-Homeric stylistic choices, but as usual it also complicates the issue. Tmesis does not occur in Attic prose (28) but how are we to judge the odd instances of tmesis found in Aristophanes? The earlier interpretations, represented e.g. by Kühner-Gerth (1898-1904 1.535), assumed that most of the instances are paratragic, but Wackernagel (1926-1928 2.173) was certainly right in pointing out that some trimeter passages (ἀνὸ γὰρ ὀλοῦμαι in *Nub.* 792 and ἀνὸ σ' ὀλῶ κακὸν κακῶς in *Pl.* 65) reflect everyday speech; one could go further and argue that an analysis of the text shows that in Aristophanes tmesis

may have a special emphatic and colloquial overtone (29). How is this to be explained? According to Wackernagel we are dealing with a limited survival of the old construction. Obviously this cannot be excluded, though, now that we know that univerbation was earlier than Mycenaean, it is more difficult to accept than previously. Yet, if this "everyday" tmesis is in fact a fossilized relic, we ought at least to assume that some sort of stylistic polarization has taken place so that synchronically the two types, the "poetic" and the "colloquial" tmesis, were no longer felt as instances of the same construction. One may also wonder whether the "colloquial" tmesis was reinterpreted as an example of secondary word-split or even whether it could have originated in this manner. As parallels we think of some emphatic pronunciations of English *impossible* as two words (*IM-poss-ible*) or of the extreme example found in the emphatic and highly colloquial insertion of expletives in the middle of some non-compound words (*absolutely-lutely*, *absolutely-lutely*, *Jee-crawling-kova*, etc.) (30). The problem must be left to the Aristophanes specialists but ought to be considered.

Whatever we think about Aristophanes it seems clear that the normal forms of tmesis "sounded" poetic at all stages of Greek literature and it is likely that this is linked with the very early date at which tmesis disappeared from common usage and the way in which it was preserved as a stylistic option in oral poetry. The chronology is worth noting because it contrasts in an interesting manner with that of another syntactic phenomenon. Constructions of the type *ἄνθρωπος ἀνθρώπου* (Il. 11, 699, etc.) or *μὴν ἀνθρώπου* (Hdt. 3, 126) must be archaic: first, the use of *ἀνθρώπος* is opaque; secondly, they often do not have the article; thirdly, they use the dative without preposition in comitative function even when it is adnominal. How did the construction arise? The most plausible explanation (Wackernagel 1926-1928 1.57) suggests that at an early stage the pronoun *αὐτός* was not used in the dative in agreement with the following noun; it was rather in another case and the dative (by itself) still had its

original comitative function or rather the comitative function of the instrumental. We may suppose that a phrase such as *X, Y αὐτοῖς ἄνθρωπος* ("X and Y with (their) horses") replaced an earlier *αὐτοὶ ἄνθρωπος* ("they with (their) horses"). After the instrumental in comitative function had been replaced by a prepositional phrase (*οὖν* with the dative) *αὐτοὶ ἄνθρωπος* was no longer understood and the construction was remodelled; the pronoun came to agree with the noun and became part of a fossilized construction (31). If this explanation is correct we can now date the creation of this type of phrase to a pre-Homeric period (since it is found in Homer), but also to a post-Mycenaean period. In the tablets the instrumental can still be used adnominally with its comitative value. If we look at the diffusion of the idiom we find that after Homer not only it occurs in high poetry (tragedy, etc.) but is also found in comedy (Aristophanes) and in prose (Hdt., Thuc., Xen.). The distribution is very different from that of tmesis, but so, as we now know, is the chronology of the two constructions.

What I have been doing so far verges on the paradoxical. I have used Mycenaean evidence, i.e. the data provided by a few administrative tablets written in an inadequate script in the Second Millennium B.C., in order to assess the stylistic value of much later constructions used in Greek literature. Are the examples I have given unique? Obviously not. At this stage we can turn to some of Professor Risch's findings. Before we do so, however, a further point is necessary, a point which is in keeping with Risch's approach. The evidence we have for Greek in the pre-classical and classical periods is extraordinarily literary in character. The texts which have been preserved are first and foremost literary texts, even when they speak about philosophy, history, or medicine. Inscriptions exist but the majority of these are legal texts characterized by the high-faluting language which is typical of such documents. With Attic Comedy we come across a different type of language – but how do we assess the individual cases, how do we recognize the "very colloquial" from the

merely colloquial? In particular, what was the normal language of educated intercourse? The extraordinary thing about Mycenaean is that its scrappy documents are almost unique in register. They are written by scribes who are highly educated (they would have to be to know how to write) (32) but the scribes aim is simply that of stating administrative facts. Presumably there is no attempt at writing literature in the conventional sense of the word and above all at playing between different stylistic registers.

With this in mind we may contrast, as Risch has done, some of the features of the language of the tablets with those found in Homer. There is not much overlap in the vocabulary for obvious reasons, but occasionally we come across a specific lexical choice which is significant. In Homer the standard Greek word for "slave", δούλος, does not occur at all; yet Homer has twice the feminine δούλη, three times the adjective δούλιος, once the adjective δούλειος, and once the abstract δουλοσύνη. The obvious suggestion that δούλος is a relatively new word in Greek and has not yet been fully integrated in the epic language crumbles when we find that δό-ε-κo : δούλος is the regular Mycenaean word for "slave". If so, why does the word not occur in Homer? The epic poets must have known it, and this is indeed proved by the derivatives found in the poems. The only possible conclusion is that at the beginning of our literary tradition δούλος was too "low" a word to be used in poetry and therefore was excluded from the epic language. Risch 1972 has inserted δούλος in a group of similar words (δωπότης, ἐλεύθερος, ἐργάτης) which are equally not attested or scarcely attested in Homer, though they occur both in Mycenaean and in later Greek, and has argued that in all these instances the stylistic level of the words has not allowed their insertion in the epic vocabulary (33). One can only point out that, if so, we owe our new understanding of this fact (a stylistic point) to the Mycenaean data (34).

At this level too, as in the more straightforward analysis of phonology and morphology, Mycenaean may complicate or even

create a problem rather than solve it. Once again I borrow the example from Risch 1972 but I try to push the analysis somewhat further. The word for "amphora" is ἀμφορεύς in Homer, ἀμφορεύς in most of Greek; obviously the second form is derived from the first through a standard process of haplology. In Mycenaean we have both ἀμφιφάρεως (a-pi-po-ke-we) and ἀμφάρεως (a-po-ke-we). Before the decipherment of Linear B the only sensible interpretation of the facts was that ἀμφορεύς was a new form created after the composition of the epic poems. Now the situation is far more complicated. Obviously ἀμφορεύς was the only form which could fit in the hexameter, but should we assume that when the word occurs in Homer it is a survival from an early stage of epic composition – and hence sounded both "archaic" and "elevated" to the poet's audience? Or is it the case that ἀμφορεύς is simply a replacement, via reanalysis or popular etymology, for the current word ἀμφάρεως which could not scan? If so, there is no reason to take ἀμφορεύς as particularly elevated, though obviously later on it was felt to be so. The problem is insoluble; from a state of certainty we have moved to complete uncertainty – a step in the right direction.

For my last example in this series I turn to some linguistic material collected and discussed by Françoise Bader (1965), the group of -ουπύος compounds (δημουπύος, λεπουπύος, σιρουπύος, etc.). Once again I am stressing two points: the importance of the Mycenaean evidence for our understanding of later Greek, and the immense part played by literary factors in the development of the language of Greek poetry. It is obvious that compounds such as δημουπύος, λεπουπύος, etc. are formed by an adjective and a root related to that of ἔργον. Yet if the second element of the compound was indeed ἔργον we would expect a different accent and a different meaning. A λεπουπύος is a man who "deals with ἔργα", not a man who "has sacred ἔργα". Given the meaning, the obvious parallel is that of compounds of the -τοπος, -φορος type, where we have a verbal stem in the -o- grade. If so, we expect for the words quoted above earlier forms such as δάμιοωαγός, κίεωαγός, etc. Yet

Homer has $\delta\eta\mu\alpha\sigma\epsilon\pi\upsilon\sigma$, etc. with $-\epsilon-$ and not $-o-$. We would reach an impasse, were it not for the Mycenaean evidence which confirms that at the period there was a large number of $-wo\alpha\gamma\omicron\varsigma$ compounds: $\acute{\iota}-\acute{\jmath}\epsilon-\kappa\omicron-\omega\omicron-\kappa\omicron$: $\kappa\acute{\iota}\epsilon\kappa\omega\omega\alpha\gamma\omicron-$, $\acute{\iota}\omicron-\kappa\omicron-\delta\omicron-\omega\omicron-\kappa\omicron$: $\acute{\iota}\omicron\kappa\omega\omega\alpha\gamma\omicron-$, etc. How do we now explain the Homeric vocalism? The only possible hypothesis is that at some stage, before the final composition of the poems, the loss of intervocalic $-w-$ and the subsequent contraction led to forms such as $\delta\eta\mu\alpha\sigma\epsilon\pi\upsilon\sigma$, etc. In some instances at least these forms were unmetrical and the original quantity had to be restored; popular etymology, supported by the existence of some real compounds of $\acute{\epsilon}\pi\omicron\upsilon\upsilon$ (which consequently ended in $-\epsilon\pi\upsilon\sigma$ from the beginning), introduced in the text an $-\epsilon-$ instead of the earlier $-o-$ vowel. There is nothing new in finding that the language of the epic tradition includes remodellings of earlier forms but what is more extraordinary is the success of the $-\epsilon\pi\upsilon\sigma$ forms and the pattern of their distribution. All dialect inscriptions, all prose, with the exception of Herodotus, all iambic poetry have contracted forms of one type or the other ($-\omicron\upsilon\pi\upsilon\sigma$, $-\omega\pi\upsilon\sigma$, etc.) (35), but in dactylic poetry, from Homer down to the Byzantine period, we find the uncontracted $-\epsilon\pi\upsilon\sigma$ compounds; from Homer they move into Herodotus. At a very late period (post-Hellenistic) $-\epsilon\pi\upsilon\sigma$ appears in inscriptions and in prose too (Bader 1965.87). In a sense $-\epsilon\pi\upsilon\sigma$ has become a hallmark; it means poetic, dactylic, epic-like, and it comes to mean high-style. We cannot deny that these $-\epsilon\pi\upsilon\sigma$ compounds are part of the Greek language even when they are simply re-placements for $-\omicron\upsilon\pi\upsilon\sigma$ forms; yet they start life as mistakes, but mistakes of a great poetic tradition. Such an instance of creation and diffusion is difficult to imagine outside Greece. The whole history could not have been understood or reconstructed with any degree of certainty without Linear B.

Finally I turn again to an ethnolinguistic or perhaps a folk-linguistic problem. We now know Greek in three different written forms: Linear B, Syllabic Cypriot, and alphabetic. It has not been sufficiently stressed how important this is for

an understanding of the Greek reactions to language. A number of inquiries can start from a comparison of these three writing systems but I shall consider one point only which concerns word-division and the Greek notion of word. I have discussed the question elsewhere (Morpurgo Davies 1985) in much greater detail but I can summarize here some of my results. It is a common observation that the Greeks did not have a word for "word"; the obvious candidates for this function ($\acute{\epsilon}\nu\omicron\varsigma$, $\mu\acute{\upsilon}\theta\omicron\varsigma$, $\beta\acute{\eta}\mu\alpha$, $\lambda\acute{\omicron}\gamma\omicron\varsigma$, $\delta\upsilon\omicron\mu\alpha$) all have a wider or different coverage. What implications does this have beyond the purely lexical ones? Do we think for instance that it would have been impossible for a Greek to ask a question such as "Are these one or two words?". The answer is certainly negative for the first centuries A.D. At that stage the grammarians did explicitly discuss the question of "wordhood" and tried to develop a suitable technical terminology. But what was the position for the earlier period? Alphabetic writing normally did not divide words, so that we cannot use that source of information (36). Moreover we know that the notion of word is not an obvious one; modern linguists still acknowledge that it is not possible to find a satisfactory definition. The lexicon, as we have seen, does not give us any help. Herodotus, for instance, uses $\acute{\epsilon}\nu\omicron\varsigma$ to refer to a single word but also to a phrase, i.e. a sequence of words. Should we conclude that before grammatical analysis started the Greeks had no notion of word?

Mycenaean and Cyprian evidence now come in useful. Linear B uses a word-divider roughly where we would put empty spaces in our modern texts; Cyprian makes a more erratic use of word-dividers but has a spelling rule which presupposes a specific notion of word: word-final consonants are indicated by a sign of the $-e$ series ($\kappa\epsilon$, $\acute{\epsilon}\epsilon$) where $-e$ is a "dead", non-pronounced vowel ($\mu\omicron-\acute{\iota}\omicron-\acute{\iota}\acute{\iota}-\kappa\epsilon$: $\mu\acute{\iota}\acute{\iota}\kappa\epsilon$). If so, at the end of the Second Millennium B.C. and in the first part of the First Millennium some notion of word is available, though admittedly the Cyprian evidence can only count as marginal to Greek. What about Mainland Greece in the Sixth or Fifth

Centuries B.C., i.e. in the period in which we are most aware of the absence of a word for "word"? So far I have not made a case for continuity between Mycenaean and Cyprian, let alone between Mycenaean and the grammatical tradition. Conceivably the notion of word could have existed in the Second Millennium and have survived marginally in the First, but could have been lost otherwise, until it was eventually rediscovered when linguistic analysis became an object of study. Yet it is possible to argue for continuity. The word-dividers of Mycenaean texts do not always overlap with modern spacing. A first impression is that Mycenaean avoids using the word-divider to mark words which are written with a single sign. This may well be correct, but a more exact formulation must also state that prepositives and postpositives (proclitics and enclitics) are treated as part of the following or preceding word: the notion of word in terms of which the scribes operate is mainly based on accent (37). The position of Cyprian is not very different: enclitics are again treated as part of the preceding word at least when they are monosyllabic; proclitics create greater problems but in most instances are treated as part of the following word (38). So far then there is continuity. At this stage, however, it becomes clear that even the alphabetic texts can produce some relevant information. In the Sixth and Fifth Centuries B.C. we have a number of inscriptions where groups of letters are separated by bars or dots. Punctuation of this sort is used for a number of purposes which I do not need to rehearse here but in some texts at least it is clearly meant to separate grammatical units or words. It is important that the units thus identified are regularly formed by orthotonic words or sequences of orthotonic words and proclitics or enclitics. In other words the criteria for punctuation are roughly similar to those used for word division in Linear B and syllabic Cyprian: the word is an accentual unit (39). When we look at Fourth Century literary texts we find recognizable hints that this is how words are conceived: Plato treats $\mu\eta\ \delta\upsilon$ "not-being" as one $\delta\upsilon\omicron\mu\alpha$ (Soph. 237 c); in Aristotle (*de int.* 16 a b) it seems that $\omicron\upsilon\chi$ $\acute{\alpha}\nu\theta\rho\omega\pi\omicron\varsigma$ and $\omicron\upsilon\chi$ $\beta\upsilon\lambda\acute{\alpha}\nu\epsilon\iota$

are also treated as units rather than as sequences of two different words (40). More important still, by the time Apollonius Dyscolus comes round to discussing the notion of word it is clear that he operates with an accentually based notion which he first tries to refine and then to supersede by making appeal to distributional and structural criteria. It seems that we have sufficient evidence for a continuity of metalinguistic or folk-linguistic reactions from the Second Millennium B.C. to the First Millennium A.D., when self-conscious linguistic analysis intervenes. It is this continuity which allows us to interpret the admittedly tenuous evidence of the Sixth and Fifth Centuries B.C. punctuation in such a way as to establish from an ethnolinguistic point of view what notion of word was available to the Greeks in that period. Here too it is the Linear B evidence that has made it possible to reconstruct a piece of Greek cultural history which otherwise would have remained obscure.

APPENDIX I

VIEWS ON MYCENAEAN DIALECTOLOGY

For a general introduction to the problems of Mycenaean dialectology cf. the bibliography in note 1 and especially Panagl's account (Hiller and Panagl 1976.93-100). Useful and informative accounts also in Thumb-Scherer 1959.314-61, Cowgill 1966 (an admirable exposition of the problems), Bartoněk 1968 a, 1972 a and 1968 b, 1972 b (results of an inquiry about the various opinions on the subject), Schmitt 1977. 111-117, García-Ramón 1980 (clear and very informative), Duhoux 1983.40-57 (an extremely clear account). Much depends on the views we adopt about the prehistory of the Greek dialects; these will obviously be based, in part at least, on the Mycenaean evidence, but in turn will influence our understanding of Mycenaean. We can leave aside the question, to my mind insoluble in linguistic terms, whether all dialect differences arose in Greece or some at least were already present when the future Greeks arrived (jointly or separately) in Greece. More important, should we assume that the distinction between the so-called Arcado-Cyprian, Aeolic, Ionic-Attic, and West-Greek (i.e. Doric and North-West-Greek) groups goes back to the Second Millennium B.C.? The problem may of course be wrongly formulated (cf. Coleman 1963, Wyatt 1970, and also Adrados 1976 a, 1976 b, Moralejo Alvarez 1983 a). Yet those who answer in the affirmative tend to treat Mycenaean either as closely related to Arcado-Cyprian (the traditional view: cf. e.g. Tovar 1964, Ruijgh 1967.35-41, Lejeune *Mémoires* 3.215-22) or as an Aeolic dialect (Gallavotti 1958, 1968) or as a *koine* which arose from a mixture of different Ionic and Aeolic dialects (Georgiev 1956, 1964, 1966, etc. but cf. Bartoněk 1966). Yet the so-called Porzig-Risch hypothesis, which has found wide following, assumes that in the Second Millennium there was no differentiation between Arcado-Cyprian and Attic-Ionic or rather the

ancestors of these groups; if so Mycenaean could be closely related to this non-differentiated group, on condition that whatever differences exist between Mycenaean and Attic-Ionic are treated as due to later innovations and the apparent isoglosses with Aeolic are explained away (cf. e.g. Risch 1956 a, 1956 b, 1979 b, Chadwick 1963; for Attic-Ionic see López Eire 1974). Further work has also argued that the main defining features of Aeolic are post-Mycenaean (García-Ramón 1975); if so Mycenaean could simply belong to a non-differentiated East Greek group. There is no agreement on any of these points and the evidence is probably too scanty and contradictory to allow any definitive solution; it is also important that according to some scholars Mycenaean has no direct continuation in the First Millennium: cf. Heubeck 1961, Adrados 1976 a, 1976 b, Dunkel 1981, with various suggestions. There are further problems: Risch 1966 has distinguished within Mycenaean a *mycēniēn nommal* and a *mycēniēn spēcial* (closer to First Millennium dialects) on the basis of contrasting forms sometimes written by different scribes (e.g. *pe-mo* : *spēmo* "seed" vs. *pe-ma* : *spēma*; *-e[í]* vs. *-i* dative singular endings, etc.). This too is not generally accepted but Chadwick 1976 a, 1976 b, 1983 has pushed the distinction much further and argued that there were two Mycenaean dialects corresponding to a sociolinguistic distinction between upper (*mycēniēn nommal*) and lower (*mycēniēn spēcial*) classes. Previous research had taken for granted that Mycenaean was a pre-Doric dialect since the *-i* > *-j* change has occurred in Mycenaean while still in the First Millennium West Greek preserved the inherited *-i*. In Chadwick's view there is no need to postulate a post-Mycenaean Doric invasion; the Doric dialects would be the continuation of the Mycenaean substandard language just as the Dorians could be the descendants of the Mycenaean lower classes (cf. also Ruijgh 1978, Baumbach 1980 and, from a different angle, López Eire 1978 b). Opinions on this matter are divided; see the objections to Chadwick's theory by Moralejo Alvarez 1977, 1983 a.64 ff., Risch 1979 b.101 ff., van Soesbergen 1981: here too the linguistic evidence is far

from conclusive. At the moment any attempt at identifying a reasonable measure of agreement in any of problems discussed above seems to be doomed to failure ; most scholars would grant that the similarities between Mycenaean and Arcado-Cyprian are remarkable but even this fact is differently interpreted (see above) ; for an interesting analysis of some of the problems cf. Moralejo Alvarez 1979, and for an attempt at etymologizing the name of the Dorians made within the context of this discussion, see Szemerényi 1982.

APPENDIX II

VIEWS ON THE MYCENAEAN CASE SYSTEM

The bibliography on the Mycenaean case system overlaps to a great extent with the general bibliography on Mycenaean morphology (cf. note 1). The clearest case for the "pluralistic" view (Mycenaean has many more case markers and more cases than Greek) is that made by Ilievski 1970 (with reference to the earlier bibliography), but his general conclusions have also been accepted by e.g. Chadwick and Risch. The traditional view (Mycenaean does not differ drastically from Greek in its case-markers or syncretism patterns) is upheld e.g. by Ruijgh 1979 and Doria 1968, both of whom believe that Myc. -Co-í must be read -Cōís and not -Cōí(h)í or -Cōí as more generally assumed (particularly after the appearance of Lejeune *Mémoires* 3.1972. 255 ff.). Cf. also Wathélet 1970.334-343 for an intermediate position, and Moreschini Quattordio 1971 for a general discussion. There are two main problems. First, given the graphic ambiguity, how many different endings should we postulate for the singular ? Any answer depends, a) on reaching agreement about the form and value of the plural endings, which have a less ambiguous spelling, and, b) on general considerations. Moralejo Alvarez (forthcoming) interestingly argues that it is typologically impossible to have more formal case distinctions

in the plural than in the singular. If this typological rule were correct and suffered no exceptions (which is rare in the case of typological rules) those who find in Mycenaean a fully marked instrumental in the plural (terminations -āphí, -ōís, -phí, contrasted with dative -ā(h)í, -ōí(h)í, -sí) ought to feel ill at ease in arguing for a much simpler set of singular endings (Dat.-Loc.-Instr. -āí, -ōí, -ēí/í). On the other hand, those who read both -Co-í and -Co as -Cōís, like Ruijgh and Doria, may simply postulate a situation not very different from that of Homer : dative, locative and instrumental have already syncretised and -phí is an oddity on its way out so that one cannot speak of a fully fledged instrumental case for Mycenaean. Secondly, one ought to settle the question of the plural endings and more specifically of the -phí case. Ilievski and others think that -phí has instrumental value but can also be used (at least with place-names) in ablative sense, indicating provenience or origin. Ruijgh and others think that when -phí is not instrumental it has a locative value. The first school points out that, a) the -Ca-í or -sí nouns (dat.-loc. plural) do not alternate with -Ca-pí or -pí nouns in the same or similar contexts ; b) the formal contrast of the po-ka-í / po-ka-pí type calls for a functional contrast such as that of locative and ablative ; c) we need in the texts some way of expressing origin and at present we have neither a case nor a prepositional phrase which answers this need ; d) for 4-stem place-names we find -Ce-é forms which alternate in similar contexts with -phí forms of other place-names, while we also have -Ce-í forms which alternate with -sí forms of other place-names. This points to a functional contrast between -Ce-é / -pí and -Ce-í / -sí : the second pair will indicate dative-locative and the first instrumental-ablative ; if so, -Ce-é may be read -Ce(h)é with an instrumental singular ending. Of these arguments a) and b) are important, though I find it impossible to decide from the context whether the -pí place-names have locative or ablative value ; c) does not seem to be a necessary conclusion for these very schematic texts ; the importance of d) is somewhat diminished because we have

(a few) -Ce-e / -Ce-i alternations in the same texts (Morpurgo Davies 1966). I find it difficult to postulate an early syncretism of ablative and instrumental (which, if I understand it correctly, should not be limited to place-names only), because, if so, I could understand the later Greek developments only on the assumption that Mycenaean is totally different from the rest of Greek (Arcado-Cyprian offers some evidence for the expected syncretism of genitive and ablative in non-prepositional phrases). My second difficulty is that I still believe (Morpurgo 1960 a) that in addition to -Co-jo Mycenaean had an alternative ending for the thematic genitive singular which was spelled -Co, was pronounced -Cō, and derived from the ancient *-ōd ablative. This would presuppose a genitive-ablative rather than an instrumental-ablative syncretism; I fail to understand how Ilievski (1970.114) can conciliate the two facts. One should admit, however, that the -Cō genitive is supported by little evidence (Wathelet 1970.114). The more traditional view (Ruijgh, etc.) does not run into these problems but cannot explain why -Ca-i / -s-i place-names do not alternate with -Ca-pi / -pi place-names in the same context and why the -Ce-e termination of s-stems is limited to place-names. In contrast with my earlier position I now prefer to suspend judgement, though I still share the view that -Co-i represents an earlier *-Cois-i and that -Co can represent an earlier *-Cois.

NOTES

- * This paper owes a great deal to the discussion which followed its presentation in Dublin and to a number of observations made then or later by various scholars (especially A. Cassio, D. Fehling, Henry Immerwahr, Henry Hoenigswald, Zeph Stewart). I owe a special debt of gratitude to C.J. Ruijgh who read a first draft and identified a number of errors and some points of disagreement; I hope I have corrected the former but I have occasionally been obstinate

about the latter - obviously at my own peril.

- (1) The bibliography is immense. I can only refer to the most important books, to those collections of articles which are mainly concerned with language, and to a few general papers. See Vilborg 1960 (obsolete, but often referred to), Lejeune *Mémoires* (most of the papers collected in these volumes deal with grammatical problems), Lejeune 1976 (important for its attempt to establish a chronology of the Mycenaean features), Lejeune 1979 (methodological discussion with special reference to the labiovelars), Risch 1971, 1976 (see below), Ruijgh 1967 (indispensable), Ruijgh 1979 (important for Mycenaean morphology), Ruipérez 1972, Szemerényi 1968 b (which is inter alia an important account of the Mycenaean features which are relevant to Indo-European reconstruction). All general books about Mycenaean (e.g. Palmer 1963, Ventris-Chadwick 1973) have chapters about the language. We owe an important survey of the main linguistic problems, which is also a useful bibliographical survey, to O. Panagl (Hiller and Panagl 1976.78-100); for a helpful shorter account see García Ramon 1980; good bibliography can also be found in Hooker 1980.75-9. Cf. Appendix II for a discussion of the dialect position of Mycenaean.
- (2) See Appendix I below.
- (3) Two of these examples could be objected to. An -i ending of athematic dative singular appears in all Greek dialects but an -ei ending may underlie those Homeric datives where the final -i is scanned long; there are also odd survivals such as Cyprian *ἰλαίφιλος*. Moreover I am partly begging the question because an -i ending of dative could be attributed to Common Greek too since it is also found in Mycenaean; yet the point is that without the Mycenaean evidence we would have felt inclined to assume that both the syncretism of dative and locative and the choice of -i as the usual marker of the new "dative" were Common Greek. For the comparative the problems are well known: Szemerényi 1968 b has argued that Mycenaean had no -n forms but it must

be admitted that, although I am convinced by his arguments, the Mycenaean evidence is too limited to give a full proof of this point. Somewhat related to the question of ϵ - $\mu\epsilon$ is that of two other changes common to all Greek dialects: the change of final $-m$ to $-n$ and the loss of the final stops. A majority of scholars assumes that both changes have already occurred in Mycenaean, but some argue that Mycenaean preserved both final $-m$ and the final stops (cf. e.g. Ruipérez 1972.156 ff., Ruipérez and Vara 1972, Moralejo Alvarez 1976). On the other hand C.J. Ruijgh reminds me that all Greek dialects share the change of k^w to p , though Mycenaean still preserves a consonant sound which, whatever its exact phonetic nature, is the continuation of IE k^w and has not yet merged with p : a further example of the general point made above.

(4) From a methodological point of view it would be a useful exercise to collect all the features which we would have attributed to Common Greek before the decipherment of Linear B in order to try to assess them in terms of the new data. It would also be interesting to see whether a majority at least of the innovations which are common to all First Millennium dialects but have not yet occurred in Mycenaean could be explained in terms of Adrados' concept of *elección*. To give a concrete example: should we assume that for the genitive of the number "one" Common Greek had both the inherited (or remodelled) **kemos* and the analogical *kemos*? If so, we could argue that the presence of *évōs* in all Greek dialects except Mycenaean is due to separate *elecciones* rather than to separate innovations or mutual influences. In this case the proposed interpretation, a) is not very persuasive and, b) obliges us in the first instance to prove that the $-m > -n$ change is pre-Mycenaean (see above note 3), but the question is worth pursuing. For some work in this direction see Adrados 1976 a, 1976 b and López Eire 1978 a.

(5) The scholar who has been most productive along these lines is undoubtedly O. Szemerényi; cf. e.g. Szemerényi 1966,

1967 a, 1967 b, 1968 a, 1968 b, 1972, etc. and the use of Mycenaean data made in Szemerényi 1980.

- (6) Chantaine 1968-1980 s.v. reconstructs **demH₂-*, but see Ruijgh 1970.316; obviously I assume that the second $-e-$ of *dém[k]onxēs* is not due to analogy.
- (7) The basic data for the Mycenaean dual are discussed by Lejeune *Mémoires* 2.49 ff. and 3.277 ff.; cf. also Ruijgh 1979.74. The objection normally raised against Szemerényi's suggestion is that we expect the $-a-$ to be present in all forms of the inflection. More recently Moralejo Alvarez 1983 b has collected a number of Mycenaean feminine duals which end in $-a$ and has argued with good reasons that these forms preserve an inherited $-ai$ ending. If so, Szemerényi's view would be proved wrong, but the point is not totally demonstrated.
- (8) For the evidence cf. Risch 1968 and Lejeune 1976. Bader 1975 and 1976 speaks of a "particule d'énúmeration", but does not seem too remote from Watkins in her views; she also finds a $-jo$ enclitic particle in the form *to-šo-jo* of PY Er312. Some of Watkins' arguments are now obsolete; nothing can be made of the fact that *jo/o* is always joined to the following word since single-sign words are not written separately in Linear B; also it seems now likely that *jo/o* derives from **yo-* and not from **šo-*, which destroys the parallelism with the use of Skt. *śa*. On the other hand the connective hypothesis is still viable if we think of a **yo* preform comparable with the Hitt. enclitic $-ja$ and the orthotonic Goth. *jah < *yo-k^we* (cf. Dunkel 1983-84. 182 ff.). Risch 1968, working in a somewhat more "Greek" frame of mind, thinks of a subordinating conjunction ("how" or the like) so that the *jo/o* sentences would be basically equivalent to participial constructions: the argument has a great deal of plausibility. I am not certain that one should accept Chadwick's (1983.83) statement "It is generally agreed that it [viz. *jo/o*] means 'in the manner hereafter specified', 'thus', since from a Greek point of

view we do not expect a demonstrative meaning for the relative stem.

- (9) A good survey can be found in Bottin 1969 (with the previous literature); more recently see Blumenthal 1974 and Lazzeroni 1977. The problem seems almost unsurmountable. Because of the so-called temporal augment (the type found e.g. in ἦν "I was") which obviously arose from a pre-Mycenaean, pre-dialectal contraction (< *e(H)₁es-) we must assume that in Greek the augment is inherited as a bound prefix, not only as an independent particle. If so, we expect to find it in use in Mycenaean and indeed the semi-isolated *a-pe-do-ke* : *apeḏōke* (?) might give evidence for its presence in the language of the time. Given this, why is the augment ignored in most cases? If *e was a prefix which indicated past time this is even more difficult to explain since the scribes are interested in indicating temporal differences. Should we assume that in Mycenaean times the forms prefixed with *e-*, whatever their origin, had an emphatic value, and that in normal circumstances this was not required on our texts? If not, should we speak of a Mycenaean loss of the augment and treat it as a purely Mycenaean innovation (as hinted e.g. by Dunkel 1981.140)? R. Schmitt's (1967) attempt to attribute to Mycenaean different functions for the augmented and un-augmented forms in parallel to what has been suggested for Vedic hits against the absence of compelling evidence for or against it. Lazzeroni's (1977) reconstruction is intrinsically plausible but still does not explain the Mycenaean facts.

Another problem of verbal morphology which is often referred to concerns the third person singular middle ending. The fact that Mycenaean has *-to-i*, like Arcadian and Cyprian, and not *-tai* certainly does not prove that we must reconstruct **-toi* rather than **-tai* for Indo-European, though it offers pleasing support for earlier hypotheses (see Ruidérez 1968, but cf. Neu 1968).

- (10) Here one must refer mainly to work by Chadwick, Chantraine,

Heubeck, Lejeune, Risch, Ruijgh, and again Szemerényi; cf. above note 1 and below *passim*.

- (11) For the distinction accepted here between *pal-mycénien* and *proto-mycénien* see Lejeune 1976; the absolute and relative chronology of the main Greek changes as reflected in the Mycenaean data is discussed in most of the general works quoted in note 1; for more specific observations see most recently Bartoněk 1978, Risch 1979 a, Doria 1979, but it should be added that, though what is stated above is not controversial, most other conclusions are.
- (12) This last point is more doubtful. For the change *kām* > *kām* we might suppose, as Ruijgh does (1967.46), a shift *kām* > *khām* (cf. the spellings of the type *xo* for *fo*) followed by a simple loss of *h* in the cluster; this does not presuppose an *h* > *k* change (cf. also Lejeune 1972.74). Yet the exact nature of the stops in clusters of the stop + *h* type is not clear (Allen 1968.56 f.) and the alternative account is equally possible: cf. Viredaz 1982.
- (13) This seems to be the only plausible interpretation of hesitations such as that between *jo-* and *o-* (cf. also *o-te* : *ote* "when" < **yote* and *jo-qí* < **yo-*); see Ruijgh 1967.64 f.; Lejeune 1976. The same alternations in spelling occur with original intervocalic *-y-* which is also changing into *-k-* (Ruijgh loc. cit.); Chadwick 1983.82 f. assumes that the shift of intervocalic *y* to *k* had already happened at the time of the tablets but does not discuss forms such as *to-ko-qe-jo-me-no*, a participle from an *-ew* verb where *-eyo-* must be inherited.
- (14) For the basic bibliography cf. Panagl in Hiller and Panagl 1976.84 f., Risch 1979 a.267 note 1, Petruševski 1979, and add Chadwick 1983.80 ff. One view argues that the *z-* signs are used for affricates (both voiced and voiceless); the alternative view speaks of either *ky/gy* clusters or *k'(k') / g'(g')* palatalized consonants. Not enough attention has perhaps been given to Risch's proposal (1979 a. 275 note 25) that the *z-* signs could correspond to more

than one palatalized series (hence e.g. k' , g' , and d'), which might explain e.g. $me-zo$ "bigger" and $to-pe-zo$ "the two tables" as containing g' and d' respectively. I share the usual objections against a ky / gy value which presupposes inter alia a shift of dy to gy in contrast with the normal reconstruction ($*gy > *dy > Gr. \zeta$ or δ), but I also give weight to a morphological argument which I have not yet seen discussed. $za-we-tē$ has been identified with the later $ōtēs$ / $tēs < *kyāwētes$ "this year". This form is analogically rebuilt on $*kyāwēton$ "today", Gr. $σήμερον$ / $tēmeron$, where $*ky-$ is the pronominal element followed by the word for "day". If at the Mycenaean or immediately pre-Mycenaean stage $*kyāwēton$ still had this form, it is impossible to see why it should not have been segmented $kí/ky + āwēton$, since the word for "day" which survived in later Greek could not have been opaque. Yet such a segmentation could not have led to the creation of $*kyāwētes$ but only to the creation of an unattested $*kīwētes$: a resegmentation of the original $*kyāwēton$ which explains the formation of $*kyāwētes$ (the pre-form of $za-we-tē$) is only plausible if a considerable modification of the initial cluster had already taken place, making it impossible to prefix a complex consonant to $-wētes$. If $*ky-$ had already become an affricate when the new word was formed my condition would have been fulfilled, but the same observation may perhaps apply to a palatalized velar or dental, though this seems less likely.

- (15) For the bibliography cf. again Hiller and Panagl 1976. 82 f. and add, together with Morpurgo 1960b, the more recent Moralejo Alvarez 1973 and Bernabé Pajares 1977. The discussion concerns the "standard" treatment of $*n$, $*m$, $*k$, and $*g$, and the explanation of the alternative treatments. Basically, when do we have an $-a-$ vocalism, when an $-o-$ vocalism, and why? The evidence, we must admit it, is probably insufficient to lead to hard and fast rules. For $*n$ it seems reasonable to say that an o treatment, in alternation with a , occurs after labial; for m the

evidence is extremely scanty; for $*k$ and $*g$ we have both treatments with o and treatments with a . Heubeck's (1972) revolutionary suggestion that Mycenaean still preserved vocalic n as such has not been generally accepted; cf. most recently Bernabé Pajares 1977.296-8 and Viredaz 1983. 170 ff.

- (16) Some of the discussion has little bearing on Mycenaean. Kiparsky 1973 pleads somewhat improbably for an Indo-European origin of Grassmann's Law. More important for our purposes is that Ruijgh 1967.44 ff. has argued for a post-Mycenaean date of the law, partly supported by Lejeune *Mémoires* 3.239 ff. (cf. also Janko 1977). The arguments have been disputed among others by Wyatt 1976.6 ff. who thinks of a pre-Mycenaean date of the law. Miller 1977, on the basis of epigraphic evidence, has spoken of a date as low as the Fifth Century B.C. but the evidence needs reconsidering.
- (17) Tucker 1981 has convincingly argued that the $-ow$ verbs of Homeric Greek (in contrast with those of later Attic) must be distinguished into factitive verbs derived from adjectives ($ὀρθῶ$ "I make straight") and into verbs with instrumental meaning ($πυρῶ$ "I equip with towers"), which are derived from nouns. In Homer the first type is found mostly in the aorist or $-ā-$ future but never in the perfect; the second type is frequently found in the perfect. The first type can be compared with the Latin and Hittite factitives quoted above which are marked by an $*-H_2-$ suffix ($*newe-H_2-$ > Lat. *novāte*, Hitt. *newaḫḫ-*). In Greek the expected $-ā-$ stem was replaced in order to avoid homonymy with the verbs derived from a -stems (*τιυῶ*, etc.) and to make explicit the connection with the basic adjective: hence *ἀλευθέρω* instead of the expected **ἐλευθήκα*. The second Homeric type is derived from nouns and the pattern may have started, as suggested by Brugmann, from derivative adjectives such as *χολωρός*. A privative **ἀχόλωρος* could have called for a positive counterpart such as *μεχολωμένος* (cf. *ἀκίνητα καὶ μεκίνημένα*

and Myc. *a-na-mo-to* / *a-na-to-mo-to-me-no*) and the whole conjugation may have been built on these participles. If so it is striking that the only example of the factitives we find in Mycenaean is an aorist (*ēleukhetose*) and that the other instance of -ow verb (*qe-qi-no-to*, *qe-qi-no-me-na*, probably *g^weg^wino^wtoi*, *g^weg^winomena*) is a perfect and may be connected with the Homeric adjective *δυνωτός* and with the noun *δύναμις*.

- (18) Cf. e.g. Szemerényi 1956 (*-āygo > -āo earlier than *-oygo > -oo), Geiss 1956 (-āo < *-ādo with *-do pronominal ending), followed and improved upon by Ruijgh 1979.72 ; alternative hypotheses in Morpurgo Davies 1968.17 note 1, Ruipérez 1972.164, López Eire 1972.274 f., Risch 1974.118, etc.
- (19) See most recently Masson 1983.274 ff.
- (20) All previous views are listed in Christol 1975, who argues, partly on the model of Ruipérez 1966, for an earlier -uia form replaced by -eia, but see in favour of a phonological solution (-wy- > -yy-) Peters 1980.216 f. note 168. Further discussion in Viredaz 1983.174-182.

- (21) See Appendix II below.
- (22) Novelities may come from Anatolian ; cf. the references and the general discussion in Risch 1980 and see also Risch ap. Panagl 1983.373 ; for the dative-locative see Villar 1981.
- (23) See above all Risch 1972 and 1976.
- (24) I am not arguing for or against a particular dialect definition of Mycenaean or for the assumption that the dialect distinctions of classical Greek must go back to a pre-Mycenaean period (cf. Appendix I). I am simply stating that, on general grounds, there must have been a number of dialect differences between e.g. the speech of Knossos and that of Thebes.
- (25) I refer here to regionally defined differences, not to the syntactical, lexical, etc. features which may (or may

not) distinguish the speech of some individuals from that of others ; see for an attempt to identify some of these features in the language of Achilles Friedrich and Redfield 1978 with the objections by Messing 1981 and their own counter-reply (1981).

- (26) The Iliad and the Odyssey are notoriously silent about difficulties in communication ; in the Iliad the very little we are told normally refers to the Trojan side (Il. 2.804 ; 4.437-8). The Homeric epithets *παρθαρόωνοι* and *ἀπειροφώνους* probably refer to non-Greeks (Il. 2.867 and Od. 8.294). The four references in the Odyssey to men *ἀλλόθροοι* are either generic (14.43, 15.453) or also refer to non-Greeks (3.302, 1.183). The only passage where dialect distinctions may be mentioned is Od. 19.175-7, the famous description of Cretan multilingualism with its mention of Achaeans, Eteocretans, Kydonians, Dorians, and Pelasgians : how the passage should be interpreted remains obscure.
- (27) A useful collection of data can be found in Pierson (1857.119-128) on whom Kühner-Gerth loc. cit. are heavily dependent. I have not been able to see the two papers by G. Schilling about tmesis in tragedy quoted in Schwyzler-Debrunner 1950.425.
- (28) Tmesis occurs in Herodotus who is obviously a case *καὶ γενεαίᾳ*. For Attic prose cf. Kühner-Gerth loc. cit. who rightly dismiss instances such as Thuc. 3.13 *μὴ εἶν κακῶς ποιεῖν αὐτοὺς περὶ Ἀθηναίων*, ἀλλὰ *εὐνελεῖσθεοῦν*. The fact that these constructions, though marginal, are possible is interesting for what I argue below — here we are certainly dealing with innovations and not with syntactic archaisms.
- (29) I cannot agree with Sachtschal 1908.41 that all the instances of tmesis in trimeters are due to metrical necessity. The passages which I want to label colloquial and emphatic are all those which have forms of ἀπόλλυμι with tmesis, viz. Nub. 792, 1440 ; Av. 1506 ; Pl. 65 (all in ia. trim.), but also Ach. 295 (cretics) *κατὰ σε χάσμεν*

τοῖς λαοῖς; *Vesp.* 437 (tr. tetr.) ἐν τῷ σοὶ παρῆσται; *ibid.* 784 (ia. trim.) ἀνὰ τοῖς μετέθευς. At the opposite end is the "poetic" tmesis of *Pax* 1092 and 1274 (κατὰ μῦθ' ἔμην / οὐν β' ἔβαλον) used in hexameters together with pseudo-Homeric vocabulary. Other instances (*Av.* 346 (lyr.), [in 1070 (lyr.)] ἔμ ... ὁλλυταῖς is not in the *Mss.*], *Ra.* 1047 (an. tetr.), *Ly.* 262-263 (lyr.), 1280 (lyr.)) are not so easy to assess, but may well be examples of "high-style" tmesis. It is worth observing that ἀνὸ μ' ὀλέετς, ἦν ποοθ' ἔγχε also occurs in Sophocles in a quick moving and highly dramatic piece of dialogue (*Phil.* 817); unfortunately Moorhouse 1982.94, while giving a good list of examples of tmesis in Sophocles, does not comment on their stylistic level.

(30) I am grateful to Albio Cassio and Nan Dunbar with whom I discussed in general terms some of these problems, though they do not necessarily agree with what I say. Zeph Stewart provided me with *Jee-crawling-kova* from Harper Lee's *To kill a mocking bird*; the other examples can be found in the *O.E.D. Suppl.* I s.vv. *absolutely* and *bloody*; J.D. Hawkins quotes to me from his own experience *a-blasted-nother*. The standard example *not-bloody-likely* in Shaw's *Pygmalion* is meant to be emphatic, colloquial and low-class, but it seems that some of the instances of *abso-bally-likely* may belong to the Edwardian upper classes.

As an example of secondary tmesis (of a different kind) C.J. Ruijgh quotes French *très* (< *trans*-) which has been separated from the normal constructions of the *très* bon type to acquire a far more independent status ("non pas très", "j'ai très faim", etc.). Within late Latin and Romance one thinks also of the instances of late tmesis discussed by Löfstedt 1959.173-175; constructions such as the late *pet etenim abscondum* have an intensive *pet* which is normally a bound morpheme but can be used in tmesis through the whole of Latin, from Plautus and Terence to Cicero's letters, late Latin, and early French. This may be a survival of an old pattern (cf. Leumann 1959.49 ff.), but the late *prode tibi fuerunt*, also quoted

by Löfstedt, and the numerous similar phrases are extreme cases of the reanalysis of a word (*prodest*, etc.) which has led to the creation of new separate lexical items. In Greek the formal identity of preverbs and prepositions must have given a special "independent" status to the preverbs even if they normally occurred as bound morphemes only. (Cf. also note 28).

(31) See also Kühner-Gerth 1898-1904 1.433, Schwyzler-Debrunner 1950.464. C.J. Ruijgh suggests that a concomitant factor for the creation of this construction may be found in phrases of the type τὰς ναῦς ἑλαβον αὐτοῖς (τοῖς) ἀνδράσι, "they took the ships with the men themselves".

(32) We ought to acknowledge, however, that we do not really know what "highly educated" meant in a Mycenaean context. We would not call highly educated a secretary who knows how to operate a very sophisticated word processor but neither knows what makes it work, nor has read Shakespeare. I assume that the position of the Mycenaean scribes was different, but I should be hard put to justify my intuitions.

(33) Cf. also Gschnitzer 1976.8-13.

(34) Also important is the example of the -lov diminutives; in Greek literature they do not appear until the Fifth Century and then in comedy and prose only. Yet, though this has been denied (Ruijgh 1967.136), Mycenaean seems to have some instances of -lov neuters with a suitable value (cf. Lejeune *Mémoires* 3.230, 233 f.). If so, we have to admit that the huge gap in our evidence between Mycenaean and Epicharm is due to stylistic reasons: the diminutives did not suit the elevated style of the earlier Greek literature. We must similarly interpret a less shaking but also interesting case, that of the -lovoc diminutives. These now occur in Mycenaean but are not found in Homer (Chantraine 1966); yet the gap in our evidence is not so great since these diminutives also occur in early lyric poetry.

- (35) I include in this category forms of the $\delta\alpha\upsilon\omicron\pi\omicron\gamma\omicron\varsigma$ types, though $\delta\tau\acute{\iota}\kappa\iota\varsigma$ *sensu* they are not contracted.
- (36) Obviously there are phonemic segments or phonetic features which can only occur at the beginning or end of a word, and which may be reflected in writing. In Attic $\acute{\alpha}$ is normally found initially, while some sequences of sounds (and letters) cannot occur word-initially or word-finally. Yet, in marking these features, writing simply reflects pronunciation features which need not be perceived or recognized by the writer as having anything to do with words or the notion of word. M. Lang 1982.80 ff. has made a good case for the use of special forms of sigma (reversed three-bar sigma or four-bar sigma) to mark the end of a word or word group in Attic ostraka of the Fifth Century (I owe the reference to Henry Immerwahr). This may again indicate a special pronunciation of sigma in these positions and consequently does not enlighten us about the Attic notion of word.

- (37) We expect Linear B to avoid single-sign words since these could be confused with ideograms; on the other hand if this were the only valid rule there would be no reason to join the two-sign $\sigma\text{-}\alpha$ "not" to the following word, as regularly happens, nor to write $\epsilon\text{-}\kappa\epsilon\text{-}\delta\epsilon\text{-}\mu\acute{\iota}$: $\{h\}ekhe\acute{\iota}$ $\delta\epsilon$ $\mu\acute{\iota}\alpha$ as a single sequence; $\delta\epsilon\text{-}\mu\acute{\iota}$ could be separated. For the rare instances in which a compound is treated as two words and for other exceptions to the general rule see Morpurgo Davies 1985.
- (38) Difficulties arise because proclitics which end in $-n$ are treated as such, while apparently proclitics which end in $-\delta$ are treated as separate words; for a discussion of the relevant problems cf. Morpurgo Davies 1985.
- (39) The standard example is provided by the $\tau\epsilon\lambda\alpha\epsilon$ $\vartheta\acute{\iota}\alpha\alpha\epsilon$ (Schwyzer 1923, no. 757); the inscription is now lost, but a new stele which belongs with the old one and has the same graphic characteristics has been recently found: cf. the excellent edition by Hermann 1981, where, however,

the photographs allow one to recognize on the stone a few more interpuncts than those reproduced by the editor.

(40) We are told by Aristotle *loc. cit.* that $\omicron\upsilon\kappa$ $\acute{\alpha}\nu\theta\epsilon\pi\omega\varsigma$ is not an $\acute{\omicron}\nu\omicron\mu\alpha$ but an $\acute{\omicron}\nu\omicron\mu\alpha$ $\acute{\alpha}\phi\omicron\lambda\omicron\sigma\tau\omicron\upsilon$ (similarly $\omicron\upsilon\kappa$ $\upsilon\tau\alpha\acute{\iota}\nu\epsilon\iota$ is not a $\beta\eta\mu\alpha$ but a $\beta\eta\mu\alpha$ $\acute{\alpha}\phi\omicron\lambda\omicron\sigma\tau\omicron\upsilon$); whatever the philosophical significance of this view, it is difficult to see why the problem of the status of these phrases would have arisen if Aristotle had not considered them as individual words.

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