MYCENEOLOGY TRENDS AND NEGATIVE CRITICISM

Lydia Baumbach
(University of Cape Town)

My excuse for adding yet another review to the many which have already been published on the progress in the field of Mycenaean Studies since 1952, is that this year marks the 'coming of age' of Mycenology; for on 1 June it will be 21 years since Michael Ventris introduced his now famous Worknote 20 as 'a frivolous digression', and headed it 'Are the Knossos and Pylos tablets written in Greek?' This 'frivolous digression' has given rise to a whole new field of Greek studies, which has drawn scholars from all parts of the world into its ranks.

In his paper at the University of London, marking the tenth anniversary of the decipherment in 1962, Dr. John Chadwick dismissed with the scorn it deserved the attempt by Professor A. J. Beattie of Edinburgh to cast doubts on Michael Ventris' integrity. Beattie's accusations that Ventris had had a preview of the famous Tripod tablet, or worse still, that he had had access to a similar tablet which he had subsequently destroyed, could not be taken seriously except by their inventor; his doubts about the correctness of the decipherment are, however, still shared by a number of scholars, and, in a review of this kind, we must give some attention to this negative criticism of the decipherment.

Some scholars reject the decipherment because they do not accept the presence of Greeks in Crete or on the Greek Mainland before the end of the Mycenaean period. So, e.g., Mr. Sinclair Hood, formerly Director of the British School at Athens, reflects this scepticism in the title of a book he published in 1967, The Home of the Heroes: the Aegean before the Greeks, which prepares us for the statement that (the decipherment) has been seriously challenged, and the language may not be Greek after all. In a more recent publication, The Minoans, Mr. Hood seems to have shifted his ground somewhat, where he says: "Even if this particular decipherment is false, it does not necessarily follow that the language of the Linear B tablets is not Greek; moreover, even if the langu-

1. The following is the text, with minor alterations, of a paper read at the tenth biennial Conference of the Classical Association of South Africa at Johannesburg from 22-26 January, 1973.


age proves to be non-Greek, it is always possible that rulers of Greek speech controlled Knossos at the time, retaining an alien written language for administrative purposes". The date of the arrival of 'the Greeks', or rather of a group of I.E. speakers whose language was later to develop into the Greek of the classical period, is still a matter of controversy, but the theory that the people whose arrival in the Balkan peninsula c. 2100–1900 BC is attested archaeologically were the ancestors of the Greeks, has much to recommend it. The intervening period, down to c. 1400 BC, would have been sufficient time for the I.E. speech of the invaders to develop into the Greek found on the tablets.

Other scholars are sceptical of the decipherment because of a misunderstanding of the nature of the script. So the late Professor E. Grumach, followed by Mr. W. Brice and others, believe that the script is largely ideographic, and they find it hard to accept a syllabic script for Crete and Greece in the 2nd millennium BC. This is what Brice has to say about the matter:

"The system of syllabic writing hypothesized by the Decipherment would have been unique at the time. In the 2nd millennium BC all the major Near Eastern scripts employed a complex combination of ideograms, determinatives, syllables of various types, and phonetic complements. A simple syllabary of the type assumed for Linear B would mean that, in the art of writing, Crete was a thousand years in advance of Egypt and Mesopotamia – a highly unlikely circumstance." Similarly Werner Ekschmitt in a recent publication: "Historisch ist es überaus unwahrscheinlich, dass dieselbe Schriftstruktur, die wir im 7. Jahrhundert auf Zypern finden, schon 600 Jahre früher in Pylos, und 800 Jahre früher in Knossos in Gebrauch war. Ein Syllabar nur aus Vokalen und offenen Silben ist für diese Zeit einfach zu schön, um wahr zu sein."

These opinions are based on a purely subjective, pre-conceived idea of the type of writing one could expect in the Aegean at this period; if we consider the signary with its 90 odd signs, about 60 of which are in frequent use, and examine the arrangement of the tablets which contain sign-groups obviously representing words, followed by ideograms and numerals, we cannot avoid the conclusion that we have to do with the type of syllabary which these scholars consider impossible for the period.

The decipherment has also been attacked on the grounds of method, and it has been suggested that Ventris and Chadwick misrepresented the rôle played by the 'grid' in the decipherment, and depended far more on the general principles and the particular sound values of the Cypriot syllabary than they wished to admit. This is stated, e.g., by Ekschmitt; it is clearly a distortion of

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the facts of the case, for both Ventris and Chadwick⁹ stress the small rôle played by the Cypriot syllabary in the decipherment, and there is no reason to disbelieve them. The clumsy orthography and the spelling conventions, too, have come under attack. On the face of it, this might seem a more justified objection. Hood, e.g., points to the two interpretations given of pa-te, i.e. pater in one context, pantes in another.¹⁰ What he does not say, is that it is absolutely clear from the context which word is to be understood; pa-te=pater is used in the same context as ma-te=matē, while pa-te=pantes is used to indicate a total.¹¹ It must be remembered that as far as we know, the Linear B script was used for nothing except the records of palace administration; that though the script would have been wholly inadequate for sophisticated literary composition, it was probably quite satisfactory when used by a number of scribes accustomed to the convention, as a means of keeping records, e.g., to remind King Nestor of the number of wheels or leather goods he had in his palace.

Ventris and Chadwick have been accused, too, of 'smuggling' the phonetic values of Classical Greek into Mycenaean, and of inventing the spelling conventions so as to be able to wrest convincing Greek words from the tablets. So, amongst others, R. Thibau in a number of articles.¹² What Thibau and other critics fail to see, is that the spelling conventions were formulated to explain the existing situation, not as hard-and-fast rules laid down beforehand.

Critics of the decipherment have also expressed doubts about the validity of the decipherment because of the many difficulties of interpretation which still remain; even the famous tripod tablet¹³ bristles with uncertainties and difficulties, e.g., the lack of concord between ti-ri-po-de (dual) and a₂-ke-u (singular) – if a₂-ke-u is in fact a descriptive adjective with ti-ri-po-de; and also between di-pa (singular) and me-zo-e (dual). Granted that there are still many obscurities, many areas of doubt; granted that some scholars have brought disrepute on Mycenaean studies by their ill-considered and far-fetched interpretations; still, these give no reason for rejecting the decipherment altogether. It is highly likely that there would have been words (apart from proper names) in Mycenaean which did not survive into later Greek; indeed it would be strange if it were not so. Certain technical terms connected with land

⁹. Chadwick, The Decipherment of Linear B pp. 23–24, 60.
¹¹. PY An 607; KN B 1055.
¹³. PY Ta 641.
tenure, e.g., would be likely to go out of use when the system of land tenure ceased at the end of the Mycenaean age. 14

The obscurities and difficulties of interpretation have led some scholars to reject the decipherment entirely; others, while accepting some of the words on the tablets as Greek, postulate a second, non-Greek language for the Linear B tablets. So Professor S. Levin speaks of Linear B as a jargon containing both Greek and non-Greek elements; J. T. Hooker, 15 too, finds in forms like e-keqe non-Greek elements in Linear B.

Ekschmitt rejects the argument that the decipherment is acceptable by its results alone; for my part, I am perfectly content to accept a decipherment which on the whole works; which in many cases gives perfectly good sense, and which moreover gives us a form of Greek which comparative I.E. studies had given us reason to expect. I think, e.g., of the presence of the labiovelars at this period, of the consistent writing of the /w/, of archaic forms such as the gen. sing. of o-stems in -o-jo (cf. Hom. -oio). Certain other declensional forms, such as the gen. sing. and plur. of masc. a-stems in -a-o (representing Hom. -ao and -aou), the old instr. in -pt (Hom. -pt), particles such as -qe (later τι) and allative and connective -de, and a number of prepositions (e.g., pa-ro, a-pa, po-si) give further support for the correctness of the decipherment. I give just one example of interpretation: some years ago I had a letter from Dr. Chadwick in which he told me of a new reading in a Knossos tablet (KN Uc 160) which lists wine, amongst other commodities. The word now read is de-reu-co, and it is squeezed into the tablet, rather as an afterthought, to qualify the wine ideogram. We both independently interpreted this word as deleukos, which is a possible early form of γλευκος, the later Greek word for 'must', Afrikaans 'mos'. This initial d was to be expected if γλευκος and the adjective γλυκος are cognate with Latin dulcis. 16 This is another case where, as in the case of the words qe-to-ro-we, ti-ri-jo-we and a-no-we on the tripod tablet to describe pots with four, three and no handles respectively, or of the occurrence of such transparently Greek names as Theodora and Alexandra on a Mycenae tablet, we must ask with Professor Blegen: 'Is coincidence excluded?' The onus is, I think, on the critics of the decipherment to prove their case, and to produce an

14. An example is the type of land referred to as ke-ke-me-na (e.g. PY Ea 305+). It is reasonably certain that this is the description of a type of land held from the da-mo (δαμο), and it has been connected with κωνος from *kei- (Palmer, The Interpretation of Mycenaean Greek Texts p. 425 and elsewhere), but no general consensus of opinion on the etymology of the word exists, as a glance s.v. ke-ke-me-na in Studies in Mycenaean Inscriptions and Dialect pp. 174f will show.


alternative solution which gives better results. Until such time I shall continue to be a 'Ventrisianer', as Ekschmitt rather scathingly calls the supporters of the decipherment, and remain with the majority of Greek scholars, who, while admitting that much work remains to be done, accept the decipherment as essentially correct.

Therefore I have considered it worthwhile to report on the steady work which has continued in the field of Mycenaean studies. There have been five Colloquia on Mycenaean Studies, to which a limited number of scholars from different parts of the world have been invited: at Gif-sur-Yvette, France, in 1956, at Pavia, Italy, in 1958, at Wisconsin, U.S.A., in 1961, at Cambridge, England, in 1965, and at Salamanca, Spain, in 1970; a sixth Colloquium is being planned for 1974, in Switzerland. I was privileged to be able to go to the Salamanca Colloquium which was attended by 33 scholars from 15 countries. It was an exciting and enriching experience to become part of an international community of scholars, and to see the happy co-operation between representatives of Western and Eirene countries.

There have also been larger conferences on the subject, such as the Colloquia arranged by the British Association of Mycenaean Studies, the third of which will be held at Sheffield in April this year, while the monthly seminars at the University of London continue. There have been Mycenaean sections of Classical Congresses in the Eastern European countries, such as those held at Brno in Czechoslovakia in 1966, and at Cluj in Rumania in 1972. In 1967 the first International Congress of Mycenaean Studies was held at Rome, organised by Professors C. Gallavotti and Anna Sacconi and their staff at the Centro di Studi Micenei ed Egeo-anatolici; this congress was attended by c. 300 classical scholars from all over the world, from as far afield as America on the one hand and Japan on the other. All aspects of Bronze Age Studies, linguistic, archaeological, historical and religious, were included in the programme. The Mycenaean Centre in Rome is also responsible for the publication of the Incunabula Graeca series of books on a variety of topics dealing with the Bronze Age in the Mediterranean and the Near East. The proceedings of all

17. Work on an alternative decipherment is at present being done by Professor S. Davis, of the University of the Witwatersrand, Johannesburg, the publication of which is still awaited.

the Colloquia and Congresses have been, or are due to be, published and represent an important part of the already vast literature in the field. A permanent International Committee for Mycenaean Studies (CIPEM) has been set up, with affiliation to UNESCO.

Important progress has been made in the editions of the tablets. In the assigning of phonetic values to the signs, a few gains have been made in recent years. At the Cambridge Colloquium in 1965, it was decided to accept the value two for *91, while the Salamanca Colloquium agreed upon the values au for *85, and twee for *87. The value au is particularly important, and has made possible a number of interpretations which add some common Greek words and personal names to the Mycenaean vocabulary, e.g., Αυγέας, ουλός, ουτός.

At the major sites, Knossos and Pylos, few new finds of tablets have been made in recent years, and few can be expected for the future; but at Knossos in particular important new evidence has been obtained by joins of fragments of tablets, made particularly by Dr. J. Chadwick, Dr. J.-P. Olivier, Dr. J. T. Killen, and lately by Dr. L. Godart. Some of these joins have made possible a more precise classification of tablets, and have necessitated a new edition of the Knossos tablets. This new edition, prepared by Chadwick, Killen and Olivier, appeared in 1971 and supersedes the earlier editions brought out by the Institute of Classical Studies of the University of London. A weakness of this and earlier editions is that they contain no photographs or line-drawings of the tablets, so that scholars using the editions are dependent only on transcriptions, and have no idea of the actual appearance of the signs on the tablets. However, an edition containing a full set of photographs and line-drawings would be prohibitively expensive.

For Pylos, no full edition of the tablets has appeared since the publication in 1961 of Inscriptiones Pyliae, an edition of the tablets in transliteration by C. Gallavotti and A. Sacconi. E. L. Bennett’s edition of the tablets found between 1939 and 1954 (PT II), which contains line-drawings of the tablets, is still a useful source, supplemented by the Olive Oil Tablets of Pylos: Texts of Inscriptions found, 1955, and the annual publication in the American Journal of Archaeology of tablets found in the excavations at Pylos between 1957 and 1965. A new edition of the Pylos tablets in transcription, compiled by E. L. Bennett and J.-P. Olivier, is being eagerly awaited.

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20. See Baumbach, Glotta 49 (1971) s.v. Αυγέας, ουλός, ουτός.
At Mycenae, the excavations of Professor G. Mylonas in 1969 revealed a Linear B tablet in the citadel of Mycenae.\textsuperscript{23} This is important, as being the first tablet found in the citadel of Mycenae itself. A revised transliteration by J.-P. Olivier of all the tablets found at Mycenae from 1950 to 1967 appeared in 1969. This edition is useful in that it brings together all the tablets from Mycenae in one volume, and indicates some changes of reading; but it does not entirely supersede the earlier editions (MT II and III)\textsuperscript{24} with their photographs and reproductions of the actual tablets, and line-drawings of the text.

The expectation of finding Linear B tablets at Thebes was fulfilled when in 1964 and again in 1970 a limited number of tablets was found in the excavations of the Mycenaean palace there. The 1964 tablets have been published;\textsuperscript{25} they are all tablets in the same hand, dealing with the same commodity (indicated by the abbreviation \textit{o}); the 1970 tablets still await publication. Though the 1964 tablets are limited in scope, their discovery is extremely important. The tablets show the same conventions of orthography and spelling as those at Knossos, Pylos and Mycenae, and confirm the picture of a uniform type of culture in Greece and Crete in the LH III period. Moreover, if the dating of these Thebes tablets to LH III A2 (c. 1350–1300 BC) is correct, they serve to bridge the gap between the Knossos tablets (traditional date for the fall of Knossos 1400–1375 BC) and those of Pylos and Mycenae (destroyed c. 1200 BC or later).

The most recent report of the discovery of Linear B tablets on the Greek Mainland comes from Tiryns. According to the report,\textsuperscript{26} two fragments have been found, one reading \textit{to-so-pe-mo}, and also a sherd with two signs. Here, too, we await the publication of the tablets with interest. Meagre though the finds at Tiryns are, they are important as showing yet another Mycenaean palace which used the Linear B script.

There have been further finds of stirrup jars with inscribed signs not only on the Greek Mainland, but also at Chania in Crete.\textsuperscript{27} The importance of the inscriptions on the sherds from Chania (possibly to be identified with ancient Kydonia) lies in the fact that this is the first evidence for the use of the Linear B script in Crete outside Knossos. Further, the sherds are dated to the LM III B period, which indicates that Crete continued to use Linear B after the fall of Knossos (assuming the early date of 1400–1375 BC for its fall). It has been

\textsuperscript{25} J. Chadwick, \textit{Minos} 10 (1969) pp. 115–137.
\textsuperscript{26} Per litteras, J. Chadwick 9.vi.72.
reported\(^8\) that a sherd containing a single Linear B sign (ι) has been found at Nichoría in the Peloponnese in the 1972 dig conducted by Professor W. A. MacDonald. This is important, for even one sign gives some indication that Linear B was not for the exclusive use of royal palaces (Chadwick).

An important discovery was made as a result of the analysis of the clay of the Theban jars, carried out by Dr. H. W. Catling and Mrs. A. Millett in the Ashmolean Museum, Oxford.\(^8\) From this analysis it appears that, though some of the jars were of local clay, the clay of others comes closest in composition to clay from centres in East Crete, notably Palaikastro and Kato Zakro. If these jars were made in East Crete, we have a further indication that Linear B was used in other places in Crete, not only at Kaossos (already suggested by the sherd from Chania). The findings of Catling and Millett are not accepted by all scholars, e.g. Professor J. Raison,\(^30\) who has brought out an edition of the Theban vases, but they certainly raise some interesting points,\(^31\) and should not be too hastily dismissed.

Steady work has also been done in the field of lexicography and bibliography. Professor Anna Morpurgo-Davies' Mycenaeae Graecitatis Lexicon, published in 1963,\(^8\) is still the standard work in the field of Mycenaean lexicography; the time now seems ripe for the publication of a supplement to this to incorporate the material produced by new finds of tablets and by joins.

A supplement to the Mycenaean Greek Vocabulary published by Dr. Chadwick and myself has recently appeared,\(^33\) in which some new entries were made, and some earlier entries revised. These vocabularies do not contain a full list of all Mycenaean words, but list only Greek words which can with a reasonable amount of certainty be identified in the tablets.

Another useful reference book in this field is Professor M. Lejeune's Index Inverse du grec mycéen, Paris 1964, which groups Mycenaean words according to final syllables.

The evidence supplied by the decipherment of Linear B has also been incorporated in recent etymological dictionaries of the Greek language, particularly in the latter part of H. Frisk's Griechisches etymologisches Wörterbuch and the volumes of P. Chantraine's Dictionnaire étymologique de la langue grecque.\(^34\)

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which have appeared to date.

Many bibliographies of Mycenaean studies exist. I mention here only the \textit{Bibliographie der Kretisch-mykenischen Epigraphik} compiled by Professor E. Grumach (to 31.12.1961) with its supplement of 1967, the monthly issues of \textit{Nestor}, edited by Professor E. L. Bennett and distributed free of charge to persons interested by the University of Wisconsin, Madison, and the annual volumes entitled \textit{Studies in Mycenaean Inscriptions and Dialect}, edited by Professor L. J. D. Richardson and published by the Institute of Classical Studies of the University of London. The first ten volumes have appeared in a combined edition.\textsuperscript{35} These volumes of \textit{Studies} contain, besides a list of publications for the current year, three indexes, one of interpretations of vocabulary words, the second of tablets discussed in detail, and the third a subject index.

With the exception of the chapters on Mycenaean grammar in general introductions to Mycenaean studies, only one Mycenaean grammar has so far appeared, that of Ebbe Vilborg, published in 1960. A section has also been devoted to Mycenaean in the revised edition of Thumb-Scherer, \textit{Handbuch der griechischen Dialekte II}.\textsuperscript{36}

In addition to \textit{Documents in Mycenaean Greek} by Michael Ventris and John Chadwick,\textsuperscript{37} which remains a standard work, a number of general introductions to Mycenaean studies have been published. L. R. Palmer's \textit{Interpretations of Mycenaean Greek Texts}, closely modelled on \textit{Documents}, has already seen a second edition;\textsuperscript{38} in French we have L. Deroy's \textit{Initiation à l'épigraphie Mycénienne},\textsuperscript{39} in Italian M. Doria's \textit{Avviamento allo studio del miceneo},\textsuperscript{40} in German Alfred Heubeck's \textit{Aus der Welt der frühgriechischen Lineartafeln},\textsuperscript{41} all useful as introductions to Mycenaean Studies. \textit{Documents} itself is due to appear in a second, enlarged edition incorporating the material discovered since its first appearance.

In the interpretation of Linear B vocabulary, we seem to have reached, to quote Chadwick,\textsuperscript{42} the point where the "law of diminishing returns" becomes established. The more obvious interpretations were made immediately, and what remains is obscure and likely to continue to defy satisfactory interpretation. The situation will be changed only when substantial new finds of tablets appear.


\textsuperscript{37} Cambridge 1956.


\textsuperscript{39} Edizioni dell' Ateneo, Rome 1962.

\textsuperscript{40} Edizioni dell' Ateneo, Rome 1965.

\textsuperscript{41} Vandenhoeck and Ruprecht, Göttingen 1966.

\textsuperscript{42} \textit{Studia Mycenaeara} p. 154.
are made, and that this is not excluded, is shown by the recent discoveries of tablets and fragments of tablets at Thebes and Tiryns.

Much attention has been paid, and is still being paid, to problems of Mycenaean phonology, morphology, word-formation and etymology. It would be invidious to single out for special mention any one or two contributions of scholars in this field; some idea of the interest in this side of Mycenaean studies can be obtained if one looks at the index to the proceedings of the Salamanca Colloquium. Well over half of Volume II of the proceedings is devoted to linguistic studies. Perhaps I may be allowed to mention one important work here, *Études sur le grec mycéenien*, by C. J. Ruijgh (Amsterdam 1967). This volume, comprising 439 pages, gives, after a résumé of the nature of the Mycenaean script and dialect, a detailed discussion of various means of word formation, and of some of the more important particles of Mycenaean, such as *-qe* and *-de*. Good news in this respect, too, is the announcement of the appearance of the second volume of M. Lejeune’s *Mémoires de Philologie Mycénienne*, 1958–1963, which has long been awaited. A further report of a work by Lejeune has just reached me: *Phonétique historique du mycéenien et du grec ancien* (Paris 1972). It is based upon his *Traité de phonétique grec*; his statement on Mycenaean phonology will be useful.

The importance of the decipherment of Linear B for the study of the Greek dialects can hardly be overestimated; much has already been written on the subject, particularly on the dialect pattern of the second millennium BC in the light of the new evidence provided by the decipherment and on the relationship of the Mycenaean dialect to the Greek dialects of the Classical period.43 Of particular importance in this regard is the questionnaire circulated to various scholars at the Mycenaean symposium held at Brno in April 1966. The answers of 17 scholars to the questionnaire, together with a summary by Professor E. Risch, are published in *Studia Mycenaea*, the proceedings of this Symposium.44 At the Salamanca Colloquium Professor A. Bartonek of the University of Brno analysed the answers and gave some of his conclusions on the matter.45 The first set of questions is concerned with the pre-history of the Greek language; Professor Pisani’s theory that Greek arose by convergence from a number of I.E. languages was discounted by all the scholars; but opinions were divided on the question whether any of the differences which characterized the dialects of the Classical period had been in existence before the Greeks entered the


Balkan peninsula. Cowgill, in my opinion, gives the most satisfactory answer here: "On theoretical grounds it is most unlikely that the speech of the invaders who brought the Greek language to Greece was absolutely homogeneous. But I do not see any reason to suppose that any of the divergences between Greek dialects as we know them must have existed already before the invasion(s)."\(^{46}\)

Closely allied to this question is the question whether the traditional view about several waves of Greek newcomers still seems plausible, and it is here that scholars took diametrically opposed views. Some, e.g. Grinbaum, Ruijgh, Tronskij, support the theory of separate waves of invasion, each wave bringing with it the forerunner of one of the dialect groups of the Classical period, while at the other end of the scale we have Chadwick, with his theory of one invasion at the beginning of the 2nd millennium BC, and the so-called Dorian invasion a secondary shift southwards from c. the 13th century BC of part of the invaders who had remained in the NW of Greece. This view seems to me the most feasible, and one which is most supported by archaeological evidence.\(^{47}\)

The second and third sections of the questionnaire are concerned mainly with problems of Mycenaean Greek, and the subsequent development of the Greek language, and cover such problems as the dialect pattern of the 2nd millennium BC, the possible continuation of Mycenaean in any of the 1st millennium dialects, and the possible existence of a Mycenaean koine. Though some scholars, e.g. Ruijgh and Tronskij, assume that the prototypes of the classical dialects already existed in the 2nd millennium, more scholars tend to the view that the dialect pattern in the 2nd millennium was a much less complicated one, and assume two or possibly three dialect groupings: Mycenaean, Aeolic and the forerunner of Doric. The position of Aeolic at this stage holds most problems; it was hoped that the newly discovered Thebes tablets might throw some light on this matter, but nothing of note has emerged from the already published tablets, certainly not the hoped-for evidence that the dialect of Thebes in the 2nd millennium was a ti-dialect, not a si-dialect, like the Mycenaean of the Peloponnesian. As to the possible continuation of Mycenaean in any of the dialects of the Classical period, while agreeing that of these dialects Arcado-Cypriot shows most affinity with Mycenaean, most scholars are of the opinion that Mycenaean has no direct descendants in the 1st millennium. Here Professor Risch's theory of a 'Mycénien normal' (the language of the tablets, a specialised language used for the specific purpose of keeping the state records, and therefore fairly homogeneous in the different centres), and 'Mycénien special' (the ordinary spoken language of the people) has received considerable support. 'Mycénien normal', being closely tied to the organisation of the palaces, would have disappeared after the fall of the palaces, while 'Mycénien special' would have continued to develop.\(^{48}\) This is perhaps just another way of

\(^{46}\) *Studia Mycenaea* p. 163.

\(^{47}\) See also the views of Cowgill and Crossland, *Studia Mycenaea* pp. 166, 167.

speaking of a Mycenaean *koine*, if one understands by the term not a mixture of dialects, but the development of one dialect into a kind of *lingua franca*, a kind of *supradialect*, in this case the court language of the palace records. This is Bartonek’s theory, and it has much to recommend it. This seems as far as we are likely to get for the present in the dialect studies of the 2nd millennium; the discovery of more tablets, particularly at Thebes and other parts of Boeotia, might throw further light on the subject; further gains can also be made by continued application of modern methods of dialect geography, as Risch advocates.

I mentioned earlier that in the interpretation of Mycenaean vocabulary we seem to have reached the stage where this side of the field has been exploited almost to the full. The future emphasis in the interpretation of the Mycenaean texts will lie mainly on the study and analysis of the texts as documents relating to the social and economic organisation of the palaces. Scholars are helped in their interpretation of the material by the work which has been done on the identification of scribal ‘hands’ in the different palaces. In this way it has been possible to re-group some of the tablets into smaller sets on the basis of handwriting. Attention has also been paid to the fingerprints of the scribes, but without any conclusive results. Important work on the identification of the ‘hands’ has been done on the Pylos tablets by E. L. Bennett and on the Knossos tablets by J.-P. Olivier. This new re-grouping of the tablets according to ‘hand’ has not yet caused scholars to abandon the admirable system of classification into series devised by Bennett before the decipherment on the basis of ideograms and recurring word groups, but certainly greater precision has been made possible. I think, e.g., of the two tablets formerly classified as PY Sn 64 and An 218, which are now seen to to form a pair, and have been re-classified as Aq 64 and 218. Sometimes, as in the case of the Pylos Ta tablets, a ‘set’ is also a ‘series’. Much valuable work has already been done on various series and sets, and from them much information has been gleaned on the economic organisation of the palaces.

I think in particular of the work done by Dr. J. T. Killen of Cambridge on the sheep and wool tablets of Knossos. Ventris and Chadwick had already noted the preponderance of rams over ewes in the KN D-series, and had interpreted the texts as records of tribute to the palaces. But Killen’s work on the tablets, which is based on a thorough analysis of the texts concerned, and on

49. Bartonek, *C.C.M.S.* pp. 95–103.
55. *Documents* pp. 19ff.
a comparison of these records with mediaeval English wool records, has conclusively proved that in the KN D-series we have records of sheep breeding and wool production. He has interpreted the 'rams' on the tablets as 'wethers', which are known from other sources to be the best wool producers. There have been several other studies of series or sets of tablets, but much work remains to be done in the interpretation of the tablets as economic documents. There have been several studies made of the land tenure tablets at Pylos (E-series) but many problems still remain because of the uncertainty of the terminology. A full study of the tablets concerning women and children at Pylos and Knossos is being prepared by Chadwick and Killen, and when it appears, it will throw much light on the organisation of the labour force in these palaces.

There have been many developments in the field of archaeology and chronology in recent years. These are subjects in which I can hardly claim to be an expert, and so I shall merely indicate some of the more important developments. First there is the research done on the clay of the Theban vases mentioned above, p. 8. Then there is the controversy raised by Professor L. R. Palmer on the date of the fall of Knossos. Disturbed by the discrepancy in dating between the traditional date for the fall of Knossos (c. 1400 BC or just after) and the destruction of Pylos and Mycenae (c. 1200 BC), and by the similarity of orthography and spelling conventions of the records from these palaces, Professor Palmer raised the question first in a number of newspaper articles whether these dates did not in fact need revision. He claimed to have found evidence in some of the unpublished note-books of Sir Arthur Evans and Duncan MacKenzie that Evans had misrepresented his report of the dig, and that this evidence made it necessary to revise the date of the fall of Knossos to a date after the destruction of the Mainland palaces. He claims, too, to have found linguistic evidence in the tablets themselves, and believes that certain forms at Knossos are later than corresponding forms at Pylos. This is a subject which would require many hours for adequate discussion; suffice it to say that the main weight of archaeological opinion is still on the side of the earlier date for the fall of Knossos, i.e. in the first quarter of the 13th century BC. I heard that at a recent seminar in London, Sinclair Hood advanced arguments for an even earlier date for the fall of Knossos, i.e. c. 1450 BC.

One of the most important developments in the archaeological field in recent years is the research done on the clay of Theban vases mentioned above, p. 8. Then there is the controversy raised by Professor L. R. Palmer on the date of the fall of Knossos. Disturbed by the discrepancy in dating between the traditional date for the fall of Knossos (c. 1400 BC or just after) and the destruction of Pylos and Mycenae (c. 1200 BC), and by the similarity of orthography and spelling conventions of the records from these palaces, Professor Palmer raised the question first in a number of newspaper articles whether these dates did not in fact need revision. He claimed to have found evidence in some of the unpublished note-books of Sir Arthur Evans and Duncan MacKenzie that Evans had misrepresented his report of the dig, and that this evidence made it necessary to revise the date of the fall of Knossos to a date after the destruction of the Mainland palaces. He claims, too, to have found linguistic evidence in the tablets themselves, and believes that certain forms at Knossos are later than corresponding forms at Pylos. This is a subject which would require many hours for adequate discussion; suffice it to say that the main weight of archaeological opinion is still on the side of the earlier date for the fall of Knossos, i.e. in the first quarter of the 13th century BC. I heard that at a recent seminar in London, Sinclair Hood advanced arguments for an even earlier date for the fall of Knossos, i.e. c. 1450 BC.

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56. For details of studies of this kind, see the tablet index in Studies in Mycenaean Inscriptions and Dialect (see above, note 35).
58. e.g. John Boardman, The Date of the Knossos Tablets (in Palmer and Boardman, On the Knossos Tablets pp. i-xi + 1-101, 17 plates); Hood, London Mycenaean Seminar 1.xi.1972, (per litteras J. Chadwick); see also Sourvinou-Inwood, Minos 13:1 (1972) pp. 88-89 for further literature on the subject.
years has been the excavation by Professor Spyridon Marinatos\(^59\) of a Minoan city under the pumice and volcanic ash on the island of Santorini, ancient Thera. One of the important finds is a vase with a Linear A inscription. The excavation of Thera in itself would be the subject of a full paper; but I must content myself with the main points. I leave to one side the speculations in the Sunday press and in popular magazines that at last we have proof for the truth of the Atlantis myth; the other question raised here is more important; whether the explosion of Thera in the LM IA period can be held responsible for the destruction of most of the centres in E. Crete in the period immediately following. That Crete, about 80 kilometres away from Thera, would have felt the results of the explosion, is only to be expected; and it is tempting to ascribe the destruction of most of the centres in E. Crete to this cause. But there is a chronological difficulty. The explosion of Thera is dated to LM IA from pottery finds in the buried city; the destruction of most of the centres in E. Crete is dated to LM IB, also by pottery finds. Much work remains to be done here; for the present, we can point to the strong possibility that most of E. Crete was covered by a heavy blanket of volcanic ash from the explosion of Thera; the researches of Ninkovich and Heezen\(^60\) who extracted cores from the seabed round Crete and discovered in them a layer of volcanic ash which is probably from the Thera explosion, give support to this. If, as seems likely, grazing land is the first to recover after a country has been covered by volcanic ash, the records of sheep and wool from Knossos would tie in well with this, and may show that E. Crete was put under sheep when first the land began to recover from the results of the explosion. Of course, I am over-simplifying here; but I do believe that the excavations on Thera, together with the results mentioned earlier of the analysis of clay in Theban vases, may in the next few years cause us to review our ideas on the chronology of Crete.\(^61\)


61. In this survey, I have left a number of important points out of account. One of these is the importance of the decipherment for Homeric Studies, a subject too vast to be considered here. Another is the work done in the field of the Cretan pictographic scripts, Linear A, the Phaistos Disc and Cypro-Minoan. It is an area in which I cannot claim to be an expert, and I must leave any discussion on this part of the field to others.
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