RECENSIONES

THOMAS L. MARKEY & JOHN A. C. GREPPIN, eds.: *When Worlds Collide: The Indo-Europeans and the Pre-Indo-Europeans,* Karoma, Ann Arbor 1990, pp. 401. $65.00.

This volume contains 22 articles presented at the Bellagio conference of February 8-13, 1988 on archaeological, linguistic, and general methodological problems regarding the Indo-Europeans, their homeland, and their contacts with other ethnic or lingustic groups. In many respects, the volume responds to the question in Marija Gimbutas' Festschrift: Can linguists and archaeologists mate (Hawkes 1987)? Many papers were direct reactions to Renfrew (1987). A transcription of some discussions is included. Unfortunately, due to technical difficulties, much of the discussion was lost. In this review I will cover those articles which deal with subjects generally found in *Minos*: specifically topics connected with Greek language and the early Greeks. My own interests are linguistic. To do justice to all papers in this volume would require a reviewer with a much wider expertise.

In «On the Problem of an Asiatic Original Homeland of the Proto-Indo-Europeans», Thomas Gamkrelidze restates his basic arguments of 1984 and earlier. G. uses primarily linguistic data, lexical and structural, to assign the IE homeland to Eastern Anatolia. His article is disappointing in a number of respects. He offers next to no bibliography and does not respond to criticisms of his views or to other views. For example, he suggests that the homeland north of the Black Sea as proposed by Gimbutas is correct as a later area of settlement of Western IE's, yet he makes no mention of David Anthony's painstaking work on the origin of equine domestication which makes the Sredni Stog culture so interesting as a candidate for the original Homeland. G. speaks of «reconstructed fragments of texts» (p. 9) that link the reconstructed culture and mythology typologically to Middle Eastern material. I assume G. is speaking of the various phrases and collocations such as κλέος ἄφθιτον and 'to kill a dragon' (Cf. Watkins 1989 with references). There are two major problems, however, with this approach. First, the occurrence of etymologically similar phrases for common concepts in languages which are related (e.g. 'undying fame', 'killing a beast with something') is not necessarily proof of a common heritage. If an English novelist speaks of «young love» and a German of «junge Liebe», one might reconstruct a Germanic prose formula, but there is no real continuity. Second, the typological commonalities with the Middle East are only valid, if a general framework of cultural typology has been set up. This is not yet the case. As in studies in typological linguistics, types are only valid if their implicational structures are a) non-universal and b) non-unique. That is to say, if all cultures exhibit a feature, then the fact of possession of the feature is trivial. If a feature occurs in arbitrary isolation, one may not use it to set up taxonomies of typologically similar languages. There may be borrowing, but this must be demonstrated by concrete formal identity of elements. G.'s structural data (linguistic) is problematic for the same reason. Although the Glottalic Theory does produce a system of IE obstruents similar to Proto-Semitic and Proto-Kartvelian, the patterns of borrowing between the languages are much less clear than Gamkrelidze makes out (cf. Diakonoff, Harris in this volume). Typological similarity of language need not be a criterion for genetic *or* areal relatedness. It is also hard to accept, at least as the case is argued by G., that the Anatolian languages are particularly archaic because of their minor displacement from the Eastern Anatolian homeland. This runs contrary to how languages are known to change. If Anatolian had been in Anatolia for centuries, in close contact with the Mid-Eastern cultures, one would expect it to have been innovative. Connection to some «Urheimat» is in itself no explanation for archaicity of language (cf. Norway and Iceland, where the «Homeland» of Norway is considerably more innovative than Iceland). G.'s suggestion that the Greeks came to Greece from the East is similar to that of Drews (1989), and raises similar questions. How does one account for the distribution of Greek dialects and the fact that the Greeks clearly displaced a non-Greek population shortly before historic (i.e. Linear B) times? If the Greeks were in Anatolia for such a long period, why was their language so different in structure from Anatolian? G. also acceps a recent identification of the 3rd millennium B.C. *Tukres* with the much later Tocharians. The problem of proper names of peoples is extremely complex and the identification of the two peoples on this basis is very tenuous, especially when so little is known about the prehistory of the Tocharians (cf. Zimmer, this volume, p. 319).

Colin Renfrew's article «Archaeology and Linguistics: Some Preliminary Issues» re-examines some of the premises of his 1987 book and deals with criticisms of it. R.'s willingness to deal carefully with criticism of his work is laudable, especially in a scholarly world where such debate it not as common as it ought to be. He clearly discusses 5 topics: a. the autonomy of the linguistic data; b. the need for corresponding historical reality; c. the problem of chronology in the linguistic field; d. the appropriate use of archaeology to aid in the reconstruction of the historical reality; e. the risk that the products of research in the field of mythology may come into conflict with the proposed «historical reality», and ways to resolve this conflict (p. 15).

Under point a, R. defends the use of the Stammbaum because it is the «dominant existing model»; it implies an Ursprache and an Urheimat. He further reacts to Baldi's criticism of his (R.'s) use of convergence theory, and suggests that an outright rejection of it would be too limiting. R. calls for the development of other models of analysis, for example, by using the Wellentheorie. R. seems to confuse the fact that these two approaches describe the same process from different viewpoints. Further, he doesn't realize that the Stammbaum theory and the comparative method are only procedures of metalinguistic formalism, created to describe a proto-language which cannot be located in space or time (cf. Pulgram 1959, 1961). Linguists are quite correct in insisting that this model not be taken literally as the way in which an actual IE developed. The task of the IE linguist is rather to determine the path from his reconstructed forms and patterns to a possible reality. But the reconstruction by comparative and internal methods must come first.

Under point b, Renfrew points out that linguistic paleontology of the spread of IE has until the present been based on overly simplistic views of migrationism. As Anthony (1990, NEH Summer Institute *Perspectives on the Ancient Indo-European World* held at the University of Texas at Austin) has pointed out, the application of theories of migration by archaeologists is just beginning. Certainly the linguist must also take them into account. Renfrew claims that we «are entitled» to have reconstructions stated in coherent historical terms. However, a proto-language need not be situated in time or space. We are not «entitled» to something which is methodologically impossible. R. also rejects the whole set of Dumézilian proposals because they are not located in time and space. To this I would add that although there are certainly problems in the approach (e.g. lack of sufficient typological comparative data for mythology and culture), the same stipulation that applies to proto-languages methodologically must apply to other such reconstructions so that R.'s criticism is irrelevant.

Under point c, R. deals with one of the major criticisms of his theory on the spread of IE, the problem of his chronology. R. has been criticized because his date for the dispersal of IE seems too early for the languages to display the close similarities they have. R. responds by citing the general rejection of glottochronological dating, suggesting that this rejection also implies a rejection of establishing standard rates of change for languages. But, while linguists have quite properly rejected the model of mathematically determinable rates of change (corresponding to radio-carbon decay), they do recognize that languages change; and they certainly change faster than R.'s theory would allow. To use a concrete example, under R.'s chronology, the Greeks came to Greece ca. 6500 B.C., bringing agriculture with them. In Greece, they coexisted with various non-Greek elements for some 5000 years before the first texts in Linear B (ca. 1400 B.C.). In all this time, the Greek language was unaffected by the other language group (or groups) except in adopting a few vocabulary items. Suddenly, as soon as texts set in, Greek began to change more rapidly than at any time previously; the labio-velars were lost, pre-Greek languages died out, dialects lost prevocalic /w/ and /h/, etc. etc. It boggles the mind that such a period of stability would be replaced by change coincident with the first real chance to check on change. The same criticism applies to other language areas and remains one of the most telling arguments against Renfrew.

Under point d, R. restates the fact that a postulated spread of language could be explained by different models, e.g., subsistence/demography, elite dominance, system collapse. R. says that the job of the archaeologist is to work within these and other models, looking for evidence for and against the model, asking the question: «What sort of material remains can lead to the acceptance of one model over another?». R. admits that his own model of advancement of IE with agriculture suffers from the drawback of not explaining well enough the situation in NW Europe, nor the eventual distributions of IE dialects. However, he feels that no other model for the spread of IE accounts for the data in a satisfactory manner. To this I would only add the question, whether it is appropriate to suggest models that are weak, rather than to admit that we have as yet insufficient data to come up with better ones.

In his final point e, R. criticizes Gimbutas' theory of a pacifistic, artistic, non-IE Old European culture which was matrilinear, matrilocal, worshipping a Mother goddess. R. correctly points out that it is impossible to claim a non-IE status for this culture. He brings up the example of the Minoan and Mycenaean similarities in religious iconography as well as the massive iconographical changes associated with the later adoption of Christianity without there being a concomitant change in language.

Hamp's article on the horse in IE looks both at the etymology of *$\acute{k}$ *eḱ*^{w}*os* and its occurrence in Homer, reconstructing a bit of poetic language. He reconstructs the words from an IE adjective **ōḱ-* 'swift, celer, uelox' < **f*^{w}*e?ḱ-* ('*f*^{w}' = H_2). He then gives the results of a computer generated collection of all occurrences of ἱππ- and ὠκ- in Homer and a short schematic diagram showing the underlying pattern. In closing, he reconstructs an IE version of Homer *Il.* 16.865. I will not go into his linguistics except to point out that his dating of PIE as around 6000 B.C. (at the time of the domestication of the horse) is most probably exaggerated (cf. Zimmer 1988).

Hamp's article is typographically very messy. References are made to works with no listing of bibliographic information. Some sentences make no sense, even after repeated readings. His transformational diagram and schematics of the Homeric passages are so poorly labelled and explained that they make little sense. I also fail to see the use of his eight-page listing of all the Homeric passages, since he does so little with the texts. The phonemic transcriptions are unclear: he seems to reconstruct an /f^{w}/ (i.e. labio-dental fricative with labialization) where what is meant is /ʕ^{w}/.

Zvelibil & Zvelibil («Agricultural Transition, 'Indo-European Origins' and the Spread of Farming») present a response to Renfrew (1987) and an «elaborated» revision of a 1988 paper on the same subject. After briefly summarizing Renfrew's main points, the authors judge him to have fallen short in trying to prove that the first farmers were Indo-Europeans. They remind us that Indo-European is only a construct, as are archaeological cultures, so that comparison is very difficult. Renfrew's two key assumptions are: 1) the widespread distribution of IE languages in Neolithic Europe; 2) the spread of the IE's as a single, continuous process. Z. and Z. point out that in many parts of Europe, non-Indo-European languages were spoken for a long time (whereby the authors' labelling of Pictish and Messapic as non-Indo-European is probably erroneous). Renfrew should be modified to allow primary and secondary dispersals. Although instances may be cited where a small group of people had its language adopted over a wide area, the technological superiority Renfrew claims for IE would not have been large enough to motivate the language's adoption. Archaeologically, there is «serious» doubt as to the validity of Renfrew's simple motivation model. The «wave of advance» does not apply in most of Europe and in contrast to his claims we see: 1) that there is a clear continuity between the Mesolithic and Neolithic; 2) that changes measured by physical anthropology are caused by change in diet, not immigration; 3) that the rate of change to farming economies is slow and complex. Data for the reconstruction of eating habits are skewed because wild animal bones tend to be poorly preserved. If native peoples took on farming and the colonizing farmers also used wild resources, then they would have had approximately equal reproductive potential. The «wave of advance» model is only plausible for Southeast and Central Europe where there are archaeologically recoverable changes in culture simultaneous to the introduction of agriculture. Z. and Z. speak of IE spreading to the east and superseding the Dravidian Harappan civilization. Although they admit some uncertainty as to the linguistic circumstances of these people, they believe there is a good case to be made for them being part of an Elamo-Dravidian grouping. However, until longer texts or bilinguals are found, more caution should be advisable when discussing Harappan.

Z. and Z. propose several modifications of Renfrew. The IE language spread was punctuated, multi-staged, and repetitive. There was no widespread displacement of populations. The IE languages spread at different times for different reasons.

They see three stages of European dispersal. 1. Agro-pastoral farming comes to Europe from the Near East via Anatolia (6500-5000 B.C.); the spread is confined to the Linear Bandkeramik culture in Central Europe, Tripolye, Balkan Neolithic, and some of South Italy. 2. This phase is followed by the consolidation of farming and «Secondary Products Revolution» (4800-2500 B.C.) with the expansion of farming to secondary dispersal areas and use of IE as a trade language, gradually replacing other languages. A reference (p. 253) to glottochronology «without wishing to get involved in the debate» ignores the fact that this method is discredited and should not be adduced. 3. Elite dominance stage (3800-ca. 1000 B.C.) where a group of IE's emanates from between the Dnieper and Urals, from the northern shore of the Caspian Sea. This is a group of horse-riding, nomadic pastoralists who reach the marginal zones of Eastern Europe and Central Asia (p. 253 read «of Gimbutas» for «of the Gimbutas»). In the East, the Āryans reach India ca. 2000 B.C. and help cause the fall of the Harappan civilization.

Polomé («Types of Linguistic Evidence for Early Contact: Indo-Europeans and Non-Indo-Europeans»), Hamp («The Pre-Indo-European Language of Northern [Central] Europe»), Markey («Gift, Payment and Reward Revisited»), and Villar («Indo-Européens et Pré-Indo-Européens dans la Péninsule Ibérique») all look at the problem of linguistic substrata and their effects on Indo-European.

P. is concerned with the typological features associated with various contact situations and their application to the reconstruction of early IE contacts. He emphasizes the danger of trying to explain changes in language by adducing unpreserved substrates. In the case of IE, two possibilities exist for the lack of grammatical categories in, for example, Hittite and Germanic. Either they were never there (as P. believes), or they were lost. In contact situations, languages will either borrow «foreign» phonemes, or substitute native phonemes for the foreign. Clues to recognizing substrate words are the preservation of words in one language or language group, especially words for objects, animals, or plants native to the area or cultural and onomastic terms.

P. proposes several methodological considerations when invoking substrate influence, namely, no use of *ad hoc* reconstructions, careful determination of the emergence date of terms, motivation of their preservation or late occurrence, examination of the terms'geographic spread, care when dealing with «affective» vocabulary, no forcing words together at the cost of semantics.

P.'s discussion is pervaded by common sense. However, I would criticize the way he connects the structural patterns of Hittite and Germanic, because the commonality of structural patterns in the languages is irrelevant unless there is consistent *formal* agreement of these patterns.

Hamp discusses the definition and use of substrata. He emphasizes that substrata are only acceptable as explanations if there are consistent regularities in languages which do not fit into the pattern of reconstructible rules from the proto-language and yet are consistent within themselves, even across several languages. H. continues in this article his work of many years on the definition of a «North European» substrate which is also to be found as far south as Greece (i.e. = Prehellenic or Pelasgian). In this article, Hamp gives no new reference to Greek, but he has appended a lengthy listing of his articles dealing with the subject.

Markey discusses the connections of IE *$m^e/_oi$* 'gift/exchange', especially in Germanic and Northern Europe. He associates features of reconstructible pre-Germanic to non-IE elements and views them in the context of a composite grouping of Indo-Europeans and non-Indo-Europeans. M. summarizes briefly (pp. 357-358) a set of cultural features he ascribes to the pre-Indo-Europeans of Northern Europe and calls for more investigation of the archaeological record in items of these cultural reconstructions.

Villar provides an interesting short discussion of the substrate problem on the Iberian Penninsula where Celtic, Iberian, Lusitanian and other languages are intermixed. He appends illustrations of the Botoritta bronze and texts of it and other inscriptions. To this may now be added Eska (1989).

Zimmer («The Investigation of Proto-Indo-European History: Methods, Problems, Limitations») is perhaps the most refreshingly forthright article in this whole book. Z. has outlined reasons for scepticism about the reconstruction of proto-culture in a series of articles. His main point is that a proto-language is by definition an artificial construct which does not admit assignment to time and space. The problems of reconstruction of semantic structures are insurmountable so that any investigation of the homeland based on language internal criteria must fail. Even the reconstructed fragments of poetic language give little help in a holistic analysis of IE culture because they only represent a very restricted portion of the populace. Only a better understanding of the general typology of culture will ever allow a sensible and acceptable reconstruction of IE culture. Although some may find Z. too pessimistic, this article should be required reading for anyone who wants to reconstruct parts of a proto-culture.

I very much miss any sort of introduction or preface by the editors. Also, for a book costing over $60, the number of typographical errors is high. Fonts and formatting are widely divergent in the articles. Extravagant spacing has made much of the book longer than necessary. Several articles are inconsistent within themselves as to indentation of paragraphs and the type of font used for languages such as Greek. Diacritics are often missing or written in by hand. Hamp's first article is so messy typographically and stylistically that several points are difficult to follow or even completely obscured (e.g., his interpretation of H_2 is transcribed as *f^w*, although this is clearly far from his intention). Markey's article also contains numerous *p*'s for *þ*'s; on p. 354, line 23 read: Av. *miθō;* p. 347, line 30 read: *Þáttr;* p. 348, line 33 read: *gørsimi;* p. 353, line 45 read: *belohnen. Polomé* and *Dumézil* occur in several articles without accents.

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Voici le contenu de ce recueil de mélanges:

M. A. Jazayery, «Edgar C. Polomé: A Biographical Sketch» (pp. 7-11). Né en Belgique en 1920, E. C. Polomé a été professeur de linguistique à l'université du Congo Belge de 1956 à 1960 avant de couronner sa carrière à l'université de Texas à Austin.

H. Thomas, «Indo-European: from the Paleolithic to the Neolithic» (pp. 12-37). M. T. s'occupe du problème fort difficile de l'origine des Indo-européens: à quelle culture préhistorique et à quelle région peut-on associer la population de langue proto-indo-européenne? La question se pose, bien entendu, pour chacune des époques archéologiques successives: Paléolithique Supérieure, Mésolithique, Néolithique, Chalcolithique. M. T. énumère les cultures successives pour les différentes régions de l'Europe et du sud-ouest de l'Asie. Grâce au C 14 et aux données dendrochronologiques, on dispose actuellement de quelques datations absolues. — En ce qui concerne les Proto-Grecs, il est toujours tentant d'admettre qu'ils proviennent de la Russie méridionale (culture des kourganes) et qu'après avoir traversé la région des Balkans, ils se sont mis à pénétrer dans la Grèce vers le début du second millénaire av. J.-C. Cependant, M. T. souligne à juste titre qu'il n'y a pas de preuve archéologique décisive pour l'invasion des Balkans en question.

E. Lyle, «Markedness and Encompassment in Relation to Indo-European Cosmogony» (pp. 38-63). Mme L. attribue aux idées cosmogoniques des Proto-indo-européens une structure fondamentale comportant trois générations de dieux. Chez les Grecs, on trouve (1) Γῆ et Οὐρανός, (2) Κρόνος, (3) Ζεύς. Γῆ est la 'femelle primordiale' préexistante: elle est la mère d'Ouranos avant de devenir son épouse. C'est pourquoi Γῆ est l'élément 'englobant' de la cosmologie, l'élément 'non marqué' vis-à-vis d'Ouranos, qui est le mâle 'marqué'. — A vrai dire, nous n'avons pas réussi à comprendre la théorie de Mme L. Il nous paraît dangereux de projeter le mythe des trois générations divines qui figure dans la *Théogonie* d'Hésiode sur l'idéologie des Proto-indo-européens. Le mythe peut bien refléter la fusion de la religion 'patriarcale' des envahisseurs indo-européens, où Zeus le père, dieu du ciel, était la divinité suprême, avec la religion 'matriarcale' de la population préhellénique, où la déesse terre-mère jouait le rôle primordial. Noter la combinaison *di-we* (Διϝεί) ... *ma-qe̜* (Μᾶ̜ κʷε; ou bien *ma-ka̜* Μᾶ̜ Γᾶ̜) dans la tablette KN F 51; cf. PY Fr 1202 *ma-te-re te-i-ja* μᾶτρεὶ θεhίᾳ 'pour la mère des dieux'. Dans le cadre de cette fusion, il était facile de considérer Zeus comme fils de la déesse terre-mère, peut-être appelée Ῥέα en Crète. Noter que Κρόνος, époux de Ῥέα, était un dieu de la récolte, non pas un dieu du ciel, comme le croit Mme. L. Pour les Grecs, Ζεύς, Ῥέα et Κρόνος étaient déjà de simples noms propres. Le nom Γᾶ, en revanche, était identique à l'appellatif γᾶ 'terre'. C'est ce qui explique que dans le mythe cosmogonique, Hésiode a introduit Οὐρανός 'Ciel' comme fils-époux de Γῆ 'Terre'. Noter que le vieux terme épique οὐρανίωνες, désignant les dieux comme 'ceux qui sont au ciel', a pu suggérer, sous l'influence de l'emploi souvent patronymique du suffixe -ιων-, qu'Ouranos était le nom de l'ancêtre des dieux. C'est ainsi qu'on peut expliquer les trois générations chez Hésiode: la première relève de la cosmogonie, la seconde concerne les dieux préhelléniques, la troisième concerne Zeus, le dieu indo-européen nouveau venu.

V. N. Toporov, «Indo-European *eg'h-om (*He-g'h-om): *men-. 1 Sg. Pron. Pers. in the Light of Glossogenetics» (pp. 64-88). M. T. essaie de reconstruire la préhistoire du pronom indo-européen de la première personne du singulier en recourant à la théorie 'nostratique' (origine commune de l'indo-européen et des langues ouralo-altaïques), à la typologie linguistique et aux théories sur la genèse du langage humain. Ainsi, le mot qui a abouti à sanskrit *ahám* comporterait un élément déictique-démonstratif **H_1e-*, une particule emphatique *-g'h-* et une particule explétive 'vide' *-om*. Le mot *mene* (génitif en vieux slave) reposerait sur la racine qu'on trouve dans μέν-ος, μαν-ία, et qui désignerait l'activité mentale. La combinaison des deux mots serait à interpréter comme «*this-is-hereness + speak (think)*». — Nous avouons que nous n'avons pas bien compris la théorie de M. T., qui nous paraît fort spéculative. Noter que sanskrit *ahám* s'explique plutôt à partir de $^*H_1eg\text{-}H_2\text{-}\acute{o}m$ vis-à-vis de grec ἐγώ, forme qui peut s'expliquer à partir de $^*H_1eg\text{-}\acute{o}H_2$. L'élément $-H_2-$ a chance de se retrouver dans la désinence de la 1[re] pers. sing. du parfait (-α < - $^*H_2\text{-}e$, vis-à-vis de 3[e] pers. *-e*) et dans la désinence thématique -ω < $^*\text{-}o\text{-}H_2$). Si l'on veut admettre que le pronom qui désigne déictiquement 'celui qui parle' comporte la notion de 'parler', il serait tentant de supposer que l'élément $^*H_1eg-$ de ἐγ-ώ s'identifiait originellement à la racine verbale $^*H_1eg-$ qu'on trouve dans ἦ < *ἦκτ < $^*H_1\acute{e}\text{-}H_1eg\text{-}t$ 'il parlait'.

H. H. Hock, «On the Origin and Early Development of the Sacred Sanskrit Syllable OM» (pp. 89-110). M. H. admet que *óm* < **aum* était originellement une particule exclamative, qui accompagnait notamment des vocatifs et des impératifs, de la même façon que grec ὦ, ὤ.

G. A. Klimov, «The Kartvelian Analogue of Proto-Indo-European *swomb(h)o- 'spongy, porous'» (pp. 111-116). M. K. admet que le verbe géorgien **çumb-* 'tremper dans l'eau, imbiber' est un vieil emprunt indo-européen: cf. v.h. allemand *swamp* 'éponge' et grec σομφός 'spongieux, poreux'. — Il faut signaler que le traitement grec normal de **sw-* initial est du type **swe* > (ϝ)ἑ. Le maintien de *s-* qu'on trouve par exemple dans σῦς (myc. *su-qo-ta* συ-γ^{w}ώτᾱς), doublet de la forme attendue ὗς, pourrait s'expliquer en admettant qu'après le changement *s-* > *h-* du proto-grec, le grec du II[e] millénaire a emprunté de telles formes à *s-* conservé à une langue apparentée ('para-grecque' ou 'grécoïde'), qui doit avoir disparu avant le premier millénaire av. J.-C.

V. Shevoroshkin, «On Carian Language and Writing» (pp. 117-135). M. S. analyse l'écriture et la langue des inscriptions cariennes. Il croit que les Cariens ont directement emprunté à des Sémites un système d'écriture alphabétique fort archaïque. Il distingue cinq variantes de l'alphabet carien. Il admet un lien étroit entre le carien et le groupe louvite-lycien parmi les langues anatoliennes.

F. Villar, «The Numeral 'Two' and its Number Marking» (pp. 136-154). M. V. croit que **duoi* était originellement une forme du pluriel, comparable à des formes pronominales comme τοί. Après la création de la catégorie morphologique du duel, **duoi* en est venu à servir de nom.-acc. *neutre* vis-à-vis de masculin **duō(u)*. D'autre part, la forme **duoi* entière pouvait être considérée comme un thème, ce qui explique la forme **dui-* (degré zéro) figurant au premier membre de composés. Dans cette fonction, **dui-* remplace la forme plus ancienne *du-* attestée dans latin *du-plex,* etc. Il n'est donc pas nécessaire d'attribuer la voyelle *i* de **dui-* à l'influence de *tri-*. — En principe, l'explication de M.V. nous paraît acceptable. Ajoutons que la forme la plus ancienne *du-* doit être le degré zéro de **déw*: comparer **sm̥-* dans latin *sim-plex,* grec ἁ-πλοῦς, comme degré zéro de **sém*. En effet, tous les cardinaux simples ont le degré *e*: **sém, *déw, *tréy,* $^*k^w\acute{e}t(w)\bar{o}r$, $^*p\acute{e}n\text{-}k^we$, **swéks, *septm̥,* $^*H_3ekt-$, $^*H_1n\acute{e}wn̥$, **dékm̥.* Noter que l'emploi de *-i* comme morphème du duel neutre se retrouve dans ϝίκατι < **dwi-dkm̥t-i* 'deux dizaines', vis-à-vis de τριάκοντα < $^*triH_2\text{-}dkomt\text{-}H_2$ 'trois dizaines' avec $^*\text{-}H_2$ > -α comme morphème du pluriel neutre. Vis-à-vis du duel neutre, le duel animé a le morphème $^*\text{-}H_1$ (cf. πόδ-ε < $^*p\acute{o}d\text{-}H_1$ 'paire de pieds'): $^*dw\acute{o}H_1$ > *δϝώ (dans δώ-δεκα) avec la variante dissyllabique δύω (loi de Lindeman). A notre avis, le thème **dwoy-* est également à la base de l'adjectif **dwoy-ó-* 'double' (sanskrit *dvayá-*), que le grec a remplacé par **dwoy-yó-* > δ(ϝ)οιό-ς (cf. l'anthroponyme mycénien *dwo-jo / du-wo-jo*). Sous ce rapport, il faut signaler que l'adjectif δίδυμος 'double, jumeau', attesté en mycénien par l'anthroponyme *di-du-mo,* comporte *di-* et non pas *dwi-*. Cela s'explique en admettant qu'il s'agit d'une forme à redoublement ayant une valeur 'iconique'. En effet, **di-du-mó-* est comparable à **di-déH₃-mi* (δί-δω-μι), forme où la voyelle *i* du redoublement est issue d'une voyelle d'appui en syllabe ouverte. Or, il était facile de réinterpréter *di-* dans δί-δυμος comme un doublet de **dwi-*; comparer ἀμφί-δυμος. Cela explique des adverbes ou préverbes comme grec διά < *δισ-ά et latin *dis-*, dont la valeur originelle était 'en deux'. Comparer l'adverbe plus récent δίχα 'en deux' avec l'adjectif dérivé *διχyός > δισσός 'double', peut-être attesté dans l'anthroponyme mycénien *di-so* ou *di-zo*. Enfin, la forme d'instrumental mycénienne *du-wo-u-pi* δϝου-φί ou δϝό*h*υ-φι a chance d'avoir été fondée sur un nom neutre en *-u-* (type γόν-υ): soit **dwoH₁-u-*> δϝου- soit **dwoy-u-* > δϝο*h*υ- 'paire'.

O. Carruba, «Searching for Woman in Anatolian and Indo-European» (pp. 155-181). M.C. essaie de retrouver les noms signifiant 'femme' dans les langues anatoliennes. Il s'agit notamment de noms bâtis sur *ser-* (cf. latin *soror* < *swé-sōr*) et sur **gʷen-* (cf. γυνή). M.C. pense que *ser-* est issu de **Hser-* et que le théonyme Ἥρᾱ (myc. *e-ra*) pourrait être issu de anatolien **Hsar-*. Nous préférons considérer Ἥρᾱ comme pendant féminin du nom préhellénique ἥρω-ς 'seigneur, maître'. M.C. croit que la forme grecque γυνή invite à partir d'un thème **gʷu-en-* ou **gu-en-* et rejette **gʷen-* comme forme originelle. Il pense, entre autres, à la racine *geu-* 'courber', qui pourrait se rapporter aux organes génitaux de la femme. A notre avis, la voyelle υ de γυνή (cf. l'adjectif mycénien *ku-na-ja* γυναίᾱ) peut bien être issue d'une voyelle d'appui dont le timbre répondait à l'articulation de la labiovélaire; comparer κύκλος 'roue' (cf. l'anthroponyme myc. *ku-ke-re-u* Κυκλεύς), issu de **kʷukʷló-* (thème à redoublement). Noter que béotien βανᾱ́ est l'aboutissement de *γʷανᾱ́ < **gʷn̥nā́* < **gʷnā́* (loi de Lindeman).

H. Craig Melchert, «Death and the Hittite King» (pp. 182-188). M.M. examine la section finale du 'testament de Hattusilis I'. Il admet que les cérémonies funèbres conservent des idées proto-indo-européennes sur la mort et l'au-delà.

J. Weitenberg, «The Meaning of the Expression 'To Become a Wolf' in Hittite» (pp. 189-198). M.W. montre que l'expression hittite qui se traduit littéralement par 'devenir un loup' signifie pratiquement 'être privé de ses droits'. Cette valeur ne remonte pas au proto-indo-européen.

P. Swiggers, «The Indo-European Origin of the Greek Meters: Antoine Meillet's Views and their Reception by Emile Benveniste and Nikolai Trubetzkoy» (pp. 199-205). M.S. publie la lettre de Benveniste et celle de Trubetzkoy écrites à Meillet à propos de la parution de son livre *Les origines indo-européennes des mètres grecs* (1923). Les deux réactions étaient fort positives. — A l'époque actuelle, on admet généralement avec Meillet que les vers éoliens et les vers iambiques ou trochaïques de la poésie grecque remontent au type des vers isosyllabiques de la tradition indo-européenne. D'après Meillet, le vers épique doit en revanche avoir été emprunté au monde égéen: la structure de l'hexamètre dactylique repose essentiellement sur le principe de l'isochronie, puisque le dactyle trisyllabique peut être remplacé par le spondée dissyllabique. Plusieurs savants de notre époque se sont efforcés de rattacher néanmoins le vers épique lui aussi au vers isosyllabique indo-européen. A notre avis, les données linguistiques des textes mycéniens prouvent qu'ici encore, Meillet avait raison. Plusieurs formules remontant à l'époque mycénienne montrent déjà l'équivalence du dactyle et du spondée. Ainsi, Ἀχιλλῆος θείοιο recouvre myc. *Ἀχιλλῆϝος θε*h*ίοιο (⏑–––⏑⏑––), tandis que Ἡρακλῆος θείοιο recouvre myc. Ἡρακλέϝε*h*ος θε*h*ίοιο (––⏑⏑–⏑⏑––). Le vers formulaire Μηριόνης ἀτάλαντος Ἐνυαλίῳ ἀνδρειφόντη doit recouvrir *Μηριόνᾱς *h*ατάλαντος Ἐνῡαλίω*y* ἀν*r̥*χ[w]όντᾱ*y*, vers holodactylique qui sous sa forme originelle comportait la liquide syllabique *r̥*. Or, les textes mycéniens les plus anciens, ceux de la salle des *chariot tablets*, attestent déjà le traitement ρο ou ορ pour *r̥* (V 280 *to-pe-za* τόρπεζα) et le traitement -νδρ- pour *-nr-* (Sc 246 *qe-ra-di-ri-jo* K[w]ηλάνδριος). Il faut conclure que l'hexamètre dactylique existait déjà à l'époque proto-mycénienne, antérieure à celle des tablettes. Dans ces conditions, il est tentant de conclure que les Grecs mycéniens ont emprunté le vers héroïque aux Crétois minoens.

K. R. Norman, «'As Rare as Fig-Flowers'» (pp. 216-220). M.N. examine des textes indiens qui contiennent l'expression proverbiale 'aussi rare que les fleurs du figuier'. Elle s'explique du fait que l'inflorescence du figuier n'est normalement pas visible.

G. Jucquois, «Règles d'échange, voeux monastiques et tripartition fonctionnelle» (pp. 221-243). M.J. s'inspire des idées de Lévi-Strauss, qui conçoit toute forme de vie sociale sous l'angle de la communication et de l'échange. D'après M.J., le Moyen Âge occidental maintenait la tripartition fonctionnelle de l'héritage indo-européen (prêtres, guerriers, travailleurs: les trois classes d'après la théorie de Dumézil). C'est dans ce cadre qu'il analyse les voeux monastiques. Il conclut que les trois voeux de religion semblent instituer en règles de vie communautaire l'inverse de ce qui, dans la vie habituelle, fonde la société elle-même.

W. Meid, «Ethnos und Sprache» (pp. 244-253). M.M. traite la question de savoir dans quelle mesure l'emploi d'une même langue est l'élément primordial de la conscience ethnique. Il distingue six éléments constituant la solidarité ethnique: descendance commune, langue héritée, tradition littéraire liée à cette langue, moeurs et coutumes, religion, territoire. Souvent, tel ou tel élément fait défaut.

A vrai dire, la lecture de ce recueil assez hétéroclite nous a déçu. Plusieurs contribuants s'adonnent à des spéculations peu fondées et dépourvues d'une argumentation précise. La typographie de certaines contributions est mal soignée. Nous avons relevé surtout les points concernant le mycénien ou le grec en général pour les lecteurs de *Minos*.

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YVES DUHOUX, THOMAS G. PALAIMA and JOHN BENNET, eds.: *Problems in Decipherment* (BCILL 49), Louvain-la-Neuve, Peeters 1989, pp. 216. 650 BF.

This volume contains papers from the 1988 Burdick-Vary Conference in Madison, Wisconsin honoring Emmett L. Bennett, Jr. The six contributions to the volume fall into three thematic categories: historiography (Emmett L. Bennett, Jr.: *Michael Ventris and the Pelasgian Solution;* Maurice Pope: *Ventris' Decipherment - First Causes*); current problems in decipherment of Aegean scripts (Jean-Pierre Olivier: *The Possible Methods in Deciphering the Pictographic Cretan Script;* Yves Duhoux: *Le linéaire A: problèmes de déchiffrement;* Thomas G. Palaima: *Cypro-Minoan Scripts: Problems of Historical Context*); and Etruscan (Giuliano Bonfante & Larissa Bonfante: *«Deciphering» Etruscan*).

Bennett's paper takes up the problem of Ventris' resources and methods in deciphering Linear B (LB) successfully. Ventris' most important progress before decipherment was in recognizing the type of the script as a syllabary, his use of Kober's work in analyzing the morphological and systematic variations in the texts, particularly seeing that the variation in onomastics is inflectional and not derivational, and in his systematic analysis of phonological variation in *CV* groups where CV_1 alternates with CV_2 in such a way that one can surmise that $C_1 = C_2$. Up to the final stages of decipherment Ventris believed that the language of the texts would be «Pelasgian» (some language related to Etruscan). The outcome shows that with sound and rigorous method, such an assumption would eventually be self-correcting.

Although much has been written about Ventris'decipherment (of the type: what did he know and when?), Bennett's paper is useful for pointing out what Ventris' work makes clear to those working at present on undeciphered scripts: 1. the importance of examining and understanding systems; 2. the initial non-importance of the actual language of texts, because a wrong preconception will easily lead one to read too much into a document. One may wish Bennett had applied his insights more thoroughly to current problems in decipherment, yet his fellow authors have done just that.

Pope, examining precursors to Emmett Bennett, shows that scholarly work on undeciphered material was pursued even in the eighteenth century. The prerequisites for decipherments were the same then as now: 1. preparation and edition of texts; 2. desire to understand these texts; 3. understanding of systems and subsystems in the texts. Pope sketches briefly the development of understanding about writing systems in general, the knowledge of which could be typologically extended to non-deciphered writing systems. Thus, eventually, the recognition of the number of signs in, for example, LB should lead one on typological grounds to classify it as a syllabary, even before a decipherment. Pope continues with a description of early scholars who began to recognize that languages could be genetically related. W. Wotten (1715 & 1731) is especially noteworthy for his claim that similarity of grammatical structures is the key to relating languages. Pope believes that Wotten influenced Sir William Jones. In any event, a curiosity about unknown scripts is the «first cause» of any decipherment. Pope closes with a rather polemical call for better education of the general public.

Although Pope's article presents interesting information, it is too clearly meant as an oral presentation and could use some re-editing. I found some passages simply incomprehensible. The material on Wotten is interesting but incomplete. To understand these earlier precursors, one cannot take them out of context. The 17th and 18th centuries saw a tremendous interest in languages and their history, culminating in first editions of numerous earlier texts in Gothic, Old English etc., as well as grammars and dictionaries. Lambert ten Kate (1710), to name just one scholar, also examined the relationship of languages by comparison of structures, in this instance, the strong verbal system of Germanic, even going so far as to reconstruct, on this evidence, non-attested forms in Gothic. Pope might well refer to the works of Metcalf, Borst, *et al.* for a better perspective on Wotten. Pope's bibliography contains interesting material, but he abbreviates titles of 17th and 18th century works, sometimes considerably. For example, he gives one work simply as: «Cornelius de Bruin. 1698. *Reizen, etc. etc.* Delft». This is unfortunate, because many of us do not have access to these earlier books and would appreciate more information on them and their contents, information very commonly contained in the title.

Olivier delineates three areas of problems in the decipherment of Pictographic Cretan: 1. terminological, 2. practical, 3. methodological. The first consideration is whether the script should be called «hieroglyphic» or «pictographic». Olivier suggests that though «pictographic» would be preferable, «hieroglyphic» is more popular, but that one ought to qualify the term with «Cretan» to avoid confusion with Egyptian Hieroglyphic.

The practical problems are qualitative and quantitative. For various reasons, there is little material in Cretan Pictographic, this scarcity being the greatest obstacle to a decipherment. The quality of the sparse material is also often sadly deficient: much is found on seals and may or may not be script; furthermore, many texts are extremely short. An important prerequisite to a decipherment is a definitive edition of the material. Olivier is working on one (to be titled *Corpus Hieroglyphicarum Inscriptionum Cretae* or *CHIC,* joining the ranks of *CoMIK* and *GORILA* as instances of Belgian acronymic whimsy), but has been delayed because of his work with new LB material. He properly points out that it is better to take more time and produce a good tool for further research than to publish prematurely.

Methodological problems include the fact that we do not know what the language of the inscriptions is. Olivier feels the language is unlikely to be the same as in Linear A texts. Palaeographical methods promise little; likewise internal methods. Olivier suggests the need for more statistical analysis of texts, hoping thus to establish facts about the typology of the language: e.g. reduplication.

On the whole, this article is very informative and interesting. Some stylistic infelicities came about in the translation from an original French version. Some points of analysis can also be disputed or revised. For example, «reduplication» is probably linguistically irrelevant, unless it can be linked to morphological functions. Until we understand Linear A (LA) and Cretan Pictographic more completely, we can hardly claim them to be separate or identical languages: cf. Alphabetic and Cypriote Greek or Runic and Latin alphabetic inscriptions for Old English. Finally, the many graphics inserted at the end of the text are generally well done (although figure 2 was unreadable in my copy of the book), yet a great deal of information in the tables is already included in the text and seems redundant.

Duhoux's contribution on LA is an excellent survey of the field. After a short description of the material and texts, Duhoux outlines necessary conditions for a decipherment: rigorous editions, sign lists, sufficient texts, knowledge of what language is in the text. Except for the last, all prerequisites have been fulfilled.

The writing system is mixed ideographic and phonetic, typologically identical to the system of LB. Duhoux's use of the term «phonetic» is not entirely accurate. A phonetic writing system is one capable of expressing articulatory distinctions on an allophonic level. LB and presumably LA are neither phonetic, nor very successful as phonemic writing systems, but rather make distinctions on the level of natural phonemic classes; e.g. velar consonants form a natural class which is represented by one series of LB signs. Because of the generally accepted genetic relationship of LA and LB, it would seem reasonable that at least some of the LB values can be applied to LA. Duhoux raises the question of how to know whether the value of a LB sign can be applied to a LA sign. He first looks at the LB vowel signs, because they are expected to occur almost exclusively in word initial position. He concludes that whereas the LB values of /a, e, i/ very probably can be carried over to LA, /o, u/ are less certain. Duhoux now goes on to morphological alternation and consonant initial syllabic signs (see above), also showing that some signs may be equivalent in LA and LB. Because the languages written with LA and LB were in contact, one would expect a certain amount of borrowing, so common groups of signs may be comparable cross-linguistically. Of greater value are longer sign groups. Duhoux finds 2 words of four common signs, 3 words with 3 signs. He also agrees with Neumann's suggestion that the sign for /ni/ which is used in LB ideographically for 'fig' stands for the Greek gloss νικύλεον from Athenaeus (3, 76e) and that this is a loan word from the language of LA; thus, the LA sign will also represent /ni/ (Neumann 1962: 51-54).

Unfavorable factors for equating LA and LB values include problems with signs of the structure *Ce* and *Co*. Oddities of the LB system (e.g. labialized and palatalized consonants) have been explained as traces in the writing system of LA phonology, but a number of the odd LB signs have no LA equivalent. Scribal corrections also cause difficulty, because, while they may indicate equivalency of consonant values, they may have other causes.

Duhoux interprets ca. 30 LA signs as equivalent to LB. Insertion of these values into texts seems to give several words or derivational elements attested in later Greek and of otherwise obscure etymology.

Duhoux proceeds to look at accounting and votive texts using the LB values and searches for structural patterns. For the votive texts from Kato Symi and Mt. Iouktas, he sets up a typology of votive syntax which will commonly indicate such elements as giver, receiving god, gift, etc. Duhoux identifies tentatively a number of recurring forms in the tablets with these semantic functions. (In an Appendix D he gives the texts themselves, although in an oversight, his discussion itself includes no reference to this appendix).

In conclusion, Duhoux points out that no decipherment has succeeded in identifying the language of LA, because there are too few texts, the texts are too short and syntactically impoverished, the writing system may not be well suited to the language, the language may be an isolate, we may not understand the writing system, and there are no bilinguals. For a decipherment to be valid, it must use properly edited texts, recognize the typological structure of the script, give proof of phonological values using proper comparative methodology, describe orthographic principles, recognize the morphological, phonological, lexical, and syntactic structure of the language, interpret a maximum number of texts, have a maximum compatibility with LB elements that are non-Greek and suspected borrowings, and satisify the principles of theoretical economy (e.g. a decipherment of LA as Chinese is historically implausible and, hence, uneconomical). An eventual decipherment will come about with more texts, better study of text internal structures, and more work on Mediterranean languages.

Space prevents a more detailed critique of Duhoux, but a few points should be made. Duhoux's list of alternations of signs in set patterns is interesting but remains trivial unless inflectional or derivational explanations can be posited. His quest for loans is interesting, but our lack of knowledge of social and linguistic patterns in the time preceding LA and up to the compilation of Hesychius makes the search of loans extremely problematic. Are the loan words in LB or later Greek from the language of LA, or unexplained original IE, or even from yet another language? Finally, Duhoux cites articles only by name of journal or anthology, not by title of article. Although this is an accepted practice in some European circles, this reviewer missed the information that titles generally provide.

Palaima addresses the problem of Cypro-Minoan (CM), first convincingly questioning the division of the script into three types. He calls upon scholars to look more closely at the actual texts in their epigraphic, historical, and archaeological contexts. Although CM has a number of longer texts and texts of diverse structure and function, hopes of decipherment will not be good until a rigorous corpus is compiled and the sign system is analyzed in greater detail. By way of comparison, CM texts have only ca. half the total number of signs attested in LA. Taking Faucounau as an example of uncareful decipherment, Palaima indicates that by looking at too few texts, one can easily read into the texts whatever one hopes to find. Palaima decries the rash of «decipherments» which may influence scholars in other fields and waste precious library funds. He presents, as well, a critical historical survey of CM research from the late 19th century onward.

This article is well written and entertaining, although it too contains occasional phrases betraying its original oral nature. On p. 147, Palaima should speak of Luwian instead of Hittite and his reference to Mycenaean Greek and Cypriote is unclear. Pages 123, fn. 3 and 125, fn. 6 should refer to Olivier 1985. Somtow 1986 is not included in the bibliography (S. P. Somtow, *The Shattered Horse.* New York: Tom Doherty Associates, Inc. 1986). Following the text is a good set of illustrations, filling at least in part the need to compile accurate drawings of CM texts.

Bonfante and Bonfante is a bit of a puzzle, for Etruscan is generally not considered a problem of decipherment, but rather of interpretation. The authors present a state-of-the-art research report on attempts to interpret the Etruscan material. Although I am not qualified to judge their study, I noticed several points. Etruscan is not «different from any other [language] in Europe or elsewhere» rather it is an isolate *genetically,* so far as we know. Characterization of Etruscan pronunciation as «harsh» is hardly objective. The reader who misses a discussion, however short or negative, of attempts to relate Etruscan to other known languages may now consult Adrados 1989 for various references.

To sum up, this book is stimulating and fulfills well the function of presenting what careful scholars consider the prerequisites for decipherment in general and for a number of specific problems. The many illustrations are attractive, but sometimes give little real information: cf., e.g., p. 57 (I suspect they often represent the handouts given to conference participants). As stated above, a few of the papers could have stood an additional re-editing, but in general the quality of contents and style is high. Typographical errors are few and generally self-correcting.

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M. BERNAL, *Cadmean Letters,* Winona Lake, Eisenbrauns 1990, pp. XIII + 156.

David Simon in a brilliant new treatment of modern police work (*Homicide: A Year on the Killing Streets,* Boston, Houghton Mifflin 1991) reports that experience has taught Baltimore homicide detectives to adhere to the following principle in conducting investigations: «They have a saying: 'Fuck the why. Find out the how, and nine times out of ten it'll give you the who'. Juries 'have a hard time when a detective takes the stand and declares that he has no idea why Tater shot Pee Wee in the back five times, and frankly, he could care less', but 'Pee Wee isn't around to discuss it, and our man Tater doesn't want to say'». This principle applies to varying degrees to the five books that I shall review jointly here. It explains their strengths and weaknesses and how they are likely to be received by scholarly juries.

Åström-Sjöquist's *KKK,* surely the most inauspiciously —for non-racist Americans— acronymicized book with which J.-P. Olivier has ever been associated, continues actual police investigative work with Linear B tablets: the identification of papillar line traces from the hands of the individuals who manufactured the tablets from the site of Knossos. As such, it is a follow-up investigation to *Pylos: Palmprints and Palmleaves* = *PPP* (*SIMA* Pocket-Book 31) which worked with the much smaller and more chronologically and spatially restricted corpus of tablets from the mainland site of Pylos. Both primary investigators would be right at home in Baltimore. They lay out clearly for us their techniques and procedures, the physical condition of the material being studied, and the evidence available to and produced by their investigation (ca. 10,000 tablets and fragments / 3,000 with some traces of papillar lines / 1,002 with traces significant enough to be useful in determining some characteristic feature of the individuals who handled the tablets during their manufacture, e.g., left-handed vs. right-handed or position of tablet relative to the hand / 388 identifiable papillar line traces / 45 'hands' and 1 'thumb' with specific identity). Olivier discusses in an appendix (pp. 122-128) his view of the significance of their results for our understanding of Mycenaean administrative bureaucracies. The numerous tables and figures are well-located in the text and well-designed to allow the reader to understand both methods and conclusions. They also give the reader a chance to make independent deductions before turning to the expert commentary. Mistakes in proofreading and in the style of the translation are minimal and in most cases self-correcting: e.g., L.*A*. Palmer (p. 6 text), *J*. R. Palmer (p. 6 n. 11), __.L. Bennett (p. 7 n. 16). Only on p. 45 is a misunderstanding possible. The authors mean to say that there might be fewer than 46 individuals associated with the 45 'hands' and 1 'thumb' with specific identity. Since they have followed the cautious practice of keeping right and left 'hands' distinct, some of these might in fact be pairs of hand belonging to the same individuals.

My only criticism of the manner of presentation concerns terminology. In *PPP*, the 10 specifically identifiable palmprints (3 certain, 7 less certain) were termed 'tablet-flatteners' and identified individually by Greek names and as *Anonymous I-VII*. They were thus distinguished from the identifiable scribal 'Hands' who wrote the texts on the tablets. In *KKK*, the 45 'hands' and 1 'thumb' with specific identity are termed 'Hands' and designated as R (right) or L (left) followed by different letters of the Greek alphabet. The tablet scribes in *KKK* are called scribes, and the variations of scribal hand 124 from the Room of the Chariot Tablets are differentiated by small Roman alphabetic letters, thus: «124» s. However, in Jan Driessen's recent work on this Knossos archives, these 'scribes' have been assigned proper names, e.g., «124» s = Simon, and this method of designation creeps into *KKK* on pp. 30-33. Thus there is a great potential for confusion among the uninitiated, especially Near Eastern scholars who will be reading this material. Does 'Hand' mean 'scribe' or 'flattener'? Does a name identify a 'flattener' or a 'scribe'? At a time when sealing experts from the Aegean and other areas of the ancient world have called for standardized terminology in the interests of cross-disciplinary dialogue, Mycenaean tablet experts seem to be creating terminological chaos for the sake of referential whimsy.

I am not opposed to whimsy on principle. In fact *KKK* refreshingly retains plenty of it. Sjöquist reports (pp. 19-22) on a replication study of tablet manufacture that he conducted with his grandchildren, using Greek and Swedish clay, in June 1986. By bribing the children with ice cream and coconut balls, he was able to get them to make tablets for a few days and thereby determine the probable rate of manufacture of the Linear B tablets (ca. 100 per 'flattener' in a full workday under optimal conditions, such as ready availability of clay and water) and the causes of odd physical marks, such as rings, on tablet surfaces. Both authors are amusingly expansive on the sequence of possible explanations they considered and ultimately rejected for the observable overrepresentation of left-hand papillar line traces (pp. 17-18): the 'tablet flatteners' were twins, or a sub-class with peculiar ethnic or religious scruples, or Bronze Age artists exhibiting the same higher tendency toward lefthandedness as the modern artist community. Again the grandchildren provided the real answer: right hand was often placed atop left to obtain added force when moulding the clay into tablets.

By concentrating on the 'how', this study does arrive at the 'who'. Among the papillar line impressions, it is possible to distinguish the hands of young children 9-12 years old and the impressions of old worn hands, probably those of individuals involved in manual labor. Among the left-hand impressions, 55% are children, 31% adult, 14% unidentifiable (p. 28: statistics here are translated into percentages within the particular group in order to avoid confusion). Thus Olivier (p. 122) argues for a strict hierarchy whereby scribes are literate functionaries of considerable status who would not stoop to 'playing in the mud', but would leave the messy task of tablet manufacture either to children who would do this for four years before being reassigned to other tasks or to aged or handicapped manual laborers for whom this was a reasonable form of social security employment. Olivier is of the opinion that none of these individuals was an apprentice scribe or a senior scribe and therefore categorically rejects the idea, still considered possible by Åström-Sjöquist and me (*KKK,* p. 119 and fig. 30; *PPP,* pp. 106-107), that some of the tablet-manufacturers were scribes-in-training or even the scribes themselves. Olivier's opinion is based on unprovable notions of the social status and cultural-aesthetic sensibilities of Mycenaean scribes and of stratification within this particular sector of the Mycenaean labor force. While agreeing that «Knossos n'est pas Pylos», Olivier proposes that practices ought not to have varied at the two sites in this regard (p. 123). Yet we are informed that physically the Knossos tablets are much wetter when used (and pinacologists know that the many small leaf-shaped tablets from Knossos are matched at Pylos chiefly by the few tablets fallen into the Throne Room), that the methods for handling the tablets showed greater variety at Knossos, and even that the sheer quantity of tablets needed to record transactions within this much more complex Creto-Mycenaean palatial bureaucratic system might have led to a kind of emergency conscription of tablet-makers. Therefore, I see no compelling reason for accepting Olivier's hypothesis about the Knossos system nor for transferring it to a mainland site with a much different ethnic and bureaucratic history. In fact it seems rather perverse to maintain that young workers who would be responsible for tablet manufacture for senior scribes for four years would then all be rotated off this assignment and out of this sphere of work, despite the insight into techniques of bureaucratic administration these four years of potential apprenticeship would have afforded them and despite the provisional conclusions of S. Hiller in Palaima *et al.* eds., *Studia Mycenaea (1988)* (Skopje 1989), pp. 40-65 —from albeit meager evidence— that children generally were trained to work in the occupations of their parents. Finally we are not even told whether the Pylos papillar impressions also were made mainly by two distinctive age groups of youngsters and oldsters. Perhaps this will require reexamination of the mainland material. But without this information, without clearer evidence for the systematic social and economic stratification of Knossian scribal Pee Wee's and tablet-flattening Tater's, and without Near Eastern parallels for systems in which tablet boys never grow up to be scribes —on the contrary, Walker in *Reading the Past,* p. 43, states categorically that in cuneiform scripts «[t]he *first thing* a schoolboy had to learn was *how to make a tablet* and handle a stylus» [italics mine]— it is much safer to dismiss Olivier's explanation of 'why'.

Reading the Past = *RtP* brings together six separate studies of ancient writing systems of the Mediterranean and Middle East which had been published separately in the British Museum series of the same name. *RtP* is a superb introductory source book for non-specialists because the specialist authors in cuneiform (C. B. F. Walker), Egyptian hieroglyphs (W. V. Davies), Linear B (J. Chadwick), the early alphabet (J. F. Healey), Greek inscriptions (B. F. Cook) and Etruscan (L. Bonfante) provide clear explanations of the 'what' and the 'how', including hundreds of explanatory charts, tables and excellent drawings and photographs of texts in these scripts. Each section ends with a useful bibliography and in some cases an explanatory glossary of technical terms.

Of chief interest to readers of *Minos* are the treatments of Linear B and alphabetic writing by Chadwick and Healey respectively and the brief introductory survey (pp. 6-13) of the general historical development of writing systems by J. T. Hooker. With his recent death, students of Bronze Age scripts and civilization have lost the greatest synthesizer and generalist of the post-Ventris years —his *Mycenaean Greece* (London 1976) and *Linear B An Introduction* (Bristol 1980) have no equals as bibliographically comprehensive and readily understandable introductions to the main features of, and the problems connected with, Aegean Bronze Age culture and scripts— and a self-styled gadfly who was always ready to question received opinions on the basis of his own idiosyncratic version of common sense and his deep familiarity with the ancient Greek and pre-Greek cultures and languages of the Balkan peninsula. Hooker's introduction treats several themes that were central to his theories on the development of Aegean writing. I disagreed with his opinions on each of these themes and wish to do no more than to call attention to them here, because I think it would have pleased him immensely to know that his ideas encouraged continuing debate on topics that other scholars were willing either to set aside as definitively settled or to ignore as minor annoyances: (1) 'double writing' in Linear A and Linear B; (2) whether Linear B is to be defined as partially 'logographic' or partially 'ideographic'; (3) the possibility that the Greek alphabet (and Linear B) might have originated from multiple prototypes and at different times under varying circumstances rather than from a single model script at a single time in a single place.

Chadwick's section is divided into seven parts: (1) the discovery of Linear B; (2) its decipherment; (3) the use of Linear B; (4) the tablets as historical documents —the typo 'document' in the table of contents (p. 139) being a remarkably rare proofreading error in so linguistically and transcriptionally complicated a volume; (5) Linear A; (6) Cypriote scripts; and *mirabile lectu* (7) the Phaistos disk. To specialists and interested non-specialists, much of the presentation will be familiar from Chadwick's other published work: *Documents in Mycenaean Greek*[2] (Cambridge 1973); *The Decipherment of Linear B*[2] (Cambridge 1967); *The Mycenaean World* (Cambridge 1976); and his contributions to the *Cambridge Ancient History* and Mycenological and Cypriote conferences. Here, however, his comments about technical details of the scripts, the reasons for their peculiarities, and the nature of the documentary evidence are much better served by illustrative material than in the works cited above. There are necessarily certain restrictions imposed for the sake of the general reader. Thus the discussion of Linear B ideographic signs that are also used phonetically —and not acrophonically associated with Greek words for the objects they represent— could be expanded to **22* GOAT and could discuss the gloss *nikuleon* but for the hypertechnicality of such points. On p. 163, the uninitiated reader might be led to draw the mistaken inference that the conventions for 'sexing' the ideograms for domestic animals are known to derive from the Minoan system. And perhaps some mention should have been made of the fact that animal ideograms *per se* in the surviving Linear A documents are severely underrepresented. For that matter, it would not have been a bad idea to stress that the ideograms for MAN and WOMAN in Linear B do not derive from the attested Linear A sign for HUMAN BEING$^{M? \text{ or } F?}$ (here on p. 181 the ambiguity in Linear A is noted by single quotation marks, thus 'man') and do not follow the 'sexing' conventions used for other animate ideograms. Likewise, the treatment of Cypriote scripts is concise and clear, and one could only suggest minor improvements, such as some discussion of the problem of transcribing the sign *le* in *Opheltau* on the bronze spit from Palaipaphos, the earliest readable Cypriote inscription. The Linear B script does not distinguish between *re* and *le*; the later Cypriote Syllabic script does. What did Minoan do? What did this earliest undoubtedly experimental version of the Cypriote Syllabic script in the Paphos region really do?

The most serious minor flaw in Chadwick's section of *RtP* is the failure to include a map by means of which the general reader could locate the sites on/in Crete, the Cycladic islands, the Greek mainland, Cyprus, and N. Syria that have produced inscriptions in Aegean or Aegean-related scripts. A map should also be added to Cook's section on Greek inscriptions. A map in Chadwick's section would have enabled the reader to define by comparison the spheres where cuneiform scripts (Walker p. 18) and Aegean scripts held sway and interacted. It would also have served as an introduction to the treatments of the spread of the early alphabet and the later proliferation of Greek inscriptions. As it now stands, *RtP* contains no map at all of Greece and the Aegean —Healey's section only provides a map of the Levant. If a Bernalite were reviewing this volume, cultural imperialism or worse would certainly be imputed to the authors and editors. They would stand accused of taking for granted that educated readers would know where all the Minoan-Mycenaean and Greek sites were located— these places after all are securely within the orbit of high western culture. The help of maps is only thought necessary with the non-Indo-European Etruscans, Egyptians, Arabs, Levantines and Middle Eastern cultures. Fortunately I am not a Bernalite.

Chadwick's bibliographical endnote (p. 195) is also the skimpiest in this collection. It could be expanded by references to Hooker's *Linear B An Introduction* (Bristol 1980) —a rather inexplicable omission even if Hooker had not been called upon to write the introduction to the entire volume— and Y. Duhoux *et al.*, *Problems in Decipherment* (Louvain-la-Neuve 1989) which provides the most recent thorough reports on the state of scholarship connected with Cretan Hieroglyphic and Linear A (in French), and Cypro-Minoan and Etruscan (in English). *Problems in Decipherment* also includes papers on the Ventris decipherment and on the development of modern scholarly interest in decipherment (in English).

One last suggested improvement would be to explain at least one complicated tablet fully through annotation of a drawing coordinated with a transcription and translation geared to the annotated sections. Otherwise, despite the charts of signs and the discussion of contents of texts, the idiosyncratic methods used to record information on specific tablets remain virtually unfathomable to the reader. Chadwick's analysis (pp. 176-177) of the notorious Tn 316 offers a good example. I think that it will frustrate the non-expert reader not to know what sequences of signs identify the date and place with which the tablet is said to begin —somewhat misleading since the tablet begins with a month name, but the first place name is buried in the repeated formula where it stands parallel to the names of sanctuaries in following sections of the tablet. I also think that an interested reader would like to be able to see where the names of familiar deities like 'Zeus, Hera and Hermes' occur and, since many such readers are likely to be Greek-literate, what form these familiar theonyms take in Mycenaean.

In regard to interpretation of this sample text, I doubt whether Chadwick's explanation of scribal fatigue gives the real reason why the last three entries have the simple form of vessel **213*^{VAS}. After all, this ideogram occurs five other times, including three successive entries to *pe-re-*82, i-pe-me-de-ja,* and *di-u-ja* in the second-to-last section (followed by **216*^{VAS} dedicated to Hermes) and two successive entries on what Chadwick considers the recto. The variation in vase shapes I think is meaningful throughout and perhaps an indication of the relative status of these deities in this particular context and set of circumstances: **215*^{VAS} and **216*^{VAS} are much more elaborate vessels and therefore more precious offerings than the simple gold cup **213*^{VAS}. Of course, we also have to keep in mind the possibility that the forms of the vases might be determined by the forms of rituals connected with the specific deities. Moreover, I know of no parallel in the Linear B tablets for a scribe disregarding the identification of a particular ideogram by substituting another simpler, but also particular ideogram. There is no evidence that **213*^{VAS} stands generically for 'vase' in the way the unsexed and unligatured livestock ideograms can stand generically for a particular species of animal. Through the cumulative weight of a series of cautious hypotheses, Chadwick raises the thrilling specter that the text of this tablet records desperate sacrifices of ten human beings as a vain attempt to avert impending disaster. If this dramatic trick wins new students of Mycenaean script and culture, there is little harm in it. But surely, given the wealth of the Mycenaean palatial elites, it is hardly unthinkable that the offering of thirteen gold vessels (eight of which are simple 'conical cups') is a part of a regular, perhaps annual, ritual. Other hypothetical props for the 'human sacrifice' scenario are just as weak. Again this explanation of 'why', while not implausible, is improbable and certainly not compelling. I should close my discussion of the Linear B section by saying that I have used it as a separate fascicle as a required text in an advanced undergraduate course on Mycenaean society at University of Texas at Austin. It was a success with the self-selecting students in this specialized course.

Healey divides his discussion into five main sections, three of which concern us here: (1) script, language and the alphabetic principle; (2) first attempts at alphabetic writing; and (3) the consolidation of the alphabet and its spread to the west. In the first section, Healey discusses not only the ways in which early scripts represented spoken language through conventional signs, but also how writing materials helped determine the form of scripts. By contrast, with the Minoan-Mycenaean evidence, the form of the scripts and certain extant materials (e.g., Minoan nodules) help us to theorize about the most important non-attested applications of script: pen-and-ink records on parchment or papyrus. The Semitic scripts Healey describes (p. 207) as 'consonantal alphabets' that «handled the root aspect of word-formation well, but [were] defective in [failing] to account satisfactorily for vowels». This feature of Semitic scripts is linked closely to the very nature of Semitic languages in which «consonants are the bones which convey the basic meaning, while the vowels add flesh to the skeleton». In the second section, Healey describes proto-Sinaitic scripts, the special 30-character Ugaritic cuneiform alphabet, and south Arabic scripts. The Ugaritic script not only provides us with our first positive evidence for canonical ordering of signs in abecedaries, but it also adds three characters, one specially designed for use in writing Hurrian, the other two to represent front and back vowels after the glottal stop as a complement to aleph which represented glottal stop + mid-vowel. This Healey considers (p. 216) «an intrusion of syllabic writing into an otherwise consonantal system». Defining the nature of these Semitic scripts is one of the most controversial topics of debate among students of writing theory. The dean of writing theorists I. J. Gelb, *A Study of Writing*[2] (Chicago 1963) p. 184, considered all scripts before the Greek alphabet word-syllabic (logographic) or syllabic. Thus he assigned them a lower rung on an evolutionary ladder wherein the Greek alphabet is the crucial and culminating step. Bernalites see this as another form of cultural imperialism, if not thinly disguised anti-Semitism. However, uninvolved literary types like Anthony Burgess (*Observer* 7 April 1991, p. 63) can use it as material for an amusing and arch display of inventiveness by coining the expression 'betagam' to refer to the scripts which constitute the crucial intellectual transition between less wieldy ideographic/logographic syllabaries (e.g., Akkadian, Sumerian, Hittite cuneiform, Linear A and Linear B) and the Greek alphabet and its descendants. This essential problem of definition and the political and cultural controversy with which humorless Bernalites have invested it remind one of the question of whether a glass of water is half full or half empty and the varying responses of pessimists and optimists. I shall not carry the analogy any further. But I shall say that I think that either Healey or Gelb is right —the Greek alphabet is a significant advance over a consonantal alphabet or a peculiar form of syllabary— and that Powell (reviewed below) makes this clear by offering to his readers clear, full and practical explanations of the mechanics of ancient writing systems.

A final point taken up by Healey in section two (pp. 218-219) is the direction of writing in West Semitic and Phoenician texts. Ugaritic alphabetic texts are written left-to-right, with very few exceptions. Proto-Sinaitic/Proto-Canaanite texts show writing in either direction. In Phoenician writing, right-to-left direction became canonical ca. 1100-1050 B.C. This matter is given such attention because it is later used in Healey's discussion of the date of origin of the Greek alphabet (pp. 239-243), which is a second controversial question central to the works of Powell and Bernal. There are three well-known approaches, and Healey concisely covers them all: (1) historical probability based both on the most likely period for Greek-

Phoenician interaction of a sort likely to lead to the creation of a new script and on the dating of extant Phoenician and Greek inscriptions; (2) palaeographical diachronic comparisons of early Greek letter forms with their Semitic prototypes; (3) other considerations such as direction of script. I shall deal with the first two approaches later in reviewing Powell and Bernal, but I wish to stress here that direction of script carries as little weight in this argument as it does in regard to the origin of Linear B (cf. Palaima in *Studies Bennett* [*Minos* Suppl. 10, Salamanca 1988], pp. 310-313). Healey suggests without absolute conviction that since Greek alphabetic inscriptions varied the direction of writing (right-to-left, left-to-right, *boustrophedon* —he omits *Schlangenschrift*) before finally settling on left-to-right, the Greeks must have borrowed the alphabet in a period when the direction had not become canonical in the prototype script, i.e., pre-1050 B.C. This is not true. The direction of most of our reasonably well dated earliest extant Greek alphabetic texts, long and short, are in fact written right-to-left (cf. Powell, *HOGA,* pp. 123-186). Nestor's cup and the Dipylon oenochoe, where the writer has conscious control of his field, are written right-to-left. Most brief early onomastic and proprietary graffiti also run sinistroverse. Variation from this pattern occurs mainly on chronologically ambiguous material (e.g., the Stillwell sherds: *HOGA,* pp. 132-133 no. 21), or in inscriptions where the field was harder for the inscriber to control: e.g., natural rock or stone statue bases. Thus, if one wanted to make anything of so arbitrary a feature of writing as direction, one could argue that the Greek alphabetic evidence actually supports a borrowing by the Greeks when the right-to-left model was already the established norm in the mother script, thus explaining the *tendency* of the Greeks in the earliest phases of using the script to defy what we assume to be their innate preference for dextroversity, a preference so powerful that it later won out over the influence of the archetype. For the Greeks eventually, when outside the sphere of strong Phoenician influence, gradually and universally changed direction and ultimately canonized this change. We should also recall (Walker, pp. 24-25) that cuneiform scripts underwent a change of direction and tolerated variation in this regard, depending on the nature of particular texts, for a considerable length of time.

John DeFrancis, the author of *Visible Speech = VS,* is emeritus professor of Chinese at the University of Hawaii. His new study and classification of writing systems was undertaken as a result of a nearly lifelong frustration with the way the Chinese writing system has been interpreted by other universal writing theorists —he deals specifically on pp. 56-64 with the schemes proposed by Gelb, Sampson and Hill. Consequently *VS* offers a much different perspective on how scripts function. So despite the fact that his treatment of Linear B and Cypriote Syllabic is limited to five paragraphs (pp. 174-175) based on general handbook information and his treatment of the Greek alphabet to seven pages (pp. 175-181) based on the 1961 edition of L. H. Jeffery, *Local Scripts of Archaic Greece* (Oxford), his theoretical analysis of syllabic and consonantal scripts might offer Aegeanists fresh perspectives on the operational principles of these writing systems.

First, let me comment on the little DeFrancis has to say explicitly about the Aegean-Cypriote scripts. Given his expressed motives for writing *VS,* he should be made aware that «turnabout is foul play». Aegeanists will be just as annoyed with his simplification and misrepresentation of the principles and details of their scripts: (1) Linear B is described as a «partially pictographic» «meaning-plus-sound script»; (2) we are told that consonant clusters are very few in Greek and that they are rendered in Linear B by «telescoping two CV syllables as in the rendering of *tr* in *ti-ri-po-de*»; (3) that Cypriote Syllabic was in use from the sixth to the third centuries B.C; and (4) that Cypriote Syllabic deals with CVC syllables and clusters of two consonants «by telescoping two CV syllables, as in the case of *ka-re* for *gar* and *a-po-ro-ti-ta-i* for *Aphrodite*». The mistakes, half-truths, and muddle here are the result of an encyclopedic instinct which forced DeFrancis to include five almost throwaway paragraphs on Minoan-Mycenaean-Cypriote scripts as a bridge to a fuller discussion of 'pure' alphabetic systems.

DeFrancis tries to concentrate on 'how' the Greek alphabet came to be, and in so doing reveals how contaminated this question is by cultural politics. While acknowledging that some modern scholars —and DeFrancis is pre-Bernal— have thought that the Phoenicians might have brought the alphabet west, he follows Jeffery in maintaining that the adaptation of Semitic to Greek alphabet required close contacts of the sort provided by 8th-century Greek settlements in the Levant as opposed to the «tenuous links» of «traveling Phoenician traders» —here he is unwittingly anti-Bernal, unless these words were written with a detached irony that I failed to notice. This Phoenician-Greek transformation is contrasted with the much less direct 'idea diffusion' from Mesopotamia which, according to DeFrancis, led the Egyptians to create their script using cuneiform as a vague inspiration rather than a strict model. Bernalites will be pleased that DeFrancis downplays the degree of genius required to create the Greek alphabet. The Greek adapter(s) needed most: (1) an ignorance of what phonemes are and how graphemes represent them; and (2) a language which virtually demanded that they add independent vowels to the independent-consonantal base of the Phoenician script. On this last point, DeFrancis does little to explain why he is convinced, *contra* Gelb, that Egyptian and the West Semitic scripts are consonantal and not syllabic. He cites briefly (pp. 150-151) general remarks of three scholars who oppose Gelb's view (Edgerton, Naveh, Barr) and more or less declares that he likes what they —Edgerton chiefly— have to say. For a full and careful setting forth and weighing of the chief arguments for and against Gelb's position on West Semitic, cf. *HOGA*, pp. 238-245, which comes to the opposite conclusion.

The essential theoretical arguments about writing systems and their classification of interest to students of syllabic scripts are presented in pp. 47-151. As with other standard studies, which DeFrancis carefully reviews, the chief problems are those of definition. DeFrancis first (p. 5) defines 'full' or 'real' writing as «a system of graphic symbols that can be used to convey any and all thought» and later (pp. 20-21, 42-43) emphasizes that speech underlies all systems of full writing. He carefully explains (pp. 48-49) Bloomfield's dictum that «[w]riting is not language, but merely a way of recording language by visible marks» by emphasizing that this only implies «that writing had to be based on speech, not that it was an accurate representation of speech, or not even, perhaps, that it did nothing but represent speech», and then undertakes an analysis of the development of writing starting with those systems that are less precise in phonetic representation and/or have what is conventionally termed an ideographic or logographic component.

DeFrancis reminds us (p. 49) that all writing systems have two components, what he later (p. 51) calls the «Duality Principle»: (1) «symbols that represent sounds and function as surrogates of speech»; and (2) «symbols that add nonphonetic information». He defines the step that created the three fundamental world writing systems as the application of the *rebus principle* whereby Sumerian (3000 B.C.), Chinese (1500 B.C.) —here ignoring the nationalist interpretation which sees a true system of writing already in existence ca. 3000 B.C. with the Ban Po pottery marks— and Mayan (the beginning of the C.E.) went from using pictographs with their «original meaning value[s]» to using them to represent «the sound evoked by the name of the symbol». Once this «epoch-making invention» has taken place, «[o]ne who continues to refer to them simply as pictographs misses the central point about the nature of writing...». We may then wonder whether DeFrancis had any clear definition in mind at all when he referred to Linear B as a «partially pictographic» «meaning-plus-sound» script. Nonetheless, he is driven to this way of viewing scripts because he believes that scholars have overvalued the pictographic component of Chinese. DeFrancis stresses that all writing systems are incomplete in representing speech —intonation, stress and tempo rarely being represented— and can be classified according to their «phoneticity»: from 0 percent (non-phonetic picture writing) to 99 percent —100 percent obtainable only by a tape recording of actual speech. Among modern languages and scripts, Finnish ranks very high because its orthography creates a close correspondence between symbols and sounds. German, Spanish and Russian are ranked lower. English is ca. 75 percent phonetic; Chinese 25 percent.

This concentration on phoneticity has much to recommend it. It lies at the basis of a classification scheme of scripts as «phonemic» (alphabets) or «syllabic» (syllabaries), both being to some degree «logographic». In order to counter the facile definition of Chinese as «pictographic, ideographic, word-syllabic, logographic, [or] morphemic», DeFrancis insists on defining the operational level of a writing system according to «the indispensable operational unit that enables the script to function». He then goes further by establishing a dichotomy between two units: (1) the meaningless graphic unit that corresponds to the smallest segment of speech represented in writing, which he calls a *grapheme;* and (2) the basic unit of writing that is surrounded by white space on a printed page, which he calls a *frame.* Graphemes are the indispensable operational units, while *frames* are usually best seen as *lexemes* in dictionaries or lexical entries, especially in those scripts (cf. Japanese and archaic/classical Greek) which write characters without techniques for separating *frames* one from the other, e.g., spacing, word-dividers, variation of letter height. English is almost forced into using alphabetic graphemes because of its large inventory of ca. 8,000 spoken syllables. Chinese, with an inventory of 398 spoken syllables (or 1,277 with tones) just manages as a syllabary. Both of these must still compensate for poor sound-symbol correspondence in a way that DeFrancis defines (p. 56), but does not explain: Finnish is 'pure phonemic'; English 'meaning-plus-sound' phonemic. Japanese is 'pure syllabic'; Chinese 'meaning-plus-sound' syllabic.

So far as I can tell, 'meaning-plus-sound' should have to do with the degree to which independent frames or lexemes which are ambiguous in phonetic representation can be differentiated by non-phonetic techniques, e.g., by determinatives or by context or by the preservation of 'historical spelling', i.e., by mnemonic processes which in some way virtually convert a phonetically represented lexeme into a logogram at some stage of the mental process of reading. Thus a Mycenaean scribe would almost instantaneously 'read' the *pa-si* in *pa-si-te-o-i* in the offering context on KN Fp 1.5 and 1.7 as dat. plur. *pansi* 'to all', but the *pa-si* in *da-mo-de-mi, pa-si* in the record of landholding PY Ep 704.5 as 3rd sing. pres. *phāsi* 'says'. Our complicated procedure of conventional transcription of the syllabary into selected Latin CV or V units which then have to be translated into restored Mycenaean Greek often relying on imprecise knowledge of the given record-keeping context forces us to reproduce in mechanical slow motion the stages of the mental-linguistic process involved in writing and reading such a script. English historical spellings such as 'course' and 'coarse' enable us to differentiate instantaneously between two different *lexemes* which could be written identically according to some scheme of phonetic spelling which opponents of historical spelling in the U.S. have long championed. I think that DeFrancis avoids discussing this process fully because the hated word 'logogram', which he believes has perverted our understanding of Chinese script, is crucial to our explanation. When he does finally define 'meaning-plus-sound' for different phonetic categories of writing systems, his definitions seem rather confusingly *ad hoc* and, in the case of syllabic scripts, to be mainly derived from the peculiarities of Chinese.

When describing his new scheme of classification, I doubt whether DeFrancis had a clearer idea than his reader of whether and how his defining categories could be applied to non-Chinese systems. Here, keeping in mind the proviso that no systems are absolutely 'pure', DeFrancis proposes three pairs of contrasting systems:

1. 'meaning-plus-sound' syllabic or *morphosyllabic* systems vs. 'pure' syllabic systems;
2. 'meaning-plus-sound' consonantal or *morphoconsonantal* systems vs. 'pure' consonantal systems;
3. 'meaning-plus-sound' phonemic or *morphophonemic* systems vs. 'pure' phonemic systems.

The reader is left without any discussion of the exact meanings of the first items in each of these pairs. Brief definitions are found in the glossary on p. 280, where, for example, we read that *morphosyllabic* identifies «a writing system (example: Chinese) that basically represents syllables but also makes extensive use of nonphonetic techniques, such as determinatives, to suggest the meaning category to which a given written item belongs». This is a far different view of meaning-plus-sound syllabic than what I had inferred, but then imagine my surprise when I looked at the chart on p. 58 and saw Linear B classified as 'pure' syllabic: recall that on p. 175 it was defined as a 'meaning-plus-sound' script, i.e., as *morphosyllabic*. The lack of determinatives in Linear B must separate it here in the author's mind from Chinese, Sumerian and Mayan; and the specialized use of logograms or ideograms in Linear B must later suggest to him that it cannot be 'pure' syllabic. It thus fits neither definition. Given that a classification scheme is only as valid as the classes it defines, we should again say that «turnabout is foul play».

The problems extend further. None of the three glossary definitions make use of the fundamental conceptual term *lexeme* or *frame* which DeFrancis took such pains to define, again suggesting the *ad hoc* nature of his reasoning. Moreover, morphosyllabic was defined in terms of the way in which nonphonetic techniques defined the meaning category of given written items: i.e., the nonphonetic techniques make distinctions solely on the lexemic level. The definition of morphoconsonantal is extremely vague: «a writing system (example: Egyptian) that basically represents consonants but also makes extensive use of nonphonetic techniques, such as semantic determinatives, to suggest the meaning category to which a given symbol belongs». The examples used to demonstrate the way Egyptian hieroglyphs work (pp. 161-163) indicate that 'symbol' here should be replaced by 'lexeme' or 'frame'. For the determinatives are used to specify the specific meaning of the sequence of symbols that make up a lexeme, i.e., they are not defining the value of single graphemic symbols. What is not clear from DeFrancis's discussion either is whether the determinatives can be used to specify different vocalic values that would differentiate distinct lexemes written with the same sequence of consonants, e.g., if one wanted to distinguish among consonantly written Greek *dls* as *doulos, dēlos, deilos*. All his examples deal with identification of a specific *lexeme* or disambiguation of the various meanings of the same *lexeme*.

Both morphosyllabic and morphoconsonantal scripts then use nonphonetic techniques to clarify *lexemes*. However, morphophonemic scripts are defined as those in which meaning is taken into account when determining sound. The specific example is the different pronunciations of the English plural indicator in *pots* and *pods*. This example deals then with the *graphemic* level: *pods* is not written *podz* because the function of the final phoneme as a designation of plurality dictates the conventional use of the grapheme *s* in both cases. Alphabetic Greek is considered to be 'pure' phonemic, apparently because it permits no such ambiguities of phonetic representation dictated by the meaning of a lexeme.

We have then seen how difficult it is to devise classifications for scripts that will satisfy experts in those scripts with which the classifier does not have a firsthand familiarity. Perhaps DeFrancis has arrived at a truer notion of how Chinese functions: I defer to Sinologists to decide. Mycenologists will not be satisfied, but those interested in the abstract principles of their writing systems will still benefit from considering the new approaches used in *VS*.

There are three points that I wish to make in closing. First, DeFrancis follows Lieberman, *AJA* 84, 1980, pp. 339-358, in criticizing the view that the tokens from Mesopotamia and Iran studied by Denise Schmandt-Besserat could be precursors of writing. Lieberman stresses the shortcomings of Schmandt-Besserat's analysis, her failure to reconstruct separate chronological, geographical and cultural token systems, and the implausibility of certain of the parallels she proposes between the shapes of tokens and early characters from Uruk. DeFrancis objects specifically on the grounds of function: the token systems simply do not work in the way that writing works. The discovery of the idea of writing is likened (p. 74) to turning on a light switch: «a sudden physical (or mental) flick, and light (or phonetic writing) appears». Both lines of criticism are justified, but only to a point. DeFrancis is extreme in stressing function. One need not maintain that the existence of token systems meant that the idea of writing was «gestating over a period of five millennia», only that physical symbols were used over that span of time in calculating and recording the quantities of certain animate and inanimate goods essential to the primitive societies that used the symbols. Certain of these symbols were then used as the archetypes for writing symbols just before, while or after the switch was turned on. Even at the stage when the tokens were impressed into the surface of bullae, writing *per se* did not exist. While DeFrancis is quite right in saying that an oxcart can never become an automobile, it is wrong then to conclude that the basic form of the first bears no relationship to the second. Again one should read two paragraphs in *HOGA*, pp. 69-70, for a clear and simple explanation of the token systems and their relationship to the earliest forms of writing.

Second, the earliest stage of writing in the Middle East (Uruk IV-II and Jemdet Nasr 3500-2900 B.C.) is described (p. 79) by Civil and Biggs, *Revue d'assyriologie et d'archéologie orientale* 60, 1966, pp. 12-14, as 'nuclear': «Only those elements indispensable for writing the phrase are represented in the writing: all, or almost all, the roots and quite a limited number of affixes». Here DeFrancis properly notes as key factors «the limited content of much of early writing and the limited circle of scribes who dealt with it». M. Civil, *Orientalia* 42, 1973, p. 23, is cited as judging that «Sumerian in its earlier stages goes further than any other known script in its omission of elements predictable only to the well-informed reader». DeFrancis then asserts that «[w]hen the Sumerians got around to it, they eventually did go in for belles lettres, and in the process they also expanded graphic representation of the phonetic component in their writing system». We are asked then to believe that the horse of document typology pulls the cart of systemic completeness and efficiency. Instead, Mycenologists will view the 'primitive' features of early Sumerian as potentially a mere problem of its selective application and documentation, not of the inefficiency of the entire system. Our Linear A tablets are almost as minimalistic as the early Sumerian texts described by Civil and Biggs. The Linear B tablets have limited content and a very limited circle of scribal writers and readers. Heading sentences in Linear A of more than four lexemes are rare. Entries consist normally of one lexeme and an ideogram and/or numerical quantity. Yet some longer syntactic sequences of lexemes occur on metal pins and even on storage pottery; and the efficiency of Linear B, despite the tachygraphic nature of many of its accounting documents, leaves little doubt that the Minoans and Mycenaeans could have used their scripts for belles lettres at any point. The systems were complete. The absence of belles lettres is the result of either a cultural choice or the hazards of archaeological discovery.

Lastly, DeFrancis (pp. 82-84) explains determinatives and phonetic complements in Sumerian in a novel way. He interprets both of these to be determinatives: the first being «semantic determinatives» which define the category of meaning to which phonetic base symbols belong; the second he calls «phonetic determinatives» which provide clues to the phonetic identification of essentially semantically defined base symbols. The virtue of his insight is that it reduces these signs to variants of the same technique. I wonder whether one might not be able to view some phonetic adjuncts and ligatures in Mycenaean in somewhat the same way (e.g., *VAS* + *di* or *VAS* + *a* using phonetic determinatives that identify the essentially semantic symbols *VAS* as specific logograms) and to trace this important technique of early scripts back to Linear A which seems to use if very frequently.

In reviewing Barry B. Powell's *Homer and the Origin of the Greek Alphabet* = *HOGA*, I must bring us back to the dictum of the Baltimore homicide cops: «Fuck the 'why'. Concentrate on the 'how'». Those who do not do this and who trust reviews that concentrate primarily on Powell's idiosyncratic explanation of 'why' will miss, as those reviewers do, the main strengths of *HOGA*. Powell's theory of 'why' certainly offended J. T. Hooker's common sense. One can read his review «The Earliest Writers in Europe», *TLS* (June 14 1991), p. 29, for highly rhetorical objections to what he termed Powell's «scarcely credible theory». I shall confine myself to a series of three questions on this point before addressing the rest of *HOGA*. If the alphabet was created by a Greek auditor of Homer to record his poems which were the central documents of Hellenic *paideia,* how was it possible for the creator of the alphabet and the preserver of the Greek national epics to have become what Hooker calls «[a] single unknown, unnamed and unremarked genius?». Why and how did the other traditions connecting the invention of the alphabet with Kadmos, Palamedes, Prometheus come into being and entirely supplant the truth? Why does no tradition contain even a hint that the preservation of important works of literature was the primary motivation for creating a script? *Tria interrogata sapienti sat.*

Of interest to Homerists will be the fourth and fifth chapters and the second appendix. The fourth discusses archaeological, linguistic, and traditional evidence for the date of Homer. The fifth presents Powell's theory. The second appendix catalogues Homeric references in poets of the seventh century B.C. Our concern here is with understanding writing systems, how they worked and how they developed. Powell treats such matters in three chapters and an appendix: (1) Review of criticism: What we know about the origin of the Greek alphabet. (2) Argument from the history of writing: How writing worked before the Greek alphabet. (3) Argument from the material remains: Greek inscriptions from the beginning to c. 650 B.C. The first appendix summarizes and criticizes the arguments of Gelb and the counter-arguments of Semitists for and against Gelb's theory that West Semitic writing is syllabic. Powell is persuasive in his conclusion that the Egyptian and Phoenician scripts are syllabic. These sections of *HOGA* are superb on all counts. Powell masterfully assembles all available evidence, summarizes often widely divergent scholarly opinions, and presents his own criticisms, interpretations and explanations in a remarkably lucid and entertaining style —and I do not intend the adjective 'entertaining' to imply that Powell in any way sacrifices scholarly substance. I would advise anyone interested in learning how writing functioned at different stages of development (especially Egyptian, Cypriote, Phoenician, and Greek alphabetic) to read Powell, pp. 5-118, 238-245, 249-253, before turning to Gelb, Sampson, or Driver, and certainly before reading DeFrancis. Powell outstrips all these in being readable, clear, fair in assessment of opposing viewpoints, practical in explanation of technicalities, and cautious in his consistent and precise use of theoretical terminology. I thoroughly enjoyed his technique of introducing sections within chapter 1 with appropriate quotations from ancient Greek poets and sophists and modern epigraphers and historians. His two modern illustrations of Phoenician syllabic on p. 101 of chapter 2 are unforgettable.

Powell's main argument is this. The Greek alphabet is the first script in the history of writing that to a great extent allows the reader to pronounce words as they were sounded by representing their basic component sound units. This is a radical advance over earlier systems that relied on mnemonic techniques which reminded the reader of words he already knew through nonphonetic and imperfect phonetic techniques. Therefore some special motive must have inspired the creator of the alphabet to produce this change —notice that Powell, *contra* Hooker, believes in a single act of creation of the alphabet. Since the separate representation of vowels, interestingly enough termed *azuga* or unattached elements in Greek, was the chief innovation that made rather precise phonetic representation possible, the special motive must have to do with the vowels. Epic poetry is the most conspicuous aspect of early historical Greek culture in which exact vowel-representation is important. Powell (pp. 18-20) adheres to a date for the introduction of the alphabet (ca. 800 B.C.) dictated by the now considerable spread of Greek alphabetic finds from the second quarter of the eighth century B.C. and slightly later, and he stresses the Euboean connection with all the important early alphabetic sites: Al Mina, Lefkandi, Pithekoussai. He then explains (pp. 20-67) logically and practically how every aspect of the transformation from, to him, a Phoenician syllabary to the Greek alphabet took place, including the names, values and order of signs and the invention or eventual addition of new signs. He summarizes his ideas on pp. 66-67, emphasizing the minimal changes that occur in the script from the point of its inception onward, i.e., the Greek alphabet requires no long period of gestation to arrive at the form in which it is used in classical and later times. Chapter 3 (pp. 119-186) contains a descriptive catalogue of early Greek inscriptions, which Powell eventually (pp. 183-186) uses to make the point that these are uniformly non-economic and non-public and that they give an impression «that Greek literacy first flourished in an aristocratic world that is socially symposiastic and temperamentally agonistic, much like the life in the palace of Alkinoos described by Homer».

Scholars interested in the technicalities of Greek alphabetic script should read Powell's explanation of how epichoric scripts and letter forms came into being. Here I shall close the substantive part of my review by asking yet another question that concerns the technical and literary sides of Powell's thesis. Powell's practical bent, reinforced no doubt by the discussions he acknowledges as having had with Emmett L. Bennett, Jr., leads him to produce (p. 65) for his readers a sample text of the first ten lines of the *Iliad* in the hand of the adapter. What strikes me about this text is that it does not indicate vowel quantity at all. As such one could make two claims: (1) The Greek alphabet is still functioning mnemonically (*contra* Powell p. 3), because the sequence of continuously written signs still only suggests the true identification of separate *lexemes.* (2) If the alphabet was invented for verse, it is puzzling that this crucial prosodic aspect was not accounted for. Perhaps the limitations imposed by the Phoenician prototype were such that the invention of a string of five long vowels was too great a change to make. Ionic only develops *eta* by the accident that it is psilotic and therefore needs the sign for nothing else. It then develops omega by altering the shape of *omicron,* this invention being suggested by the practical symmetry of the perceptible difference in articulation of long and short versions of the mid-vowels *e* and *o.* One could also wonder why the notion of separating the lexemes, which surely would have been an expedient measure for recorders and readers of epic, did not occur. Again the force of the prototype and the inherent conservatism of users of script probably provide the answer.

There are some small oversights in internal referencing. Page 6 n. 4 should read 233 ff. The reader should be informed on p. 14, in the text preferably, that the crucial inscribed Late Geometric Attic sherd from Al Mina is in fact included in Powell's catalogue of early Greek inscriptions: p. 129 no. 12. The peculiar form of

Corinthian *epsilon* discussed on p. 29 is missing from Table II on p. 9. Only the second of these corrections, however, affects the way in which the reader can appreciate the arguments being presented and the evidence for them. My final recommendation is to read this readable book.

My first recommendation with regard to Martin Bernal's *Cadmean Letters* = *CL* is not to read this book unless you are less sensitive than I am about scholarship being politicized, sensationalized, and misused for personal psychological needs or about scholars being accused, either directly or by insinuation, of racist, anti-Semitic or merely elitist prejudices which strongly influenced or wholly determined their published ideas and interpretations. *CL* should also not be read by any scholar with little patience for evidence being interpreted or half-interpreted by *ad hoc* methods in order to support a preconceived thesis, mostly with an utter disregard for the consequences, historical or otherwise, of any particular hypothesis. Many readers of *Minos* will be familiar with the scholarly methods used in *CL* from their familiarity, voluntary or involuntary, with proposals for the decipherment of Linear A or the Phaistos disk and/or for the redecipherment of Linear B.

CL is a specialized monograph about the origins of the Greek alphabet, arising from Bernal's preoccupation with the Afroasiatic origins of Classical civilization: two volumes entitled *Black Athena* are in print. Although what I am now going to say will undoubtedly be construed as the equivalent of a statement like «Several of my friends are black or Jewish or Catholic or gay or Martian», it should be clear to any reader of *Black Athena* or *CL* that the underlying thesis that fuels Bernal's pseudo-scholarly machine cannot be faulted. The origins of modern Classical scholarship are demonstrably Indo-Aryan and Germanic and the process and results of Classical research show the effects of the idealization of early Greece and classical Athens by eighteenth-and nineteenth-century primitivists and romantics, by British imperialists and cultural elitists, by twentieth-century Nazis and fascists and White Anglo-Saxon Protestants. This is certainly not a revelation. Even within my memory, William Calder created a stir in one of his history-of-scholarship pieces by examining the National Socialist connections of Werner Jaeger. And as a Lithuanian-Polish Roman Catholic Mycenologist from the working class of Cleveland, I am familiar intellectually with such things as Evans's culturally dictated distortions of Minoan culture and Blegen's and Mylonas's Aryan interpretations of a 'royal portrait' from the Shaft Graves. I am also familiar emotionally and by experience with subtle and not-so-subtle forms of prejudice that are still bred and active on the Main Line in Philadelphia and in and around Harvard Square. I was stunned not so long ago by the revelation at a reception at a foreign archaeological school in Athens that racial prejudice no longer existed in the United States because the first black couple had been admitted as members of an exclusive Main Line country club— «It's simply a matter of them learning to speak our language». —and because black inner-city children were allowed to play once a year on the tennis courts of the club— «Of course, we teach them first how to behave». These and equivalent forms of prejudice are essential parts of cultural systems and breed mythologies that will control lesser intellects and incautious higher intellects and will predispose scholarly minds to conceive of problems and solutions according to the basic principles of those mythologies.

Yet no one has argued before with such fanaticism that so many of our fundamental notions about the formative stages of Classical Greek culture are grossly in error because of a general passive or active anti-Semitic bias. Bernal seems convinced that few Classical scholars are capable of forming interpretations and theories substantially independent of their cultural prejudices and personal psychological predispositions, i.e., that few can conduct themselves in any way other than he does in the pages of *CL*. The tone and undertone of the argument in *CL* remind me very much of McCarthyist techniques.

Again those familiar with the mentality of 'decipherers' will understand my difficulty in arguing against the thesis about the Greek alphabet advanced in *CL*. By long experience, I have learned that 'decipherers' attribute any rational criticism of their proposals to one's being part of a hidebound scholarly establishment or intellectual inner circle of the sort that rejected Copernicus's new ideas. Again one cannot deny that such closed circles exist and that they often behave in exclusionary ways for other than intellectual reasons —I suspect that there was some experience of this in J. T. Hooker's career. But I have a confidence in the integrity of scholars —I have been fortunate to meet a few whose 'prejudices' in this regard were all intellectually determined— and even in the integrity of Classical scholarship which decipherers and Bernal do not share. Bernal's technique of argumentation is diabolical. By attributing the errors in accepted modern theories to conscious or unconscious prejudice, he creates a situation in which rejection of his new ideas is a defense of the old ideas and therefore inherently racist. He then adds a further subtlety. Accepted modern theories are not only erroneous, but they are the result of a systematic and intentional eradication of old, true ideas that were commonly accepted in antiquity. Thus to argue in their favor is to be a conspirator in the perversion of the past. One feels a bit like Winston Smith. Here is what I mean, and here are also examples of flaws in Bernal's reasoning.

The central thesis of *CL* is that the alphabet was transmitted to the Aegean during the Bronze Age, about the middle of the second millennium B.C. and in conjunction with a colonization and settlement of Greece by Phoenicians and Egyptians that was known and accepted by the ancient Greeks, but has been deliberately suppressed by modern Aryanists.

P. 1: Rhys Carpenter's theory (1934) of a date of origin of the Greek alphabet in the late 8th century B.C. has held sway. It was written during the 1930's which «saw a zenith of scientific confidence and positivism in disciplines on the fringes of natural science» which was experiencing what we are to think of as a healthy «relativism and uncertainty». *Implications:* Carpenter's work is an example of writing that was too confident and sure of its results in a discipline that was then behaving unscientifically.

«During the climax of modern anti-Semitism between 1925 and 1940, a number of attempts were made to prove the Aryan origin of the alphabet». *Implications:* Carpenter was writing during an intensely anti-Semitic period and cannot have escaped its influence.

Pp. 2-3: «[L]ate nineteenth- and early twentieth-century scholars identified the relationships between 'primitive' Semitic alphabets and the 'noble' alphabets of Greece and Rome with that between the early, simple forms of life and humans, and with that between 'primitive peoples' and 'the glories of the Caucasian race' seen in Darwinism». Carpenter described Semitic letters in terms of their deviations from Greek letters: aleph was horizontal instead of vertical; zai «low and squat with a slanting bar»; ḥēt had two slanting bars instead of three; yōd had a stroke too many; pē is «hooked instead of bent». *Implications and Criticism:* Bernal believes that Carpenter's description, with its concentration on the bent and slanted physical features of Phoenician signs, is unpleasantly anti-Semitic in tone. Ullman, *AJA* 38 (1934) 366 n. 1, is cited as contemporary confirmation of this. However a look at Ullman's article and note reveals no mention of it at all. Bernal, however, will eventually declare (p. 8): «Carpenter's article proposing 720 B.C. as the date of transmission [of the alphabet]... should, I believe, be seen in the context of the sharp intensification of anti-Semitism in the 1920s». On the same page he links Carpenter with Havelock as practitioners of «unabashed Aryanism».

P. 3: There is no parallel for the casual transmission of the alphabet by or among merchants. Bernal accepts Lejeune's contention that alphabetic writing is not transmitted by «diffuse popular imitation, but is guided by experts under the local (civil or religious) powers». *Implications and Criticism:* According to Bernal, this means that it would take longer for the «bewildering variations» of the Greek epichoric scripts to develop. He thus makes an unsupported claim about the rate and degree of sign variation in the epichoric scripts. What does he mean by the loaded term 'bewildering'? Students in an introductory course on epichoric scripts and dialects can learn to recognize these relatively minor variants within a matter of weeks of part-time study. The changes region to region developed, according to traditional theory, over nearly a century during a period of intensive trade and colonization. None of this strikes me as bewildering. *Caveat lector passim* for this kind of dramatic use of language. Lejeune's theory should also commit Bernal to explain the circumstances for such a non-casual transmission of the alphabet through the agency of the ruling authorities of Aegean Bronze Age palatial societies. The Minoan and Mycenaean ruling elites already possess functioning syllabaries of their own creation and use them extensively in daily administration and even (the Minoans) on religious artifacts. Why would they have commissioned an alphabetic script? Why did they not then use it on any surviving documents? What imaginable use could it have served in the context of the highly restricted literacy of the period? Bernal here answers none of these questions which are key to his own theory, because he is intent on arguing against what he terms the Carpenter theory by any means at his disposal. He picks and chooses various alternatives without bothering to see that they make no sense in terms of his alternative theory.

Pp. 4-5: The ancients (Hdt. 5.58-59) believed that Kadmos brought the alphabet into Greece in the Bronze Age from Phoenicia. Hekataios associates Danaos from Egypt with colonizing Argos and introducing the alphabet. Josephus speaks of Homer being an oral poet. In the seventeenth century C.E., the Homeric question begins by emphasizing this idea. During the eighteenth century, the rise of romanticism creates a cult of the primitive which highly values illiterate nationalist folk songs. The notion of a late-developing Greek literacy actually made the Greeks seem even more superior to Near Eastern cultures. F. A. Wolf in 1795 canonized the Homeric question. B. Niebuhr was one of the few who still maintained that the Phoenicians had colonized Greece and introduced the alphabet. We are told later (p. 15) that «[i]t is interesting to note that it was during the late 1920s that Milman Parry began his study of Serbian folk epics to show that the

Iliad and *Odyssey* could have been composed without writing». To Bernal (p. 15 n. 19) the Bellerophon story in *Iliad* 6.115-206 and references to *spondai* in *Iliad* 2.339-341 are sufficient indications of Homeric literacy. He cites A. Johnston in R. Hägg ed., *The Greek Renaissance of the Eighth Century* B.C. (Stockholm, Paul Åströms 1983), p. 67, in support of the second part of this claim. *Implications and Criticism:* Quite frankly it is hard to know what Bernal is implying. First, Bernal here again, as he will later, picks and chooses within the wide body of ancient tradition those legends which emphasize a Bronze Age invention of script. He then seems to be suggesting that a conspiracy began with Josephus and was taken up again in modern times by the Abbé d'Aubignac to view Homer as an oral poet in order to create an illiterate Greek Dark Ages. Two possible references to the art of writing in the whole of the two epics is hardly sufficient to demonstrate that Homer is literate or to wipe out an illiterate Dark Ages, as Bernal implies. There is an enormous bibliography on this aspect of the Bellerophon story, and I would suggest that Bernal read the article by W. Burkert in *The Greek Renaissance,* pp. 51-56, for an Aryanist interpretation that stresses that the story does refer to Phoenician-Greek alphabetic writing on a writing tablet, but that the biblical and Anatolian parallels for the story *per se* suggest that it is an orientalizing novella of ca. 700 B.C. Johnston mentions the reference to *spondai* in the same context: eighth century attestations of writing.

I shall simply ask any reader of this review to explain to me what Bernal finds interesting about the date when Parry began his work. In its context on p. 15, where Bernal explicitly states that «Carpenter's securing of an illiterate and impermeable dark age» was used to uphold the 'Aryan model' and discredit the 'ancient model' (on which see below), I think we are supposed to react like good patriotic citizens during the McCarthy era and view the date when Parry happened to come of mature, if precocious, intellectual age —a complete biological accident so far as I can judge— as suspicious behavior, as if his scholarly activities contributed to «the climax of modern anti-Semitism between 1925 and 1940». I hope that readers of this review will think that such an insinuation is far from interesting, but rather stinks and is something that cannot and should not even be pardoned by calling it reverse bigotry. Readers who have doubts about what Bernal is implying about Parry here should recall his initial insinuations and eventual clear accusation concerning Carpenter and Havelock (pp. 1-3, 8).

Pp. 6-7: As he believes he has proved in *Black Athena,* Bernal claims that by the fifth century B.C.E. the Greeks conceived of their past according to an 'ancient model' which maintained that Greece had been settled by the Egyptians and Phoenicians around the middle of the second millennium B.C.E. This 'ancient model' stood in place until the 1840's when it began to be replaced by an 'Aryan model' which saw the Greeks entering their eventual homeland through a northern invasion, unattested in antiquity. Gradually the Phoenician-Semitic contribution was deemphasized. The non-Greek elements of Greek culture were now attributed to a pre-Hellenic substrate population «the racially Caucasian but linguistically non-Indo-European Aegean population». Such theories were first proposed at Göttingen where J. F. Blumenbach, «the first systematic classifier of human races and the inventor of the term Caucasian», also taught. *Implications and Criticism:* The implications are patent. Again by selective use of a few ancient traditions, Bernal posits that Greece generally was settled by Egyptians and Phoenicians in the Bronze Age. Advances in our views of cultural development on the basis of progress in the fields of linguistics (unpardonably Indo-European) and archaeology (the discovery of Mycenaean and Minoan cultures) and even through a more sophisticated historical interpretation, from George Grote onwards, of the entire, often internally contradictory, body of Greek legends are again attributed to the impact of Aryanism. If the Greeks of the fifth century generally thought that Greece had been significantly colonized and settled by Egyptians and Phoenicians in the mid-second millennium, why is no mention made of this in the *archaeologia* of Thucydides, which analyzes the important changes and influences in Greek prehistory? What was the impact of this colonization, besides the invisible and implausible Bronze Age alphabet which Bernal wishes to invent for us? If this colonization were recognized as a real event, what aspects of Aegean Bronze Age and later Greek culture would have to be interpreted differently than they are now interpreted according to the 'Aryan model'? Even confining ourselves to Kadmean Thebes and Danaid Argos, what in their post-'colonization' Bronze Age histories and archaeological remains gives any clue of Egypto-Phoenician influence? What about ancient legends linking the ruling house at Mycenae, the Pelopid dynasty, with Anatolia? How does this fit the 'ancient model'? A serious scholar would sit down with the first volume of Grote or a mythological handbook and analyze the foundation legends for all major Greek communities with Bronze Age antecedents to see what the pattern of foreign connections, if any, is. He or she then would carefully study the archaeological remains from the Middle Bronze Age onwards for clear indications of Egypto-Phoenician presence or at least influence. This is not done here and it is not done in the pages of *Black Athena*. Bernal avoids this scholarly responsibility by his assertion in *CL*, p. 1: «I am not able or even attempting to *prove* my case; I am merely proposing what I hope to be plausible and heuristically fruitful hypotheses that make more sense and provoke more interesting questions than conventional wisdom». This is McCarthyist insofar as it pertains to assessments of the scholarship of other individuals, and it is irresponsible insofar as Bernal has made his theories into a sociopolitical cause and will attract a readership incapable of doing the technical research needed to evaluate his «more interesting» unconventional hypotheses and questions.

I shall limit myself to one more example of insinuation. On pp. 20-22 Bernal discusses an article by Naveh in the 1973 *AJA* on Semitic epigraphical aspects of the dating of the Greek alphabet. As an example of how Aryanists ignore the scholarship of Semitists, Bernal writes (p. 22), «As late as 1983, Alan Johnston was able to publish an article on the subject with no mention of Naveh's work». Again this is mere inflammatory rhetoric intended to touch the nerves of naive readers who are susceptible to the McCarthyist tactics that brand anyone who does not think politically correctly an anti-Semitic Aryan. I invite readers of this review to read Johnston's article (complete citation above). It is a straightforward account of the most recent archaeological evidence for early Greek inscriptions, how this documentation had changed in the preceding thirty years, and its implications for the question of archaic Greek literacy. Consequently, his bibliography, aside from references to the reports of new finds, contains selected references to a few articles on early literacy in note 1. There is no reference to Carpenter, Ullman, or any later works, Aryan or Semitic, dealing specifically with the question of the date of introduction of the Greek alphabet or how the problematical interpretation of letter forms in surviving Greek and Semitic inscriptions relates to this question. Johnston is guiltless, but so were many of those in the United States in the 1950's who were the victims of interesting hypotheses about their actions that their accusers felt no need to prove. Fortunately no careers or lives are likely to be ruined by the malice contained in this monograph.

I shall conclude my review of this pseudo-scholarly monograph with several random examples of its self-contradictory or non-existent logic. First, Carpenter is taken to task for using the *argumentum ex silentio* to support an eighth-century date for the introduction of the Greek alphabet, and then for readily seizing upon Al Mina as the likely place of transmission, despite the total absence of Greek inscriptions from this N. Syrian site. Yet Bernal seizes upon the single Phoenician-inscribed metal bowl, not stratigraphically dated, from Tekke near Knossos in Crete as sufficient evidence of a strong Phoenician presence in the Bronze Age Aegean of the sort that makes a prehistoric introduction of the alphabet likely. If this is so, the publication of a Greek sherd from Al Mina (J. Boardman, *OJA* 1, 1982, pp. 365-367, not cited by Bernal) can now stand as sufficient proof of Carpenter's theory. It, in fact, holds more weight, since it is easier to explain why a luxury item like an inscribed metal bowl would be imported into an illiterate region than it is to explain an ostrakon. Moreover, it is a known fact, again conveniently ignored by Bernal, that the cemetery area at the site of Al Mina was destroyed, thus eliminating the best potential source of inscribed sherd material.

Bernal (p. 25) cites the fact that Ugaritic signs are found on Mycenaean pottery as early as 1300 B.C. as evidence for an early introduction of Semitic scripts into the Aegean. Does he know of the large number of Mycenaean pots and Canaanite jars in Cyprus and the Argolid with Cypro-Minoan marks? Are we to use Bernal's logic and conclude from this evidence, certainly far more forceful than the single Tekke bowl or Al Mina sherd, that the Mycenaeans and Canaanites were using Cypro-Minoan at this time?

On p. 8, the theory that the Greek alphabet originated in Cyprus is dismissed because «the Greek Cypriots continued to use a syllabary into classical times». How does this same logic apply to the introduction of an alphabet ca. 1500 B.C. into societies that used syllabic scripts (Linear A and Linear B) to the end of the Bronze Age?

Finally, Bernal, *CL*, xii, 30, pp. 113-116, advances the theory that Greek *phi* was borrowed from South Semitic *qop* to represent the original Greek labiovelars. When labiovelars were eliminated by several sound changes, this sign was then applied to *phi*. We are told that this is reasonable because of evidence of the Greek treatment of the foreign place-name Gublum/Byblos (p. xii), originally heard by the Greeks as a labiovelar in the form G^{w}i/eblum. We are even treated to the hypothesis (pp. 30-31) that ideogram **124* in Mycenaean might be *biblos* 'papyrus, scroll' because of its resemblance to the Egyptian hieroglyph with this value. Think of «interesting questions» raised by these related proposals, the practical implications for the functioning of a Bronze-Age alphabet with a single (?) labiovelar sign. Contemplate what later happens to labiovelars in the environment of *u*, of *i* or *e*, of *a* or *o*. Actually look at Mycenaean texts to see where and how sign **124* occurs. Go figure.

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T. G. PALAIMA, C. W. SHELMERDINE and P. Hr. ILIEVSKI eds.: *Studia Mycenaea (1988)* (*Živa Antika* Monographies 7) Skopje 1989, pp. 193 + vii. $30 US from Program in Aegean Scripts and Prehistory, Department of Classics WAG 123, University of Texas at Austin, Austin, TX 78712-1181 USA.

The papers in this volume were presented at the fifth section, devoted to Mycenaean studies, of the XVIIIth Eirene Congress of the Eastern European Classical Associations held in Budapest, Hungary on August 29 to Sept. 2, 1988, and now appear separately as a monograph of *Živa Antika.* These ten papers cover a variety of subjects and approaches, from the succinct presentation of epigraphic and linguistic problems to a wide-ranging and systematic synthesis of textual, archaeological, cultural and historical data. Several of the papers touch on different aspects of the same texts, in particular the Pylos Aa/b/d series (de Fidio and Hiller), and the Ma and N- assessment and collection texts (Shelmerdine and Stavrianopoulou).

J. Makkay and W. C. Brice both focus on sign formation and transmission of scripts. Makkay discusses a group of inscribed clay objects dating to the Middle Bronze, ca. 1200-1100 B.C.E., from the same general area in Yugoslavia. All bear incised signs, and some are elaborately decorated. Except for a pierced clay ball with 5 signs that resembles Cypro-Minoan examples (Makkay, Fig. 5), all the objects are types whose Aegean parallels are usually not inscribed. The most interesting example is an inscribed bowl of local ware, whose exterior was covered with signs arranged in rows. The signs incised on these objects are all angular, with very few curving lines, and bear a general resemblance to the Aegean scripts. Makkay himself draws no conclusions from this material, apart from the general observation that such examples may be inspired by Mycenaean contact; instead he invites his audience to draw their own conclusions.

In a short note, Brice also deals with the shape or style of individual signs in the Cretan scripts. He distinguishes between a 'bureaucratic' writing style on clay for record keeping, and a 'monumental' style inscribed or written on sealstones, pottery, metalwork and tables of offering. He states that there are greater similarities in sign shapes between Hieroglyphic and Linear A forms found on these 'monumental' objects than between forms found in the clay records. Brice suggests that the 'monumental' style is somehow inspired by the Anatolian scripts, while the 'bureaucratic' style is influenced by Mesopotamian writing conventions. Brice thus adds a new aspect to the problem of the relationships between proto-Linear A, Hieroglyphic and developed Linear A. However Brice does not specify at what time the Anatolian scripts came to the notice of the Minoan craftsmen inscribing these 'monumental' objects, nor to what extent the forms of the signs in the 'monumental' style influenced the 'bureaucratic' style, and vice versa.

P. Hr. Ilievski presents a thorough analysis of the linguistic and cultural background of Mycenaean *ti-ri-se-ro-e,* in answer to problems raised concerning the form of the word and the interpretation of the word ἥρως as 'ancestor' (see *Lexicon,* p. 332 and *Documents,* p. 586). He presents a wide range of Greek compounds to show that τρις- can be used instead of τρι- when the following base begins with a vowel, and favors the use of τρις- as an intensifying prefix meaning 'very' over its more literal meaning of 'three'. Thus *ti-ri-se-ro-e* would mean to the Mycenaeans 'the very great hero'. Ilievski emphasizes the interpretation of ἥρως as 'person of valor' rather than 'revered ancestor', citing primarily the use of the word in the Homeric poems, but also its application to Brasidas at Amphipolis. He argues that the *ti-ri-se-ro-e* could be commemorated by the Mycenaeans primarily through the medium of oral poetry, rather than ritual. I do not see that the contexts in which this word appears, in tablets recording offerings to divinities (PY Tn 316.5, Fr 1204), demonstrate that the *ti-ri-se-ro-e* is commemorated primarily through heroic poetry rather than through ritual. The rebuilding of Grave Circle A at Mycenae and the raising of the tholos tombs as symbols of power imply that the Mycenaeans glorified, if not worshipped, their predecessors through ritual. Brasidas of Sparta was adopted as a founding hero, that is as instant ancestor, by the people of Amphipolis as a slap in the face to Athens, since by their action they broke all ritual ties with their mother city (Thuc. 5.11). Furthermore, the interpretation of *ti-ri-se-ro-e* as 'very great hero' has a certain anonymity about it that argues against a specific personality with its own story. But if τρις- were interpreted as 'three', the title 'Triple Hero' might refer to a specific character such as Geryon, whose cattle Heracles stole (*Theogony* 287-294). Greek myth contains many multiple-headed and multiple-bodied monsters with ancient pedigree.

R. Witte in his contribution investigates the nature and the use of three types of data, archaeological, epigraphical and literary, to produce a synthetic view of Minoan society. He proposes a working model for identifying characteristics of the Minoan state, based upon several universal criteria, primarily the identification of territorial units (whether based purely on residence or upon ethnicity) and the nature of state authority both inside and outside Crete. In his model, Witte emphasizes that literary data should be thoroughly integrated with the epigraphical and archaeological material. Epigraphical and archaeological data have been studied jointly with great success, since in a basic sense epigraphical material depends upon the archaeological record for much of its meaning. However, I feel that the proposed integration of the literary data into this overview of Minoan society presents problems which Witte does not acknowledge. Facts may be passed down accurately for a century or more through oral transmission, but their contexts may be quickly lost as the information is constantly reinterpreted to fit changing political and social situations.

I. Tegyey's study of the tablet find groups in the West Wing of the Palace at Knossos continues and refines the classification of texts set out in the well-known works of Olivier (*Les scribes de Cnossos*) and Chadwick («The Classification of the Knossos Tablets», in *Acta Mycenaea* I and *KT4*). He concentrates on correlating find spots and scribal hands belonging to the specialized department for textiles. Several scribes in F6-F7 work on texts relating to varied aspects of cloth making: women cloth workers, including apprentices (Scribe 108), cloth 'to be paid' (*qe-te-o*) to finishers (Scribe 209), different types of cloth (Scribe 208), dyed wool and cloth from east Crete, and totalling tablets (Scribe 113). Scribe 103 is the most prolific; his tablets listing deliveries were found in Magazine F10, but he worked mainly with Scribe 116 in the isolated room F14, apparently the headquarters for the department. Thus Tegyey shows how the tablets of the department of textiles are interconnected spatially, as well as by hand and content, and how the compartmented scribal organization recording the textile industry reflects the different steps involved in clothmaking and management of textile workers.

T. G. Palaima presents an extensive and detailed study of the contexts in which oxen and oxherds (*qo-u-ko-ro*) are found in the Pylos tablets. He places the evidence into a socio-economic framework, using interconnections within the tablets between scribal hands, tablet categories, and palace officials, introducing geographical and zooarchaeological data, and employing ethnographic parallels (including some from the state of Texas). Only eight Pylos tablets list oxen, in small numbers by themselves or with other commodities apparently assessed or collected by officials for sacrifice or offerings, but for no other purpose. In contrast, a total of 294 oxherds (*qo-u-ko-ro*) are recorded, none in any context directly connected to oxen. Palaima shows that these two groups of tablets are linked geographically and administratively, as the places recorded on these tablets belong to the prime cattle grazing areas in the lowlands of the Further Province. The important official **we-da-ne-u* oversees the movement of oxen destined for sacrifice as part of his duties concerning sheep and other livestock, and flax and grain. No set of texts exists for Pylos comparable to the Knossos series recording working oxen, but Palaima proposes that the palace at Pylos also assigned oxen to *qo-u-ko-ro* sent out to assist in the settlement of marginal areas (p. 115). However, Palaima's reconstruction of the palatial manipulation of oxen leaves unclear how many farmers and herders not affiliated with the palace had oxen. The zooarchaeological material from Nichoria cited by Palaima (R. E. Sloan and M. A. Duncan, in G. Rapp, Jr. and S. E. Aschenbrenner eds., *Excavations at Nichoria in Southwest Greece*, 1978, pp. 60-77) shows that in the Middle Helladic settlement, oxen were important working animals which were slaughtered for meat and hides later in life. In LH III sheep and goats might have replaced oxen for meat, milk and hides, but could not have replaced them as plow or draft animals. Palaima describes the pressure on land caused by palatial and private economic needs (pp. 112-113), but it seems unlikely that a population practicing traditional plow agriculture would have become totally dependent upon the palaces for their plow animals. As his quotation from Hesiod demonstrates (pp. 97-98, *Works and Days* 405-406), every farmer would have wished to own his own working oxen, and many must have done so. The exact meaning of *qo-u-ko-ro* remains unclear, as to whether these men herd oxen as their profession, or whether their ownership or use of working animals establishes a certain status.

C. W. Shelmerdine's paper rigorously tests the system of portioning for assessment and distribution tablets proposed by P. de Fidio in *SMEA* 23, 1982, pp. 83-136. It has long been recognized that the Pylos assessment and distribution systems, as typified by the Ma series, are based on a 'top down' system, where the amount of goods to be collected or distributed is established first as a round number for the kingdom as a whole, then subdivided according to province, and then according to district. However, the amounts actually assessed do not always fit the estimated proportion, but may be reduced. De Fidio proposed that all assessment and distribution within the kingdom was based on an overall target figure of 200 fiscal units of each commodity, 100 per province, instead of a separate target figure set for each item, according to the nature of the item, and the palace's need for it. In her system, the actual size of each fiscal unit would depend upon the quantity required by the palace, but the fiscal units would always total 200. The irregularities in the proportions actually recorded are due to a standardized system of reductions. Shelmerdine supports de Fidio's proposal that the total amounts in the Ma series were expressed in units of 100 rather than 80, but sees the target figure as 100 units for the whole kingdom (50 per province) rather than 200 units. She also finds that de Fidio's two-tier system of reductions does not work regularly or equally, and a system which expresses small quantities of goods in terms of 100 or 200 fiscal units proves cumbersome. For example, if the 30 pigs listed on Cn 608 were calculated as equalling 100 units, each unit would equal one-third pig. Shelmerdine also shows clearly that the same overall fiscal scheme should not apply to the bronze or flax tablets, which serve a different purpose of collecting or distributing commodities vital to the palace economy and which are not commonly produced throughout the kingdom.

S. Stavrianopoulou's study of the various tax systems at Pylos provides an overview of the relations between the palatial industrial organization and the collective and individual obligations to the palace designed to support these industries. The Ma and Ac series show how goods and labor were assigned first on a provincial, then district level, while the Na and Nn flax tablets demonstrate assessment based on individual landholding. The two systems of collective and individual obligation were completely separate. Stavrianopoulou explores the strong connection between individual obligation and landholding, especially for bronzesmiths and military personnel, and concludes that the palace used land to procure craft and military service in a mutually beneficial relationship. The land awarded by the palace would be located in all the districts, and in the case of the flax tablets, would include valuable, well-watered parcels. This raises several interesting questions concerning the nature of palace control over this land. Once awarded, did it still belong to the palace, or was it considered to belong to its holder? How did the palace acquire the land in the first place?

St. Hiller presents a comprehensive survey of family relationships and terminology in the Linear B tablets. The types of information vary according to the status of the people listed. Working carefully with the limited evidence available, Hiller shows that at all levels, children take up the same occupation and status as their parents, and frequently are named after them. The largest body of information on family relationships comes from the PY Aa/b/d dependent personnel texts, where the large numbers of workers listed by sex, occupation and general age grouping provide the opportunity for demographic speculation. Using the scant information about fathers and children in these texts, in particular in Ad 684 and 697, Hiller argues that these children are the product of socially accepted, palace controlled liasons. He shows persuasively that *tu* (= *tu-ka-te*) 'daughter' and *i-jo* 'son' in the texts refer to grown children listed with a parent of the same sex. However, the terms *ko-wo* and *ko-wa,* carrying the combined meaning of 'son/ daughter' and 'immature human being', bear a more general connotation that cannot refer to a specific age grade, and in fact may overlap with the use of *tu* and *i-jo*. Hiller demonstrates that the records for the lowest ranks identify children primarily in terms of the mother, even when boys join their fathers. In contrast to this matrifocal system, family relationships in the middle and upper ranks are expressed in terms of fathers or husbands.

P. de Fidio covers several of the same problems as Hiller concerning the relationships within the PY Aa/b/d tablets, in her study of rationing systems. She employs an imposing range of data, from studies of prehistoric Aegean human remains, to the nutritive value of barley and wheat, to age grades and rationing systems in Mesopotamian texts, to the absolute value of the Mycenaean measuring systems. She focuses in this paper on the fixed proportional values of foods found in rations: wheat, barley and figs, using primarily the PY Ab and Fn series, and

KN Am 819 (but she does not accept PY An 128 as proof of the 2:1 proportional value between barley and wheat, cf. fn. 51). De Fidio also thoroughly reviews the conflicting reconstructions of the ration systems put forward by Palmer and Chadwick. Palmer established a complex scale of rations based on status, sex and age grades, following Near Eastern parallels, while Chadwick presented a much simpler system wherein the rations for a man and for a woman are nearly equal. After exploring the weaknesses and strengths in both systems, de Fidio ultimately favors Palmer's reconstruction, because of the emphasis on status found in the tablets in general, and the evidence for age grades found in the KN Ak tablets. De Fidio explores (p. 24) the possibility that barley and wheat for the purposes of rations may have been interchangeable, and later (p. 35) proposes that rations listed as GRA were actually paid out in HORD at double the amount. She then applies calorific values for monthly rations calculated in barley meal with a base rate of V 1 of meal per day for a man, and shows (pp. 36-37) that all the men, women and children of the lowest rank would have been badly undernourished.

I see some problems with this synthetic study of the ration system. First, de Fidio has drawn upon data from different administrative centers and different contexts, which overlap little or not at all, and may produce conflicting information. Do the PY Fn tablets really have the same purpose as the Ab series or KN Am 819: i.e., can we be sure these are all texts of the same administrative type? The Knossos archives have not preserved the ration texts for slave women and children, nor do the Pylos texts show rations for men according to the format of KN Am 819 or the format of the PY Ab series. The terminology of the age grade system at Knossos does not appear at Pylos, although Hiller (in this volume) and Chadwick (in *Studies Bennett,* pp. 43-95) have shown that there was a change of age grade when girls and boys left their mothers. The formula *ko-wo* VIR which appears on PY Ad 326 may refer to age grade of *ko-wo me-zo-e* or VIR at Knossos, thus eliminating one age grade. In KN Am 819, which lists barley rations for a month for 18 VIR and 8 *ko-wo,* no age grade designations are given for the boys. However, Palmer's reconstruction of the ration values, followed by de Fidio, requires the presence of one smaller boy, below the age of apprenticeship, and 7 larger boys, to make the numbers come out right. On the one hand, the presence of an age grade system for children at Knossos argues strongly for a ration system corresponding to the age grades. On the other hand, evidence for this complex age grade system or a corresponding ration system does not appear at Pylos, and the terminology of KN Am 819 does not reflect age grades. Based on this extremely inconclusive evidence, I put forward two alternative explanations: either Pylos did not use the same age grade or rationing system that Knossos did, or the Knossos age grade system may not have been reflected in the ration system. Moreover, the Mesopotamian parallels present their own inconsistencies, as the absolute value of the *qa* unit is not set, and the low rations for the women and children may have been supplemented by food from the men's higher rations. For a further discussion of the problems involved, see Appendix III, pp. 121-124 of my article in *Minos* 24, 1989, pp. 89-124.

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E. STAVRIANOPOULOU: *Untersuchungen zur Struktur des Reiches von Pylos. Die Stellung der Ortschaften im Lichte der Linear B-Texte* (*SIMA* Pocket-Book 77), Göteborg, Paul Åströms Förlag 1989, pp. II + 252, 35 pages of tabular appendices.

Stavrianopoulou [hereafter S.] states that this study is a «leicht veränderte» version of her dissertation, written at Heidelberg under the direction of F. Gschnitzer. In it she collects the information available about the location of places mentioned in the Pylos tablets, their function in the economy of the kingdom, and relations between the district centers both to each other and to their subsidiary towns, and between the two provinces. Each relevant series receives a separate treatment; subsequent chapters address topics for which the data is more scattered, then summarize the results.

As in many dissertations, much of the emphasis is on presenting and sifting the results of previous scholarship. The data are ordered by calculating totals and percentages of contributions, goods handled, craftsmen and the like, but too often this exercise seems an end in itself, rather than leading to new insights. S. has not been able to break new ground in most areas, so the chief value of the book must lie in the convenience of collecting the relevant material and summarizing the state of research at the end of 1988. However, a word of caution is necessary. Errors of detail are pervasive, especially numerical errors, and only sporadically is it noted whether a figure is complete, incomplete, or wholly or partially restored. Thus the book cannot be used without constant reference to *PTT.* Where so much space is devoted to statistical analysis, this is a particularly serious failing. The following paragraphs outline the subject matter of each chapter after the introduction, along with some specific comments.

Chapter II: Districts and provinces (Ma series)

Previous theories about the grouping of towns on the Ma tablets, and the means by which taxes were assessed, are surveyed, with some well-taken objections to each (pp. 18-21). S.'s own contribution is to concentrate more closely on the exemptions allowed to groups at various district centers, with Killen's classification («Last Year's Debts on the Pylos Ma Tablets», *SMEA* 25, 1984, pp. 173-188) as a starting point. She argues in particular that, proceeding from extant figures, the difference between the total tax assessed and the total minus exemptions is twice the taxation unit for each commodity. Unfortunately, the calculations are seriously flawed in several respects. Editors have traditionally restored missing figures according to an observed ratio among commodities of 7:7:2:3:1.5:150 taxation units; S. treats this policy inconsistently in her tabulation (Appendices I and II). For example, she accepts some restored figures, but without using brackets to indicate their status. In other cases, she uses the extant figure, even when it is clearly incomplete. [400] *ME* is restored for *ro-u-so* where there is no break, but a blank space, on the tablet. Furthermore, even her own figures do not support her contention that the exemptions total two taxation units. Only one commodity (**146*) in the Hither Province and one **152* in the Further Province do show exactly this amount, and the former depends on an error in arithmetic (17-1 = 18 for *ro-u-so*)! It is worth reproducing her results, with corrected figures shown in parentheses: **146*: 14 = 2 × 7 (but faulty arithmetic; actually 16); *KE*: 1 ≠ 2 × 2 (actually 1.5): **152*: 7 ≠ 2 × 3 (correct, unless the assessment 22 for *pi-*82* is an error for 12, which fits the ratio): *O*: 2 ≠ 2 × 1.5; *ME*: 196 ≠ 2 × 150 (actually the correct figure is unknown, because the exemption for *pe-to-no* falls in a break and cannot be restored; S. is wrong to assume no exemption). For the FP: **146*: 13 ≠ 2 × 7; *KE* 3 ≠ 2 × 2; **152*: 6 = 2 × 3 (one exemption is restored in brackets); *O*: 2 ≠ 2 × 1.5; *ME*: 245 ≠ 2 × 150 (actual figure probably 255 or 265, depending on the exemption for *a*-[.]-ta_2). The data just presented derive from the tables. Presentation in the text (pp. 14, 16-17) contains several further errors, and readers should check all figures for themselves. One also misses a discussion of the *a-ne-ta-de* entry on Ma 393; if the word indicates a remission of tax (*Docs.*[2] glossary) this should perhaps be included among the exemptions. The figures thus do not support S.'s contention that a meaningful order of district centers different from that on Jn 829 results from listing them by taxes paid after exemptions. The significance of this new order is not pursued.

Chapter III: Animal husbandry (Cn series)

The nature of the different types of flock tablets is outlined, and the number of animals for each district is determined, but S. has been unable to add much to the work that has already been done on these texts. She follows previous scholars in assigning smaller places to particular districts based on their associations in the tablets. Spot-checking of the tables reveals some of the same kinds of errors as those observed for the Ma tablets: failure to distinguish whole from incomplete figures, inconsistency in treating bracketed (i.e. restored) figures, and lack of attention to the notes in *PTT.* For example, in Appendix VII, detailing numbers of flocks for different 'collectors', one total for *a-ke-o-jo* depends on taking]1̣4 in Cn 655.12 as complete, and assuming the ideogram OVIS^{m}. Actually the ideogram is missing, and the probable figure is 44. Another requires the restoration of a missing place name; in a third case 90 is a misprint for 100.

Chapter IV: Flax production (Na series)

In general the presentation of material is straightforward. The tablets are classified in two groups, those with and those without exemptions of some kind. The former is subdivided according to beneficiary. However, the assignment of tablets to the various categories (notes 163-170) is not reliable. Some (e.g. Na 66, 537) are omitted; for 14 (of 48) tablets assigned to the group with no exemptions, too little remains of the tablet to permit this judgment (e.g. Na 342:]*SA* 10). The Xa tablets now reclassified Na are omitted altogether from consideration. Most of the chapter concerns the assignment of place names to provinces. Like others before her, S. observes that such an ascription is possible in only about half the cases. The details of her presentation must be used with great caution. For example, in a list of place names and their links, if any, to other towns (pp. 59-63), which duplicates with rare exceptions the presentation of A. Sainer, «An Index of the Place Names at Pylos», *SMEA* 17, 1976, pp. 17-63, she assigns 2 to the Further and 20 to the Hither Province. Subsequent discussion cites the figures as 3 and 21 respectively. In the case of the Further Province the additional place name is *qe-re-me-e* (p. 65, without explanation). She then totals the extant amounts of flax for these assigned towns (the figure for the Hither Province is wrongly given [p. 65] as 601 *SA*; it should be 610 *SA*, or with the new reading of 4̣2̣ on Na 1054, 612; the figure for the three towns of the Further Province [p. 65] is 94 *SA*, subsequently misstated as 148). The errors in detail are easily enough corrected. A more serious concern is that S. uses these figures to prove that the HP was more heavily involved in flax production than the FP. The partial nature of the figures used makes this conclusion totally unwarranted (though it is properly inferred from the totalling Ng tablets), and is symptomatic of the improper use of information which pervades the book. S.'s own contribution in this chapter is an attempt to determine the flax assessments of various districts in the Hither Province. This is done by assigning minor towns to the major districts based on associations between them in other series. For example, *e-ko-me-no* appears with several places on Cn 40 and Cn 599 which S. linked (p. 37) with *pi-*82,* and *re-u-ko-to-ro* appears with it on Ma 225. The figures for all three places on the Na tablets are thus added, for a total of 94 (wrongly printed as 96) *SA*. The assumed links in themselves are generally accepted, except for the association of *re-u-ko-to-ro* with *pi-*82,* which involves S.'s conclusion (Chapter V) that this is not the capital of the Further Province (see below). The reasoning behind the associations is not expressed, but may usually be inferred in cases (unfortunately not all) when tablet references are given. For instance *ku-]pa-ri-so* is assigned to the district of *me-ta-pa* on the basis of An 657; *me-ta-pa* does not appear there, but presumably this location is inferred from the ordering of the *o-ka* tablets. The district flax assessments thus arrived at bear no relation to the proportions observed on Cn 608 and Vn 20, which is not at all surprising given the specialized nature of flax production. The case for associating certain towns with certain districts is nevertheless of interest. If S. is right, *a-ke-re-wa* emerges as the district most heavily involved in flax production.

Chapter V: Textile work (Aa/Ab series)

Presentation of this material generally follows previous scholarship, but one suggestion deserves some attention: the argument that Leuktron is not, as generally believed, the capital of the Further Province, but a subsidiary town in the district of *pi-*82.* The positive evidence cited for its inclusion in the Hither Province is Ma 225, where the name appears with *pi-*82,* and Mn 456, where the places which can be localized are all in the Hither Province. As negative arguments, S. notes that the rather ordinary activities attested do not suggest the special status of a capital city, and she counters E. L. Bennett's thesis («Correspondances entre les textes des tablettes pyliennes des séries Aa, Ab et Ad», in *Études Mycéniennes,* pp. 121-136) that relevant Ad tablets and Wa 114 link the set Aa 60-98 with the Further Province and identify Leuktron as its capital. Her reasoning is, first, that the omission of Pylos from the Aa series is unsurprising, but a second omission of Leuktron is hard to accept, in the absence of support from other tablets. Second, none of the place names in the set Aa 60-98 is otherwise linked to the Further Province. This statement involves rejecting the common belief that *pu-ro* on Aa 61 is an abbreviation for *pu-ro ra-u-ra-ti-ja* on the corresponding tablet Ad 664. Third, the Further Province connection of set Aa 60-98, and its scribe, Hand 4, comes through the label found with them, Wa 114. Bennett originally ascribed it to Hand 4 (J. Chadwick, *BICS* 1958, p. 2); it soon became clear that the label is by Hand 1 Stylus 240, along with the other set of Aa tablets. Chadwick was not troubled by the removal of this piece of support for the traditional view (or by the other objections just noted, which he considers in a footnote), and in fact the association is still strong because the label and Aa 60-98, were found together, away from nearly all the remaining Aa tablets. S's contention that Wa 114 is not a label for the set in question, but merely refers to a group of women from the Further Province, fails to take into account the evidence of the find-spots. This evidence, and the appearance of *re-u-ko-to-ro* on Ad tablets corresponding to this set, in contrast with *pu-ro* on Ad tablets corresponding to the other Aa tablets, still seem to me more persuasive than S.'s counter-arguments. Finally it may be noted in passing that *re-u-ko-to-ro* appears with the Further Province district center *sa-ma-ra* on An 35.3; Pylos and the Hither Province town *me-te-to* occur together in line 2.

Chapter VI: Bronze work (Jn series)

Following the work of M. Lang, «Jn Formulas and Groups», *Hesperia* 35, 1966, pp. 397-412, S. analyzes the work of smiths, calculating the numbers and percentages of *ta-ra-si-ja* and other smiths and the amounts of bronze worked at different sites. The locations of these places are then considered, and comparisons drawn between the amount of bronze working documented for the two provinces, and for district centers versus smaller towns. The rarity of district centers shows the same high degree of specialization and decentralization that was observed for flax production. Numerical errors are frequent, especially in the tallying of towns by province, and readers should verify the statistics for themselves, though the mistakes are not large enough to affect the argument.

Chapter VII: Other craft activities

The role of individual towns in various crafts is considered, and again specialization is stressed. For example, S. states (p. 118) that most of the places mentioned in this chapter were home to only one activity. This view is contradicted, however, by Appendix XI, where, apart from Pylos itself, 11 of 20 entries show several activities. It is not clear why only four of these are singled out in the text as exceptions. One of them, *re-ka-ta-ne,* is noted in the list (pp. 116-117) as involved in three different crafts; five are mentioned in the text (p. 118), and Appendix XIV gives it six. While it is true that most such activities take place in Pylos itself, the dispersal of others is not as great as S. asserts.

Ch. VIII: Cult places

This chapter addresses the issue of a temple economy in three areas: landholding, animal husbandry and craftwork. She notes that it is not always possible to tell whether a distinction is felt by the palace between a cult-place functioning as such, and the same functioning as an ordinary town. A discussion of Amnisos on the Knossos tablets, where such a distinction does seem to exist, might have been helpful here. It is surprising to find no mention of «Potnian» perfumers alongside the discussion of Potnian smiths. Some attention is paid to Near Eastern evidence, but closer comparanda from Thebes and Knossos, while outside the strict limits of a book on Pylos, provide important analogies and should have been included. So should the ambiguous reference to Potnia in a tablet (An 1281) from the Northeast Workshop at Pylos itself. In this connection one should add to the bibliography I. Tegyey's article, «The Northeast Workshop at Pylos», in *Pylos Comes Alive,* pp. 65-79.

Chapter IX: Military obligations

This chapter documents further the link between land allotments and military service, and the special status of *pa-ki-ja-na*.

Chapter X: Geography of the Pylos kingdom

S. combines the twin sources of tablets and archaeology to summarize what is known about probable locations of the district centers. She follows Chadwick's analysis of Pylian geography in general, and his chapter in W. A. McDonald and G. R. Rapp, Jr., *The Minnesota Messenia Expedition,* Minneapolis 1972, chapter 7. The districts are listed with a summary of indications of location and craft activities (pp. 137-138), as well as smaller places associated with them.

Chapter XI: General analysis

Here S. sums up the findings of previous chapters. The results yield (pp. 153-154) a tally of numbers of places involved in various activities, and S. groups the towns according to their involvement in one, two or three activities. The last group comprises both district centers and smaller places; S. suggests that each center might have a cult place, as we know to be true of *pa-ki-ja-na* and *ro-u-so* (*ro-u-si-jo a-ko-ro*), as well as storage facilities (Ma series). Workshops, though, are not generally situated there, but in the smaller towns. (For the suggestion that some of these smaller place names might refer instead to quarters of the centers themselves, see M. Lang, «Pylian Place-Names», in *Studies Bennett,* pp. 185-212). Some indication of the size of each district is derived from such clues as the number of animals steaded there; this may be true of area, though not of population (a point inferred on p. 157). The Hither Province is seen as more heavily involved than the Further Province in animal husbandry, by a factor of 7:1 (p. 148). This ratio was shown in Chapter III (p. 46) to be 4:1, not 7:1; the reason for the discrepancy is unclear. Further, the difference is not necessarily significant, as not all places can be assigned to a province (the same objection was raised in the discussion of flax production above). Finally, S. suggests an explanation for this perceived discrepancy between provinces. S. observes that the Further Province is mentioned less often. However, rather than postulating a second archive, now missing, she argues that the importance of a district to the palace rested not on its production capacity, but on other activities. Thus the Further Province, relying more heavily on agriculture, was not as carefully monitored as the Hither Province, where more diversity of activity is recorded.

It is clear that a lot of study went into the preparation of this book. Unfortunately, the frequent numerical and statistical errors will make it difficult for a novice to use, and most of the material will be familiar to previous students of the tablets. By the same token, S.'s new suggestions are not always persuasive because the evidence on which they rest is not reliable. However, if used with caution, the book will have some value as a compendium of the information available about places named on the Pylos tablets, their locations and the economic relations among them.

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MARTÍN S. RUIPÉREZ and JOSÉ LUIS MELENA: *Los griegos micénicos* (*Biblioteca historia* 16), Madrid 1990, pp. 267. 850 ptas.

This book, although intended as an introduction for a general audience, offers much new information of interest to scholars of Mycenaean Greece. The authors provide an historical overview of Mycenaean palatial civilization based primarily on the material contained in the Linear B tablets from mainland Greece and Crete. Of considerable value is their discussion of the most recent discoveries and trends in Mycenaean studies, wherein they rightly emphasize the importance of an interdisciplinary approach to the texts which considers palaeography, archaeological context and archival administration. The most significant recent advances in the field discussed in this book have been achieved by using such methods, and the authors indicate that many more such discoveries will be possible if this trend continues. In order to extract any further information from the texts, they say, we need to turn our studies in this direction.

The book consists of eleven chapters in which the authors provide the reader with general historical background and then discuss various specific aspects of Mycenaean studies, such as geography, economy, political and social structure. The first chapter presents a general archaeological overview of the Aegean area throughout the Bronze Age, including such topics as the transition from the Neolithic period, relations with the Near East, the most recent problems concerning the date of the Thera eruption, and the ethnic composition of the Greek people. A chapter follows surveying the various Bronze Age writing systems employed within the later Mycenaean Greek sphere, including Cretan Hieroglyphic, Linear A and Cypro-Minoan. Chapter 3, entitled «The Discoverers of the Linear B Texts», contains a brief yet comprehensive survey not only of the excavations of the Bronze Age palatial sites —including the very recent work at Khania— but also of the long and bitter debate over the date of the Knossos tablets. Jan Driessen's new work, which has done much to clarify the evidence involved in this controversy, is outlined by the authors, who also discuss clearly his theory of three destruction phases at Knossos, dating from LM II through LM III B. An excellent overview of the study of Mycenaean texts is given in the fourth chapter. Beginning with their definition of a «palace» as «a union of dependencies around a central power, sustained by the accumulation of surplus resources, and engaged in the production of luxury and military items» (p. 50), the authors then explain the various physical characteristics of Mycenaean palatial archives. They discuss the manufacture of tablets, the identification of scribal hands and various types of administrative units, the types of documents (page-shaped, leaf, labels, sealings, stirrup jars), tablet sets, and the classification of tablets into separate series according to subject. Melena makes use of this opportunity to introduce perhaps the most recent innovation in our examination of the physical characteristics of the tests, the study of «contiguities» (p. 60). Melena, T. G. Palaima and E. L. Bennett, Jr., have recently observed that it is possible to determine the positions individual tablets had within their various storage «baskets» by examining the color, breaklines, wicker marks and shapes of documents in the same series. We may expect to hear more about this important discovery, with its implications for our understanding of the archival filing system, as well as the added knowledge it may provide us concerning the relative order of tablets in certain series (Palaima's recent work on the Pylian Sh series is the most obvious example at present).

After two more chapters concerning first the decipherment of Linear B and then the Mycenaean dialect, the authors present in-depth studies of many of the current areas of interest in Linear B studies. These chapters present all the relevant information and methodology in a clear and thorough manner. They are therefore valuable not only as an introduction for the novice but also as a review for the scholar already familiar with these questions. For example, the chapter on Mycenaean geography concentrates first on Pylos, then on Knossos, analyzing the different kinds of information used as evidence for locating toponyms in the respective areas of palatial domain (Pylian province lists versus Knossian toponymic pairs, etc.). The various terms indicating a «social hierarchy» are discussed individually in Chapter 8, then several key tablets relating to this topic are examined and interpreted. The chapter (9) on Mycenaean economy is perhaps the most valuable of the whole work. Here in eleven individual sections Ruipérez and Melena systematically survey almost every conceivable element of the palatial economy, including agricultural topics, animal husbandry, crafts and the textile industry. The two final chapters on religion and on military matters round out this thorough introduction to the Mycenaean period in Greece.

Mycenaean specialists will be most intrigued by the discussions of topics hitherto unexplored. For example, Ruipérez and Melena raise the question, pointing to an enigmatic series of signs on PY Aq 218 and PY Xa 421, of the existence of an ordinal system of the Linear B signs, similar to our «alphabetical order», which would be useful for memorization and also account for the invariable order of certain lists in the Linear B documents, where words beginning with the same sign occur next to each other. In their discussion of place-names and geography, the authors examine for the first time place-names in the Theban tablets and sealings, and are able to reconstruct a wide sphere of Theban influence. They observe that place-names occur in a much higher percentage in the Thebes texts than in those from any other site, perhaps indicating a more decentralized administrative system. In their chapter on Mycenaean economy, the authors offer several topics for further exploration, including the roles of hunting and the tanning industry in the texts. A further problem they pose to the reader is that of the possible existence of some common form of currency or means of exchange in Greece, as evidenced perhaps by the term *o-no* («burden») on certain texts (p. 80).

Although the contents of the book are excellent, there are several problems with its presentation. There are only two photographs in the entire work, and these are of the distinguished-looking authors! There are no more than eleven drawings/charts, and while these are well chosen (two maps, drawings of sample texts, charts of the Linear B syllabary and its ideographic repertory), one could certainly wish for more illustrations in an introductory text, which would enable the reader to understand the plans of the various palaces, the actual layout of several key tablets, the archaeological contexts in which tablets and materials have been discovered, and so on. There are also numerous distracting spelling errors, for which the authors disavow responsibility in a separate insert. However, the bibliography, broken down by chapters, is comprehensive, concentrating primarily on the most recent work on various topics. The thoroughness of this bibliography, in light of the expertise of the authors, is not surprising, although the citations are somewhat too condensed. There is an excellent appendix in which some ninety representative tablets from all series and sites are explicated, with some reference to where they are discussed in the main text of the book. Again, the citation of bibliography for these individual texts would have been a valuable addition.

It is obvious that this book has much to recommend it both as a general overview and as a reference work to the most recent activity in this field. Such a comprehensive and thorough introduction to Linear B studies is a most welcome update to Chadwick's *The Mycenaean World* (Cambridge 1976) and Hiller-Panagl's *Die frühgriechischen Texte aus mykenischer Zeit* (Darmstadt 1976).

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LOUIS GODART: *Le pouvoir de l'écrit. Aux pays des premières écritures,* Paris, Editions Errance 1990, pp. 240.

In this book, Louis Godart surveys Aegean Bronze Age writing systems. He discusses where, how and why they developed, describes their rediscovery by the modern world, and explains the attempts made, both successful and not, at deciphering them. He also compares the development and use of Aegean scripts to the writing systems of Egypt, Mesopotamia and Anatolia. Godart argues that the invention of writing in the Aegean and similar Near Eastern civilizations is tied to the rise of the so-called 'palace' economy, a system in which a strong central power collected and redistributed the goods produced by the labor force and land under its control. Beginning with the use of simple clay sealings to monitor access to vases and storerooms and proceeding to the use of easily recognizable pictographs to record goods, the palace administrators developed an increasingly more complex accounting system. This eventually took the form of a syllabic script written on clay tablets capable of expressing abstract concepts. Writing was [p. 8] «une sorte de sceptre d'argile consentant aux administrateurs des palais de commander sur des hommes et de contrôler des provinces».

This book is aimed at the intelligent general reader and does not require any prior knowledge of Greek or Aegean Bronze Age studies. Yet even professional Aegeanists will find it useful as an all-in-one source for concise, clear descriptions and summaries of all Aegean Bronze Age writing systems. As such it would even be useful as an introduction for a graduate seminar on Aegean scripts. Godart writes engagingly, sprinkling his narrative with amusing anecdotes, and is adept at presenting complex technical material such as Ventris's decipherment of Linear B so that it is easy to understand. Illustrations include well-chosen and convenient drawings, maps, and color and black and white photographs. The color photographs of objects like libation vessels, tablets and sealings are especially striking in detail and quality.

Godart begins with a brief history of archaeology and the rebirth of interest in the ancient world following the discovery of Pompeii in the eighteenth century.

Then he describes in separate, concise chapters the discovery, nature and decipherment of the writing systems of Egypt, the Near East and Anatolia. He then turns to the Aegean world, devoting a chapter to the contributions of Schliemann, Evans and Ventris in opening up Bronze Age studies. Ventris's decipherment of Linear B receives special attention. Godart gives a general outline of the methods and techniques of decipherment before taking the reader through the stage-setting contributions of A. E. Cowley, Alice Kober and Emmett L. Bennett, Jr. concluding with a clear and succinct explanation of Ventris's breakthrough. He makes good use of explanatory charts, including most notably two grids from Ventris's work notes.

The rest of the book is divided roughly into two parts. The first contains Godart's views on the nature of Mycenaean society as reflected in the Linear B texts. These are restricted chiefly to observations on the possible relationship (1) among the Mycenaean palace sites on the Greek mainland, (2) between the mainland sites and Crete, (3) between the Mycenaeans and the rest of the world, and how these relationships may have affected the origin, the development, and the use of writing in the Mycenaean world. Also examined in this context are the possible references to Mycenaeans and Minoans in contemporary Egyptian documents. Briefly, Godart suggests that Linear B came into existence on the mainland, probably at Mycenae, during the period of economic growth exemplified by the rich contents of the Shaft Graves, c. 1700 B.C. By 1450 B.C. a trade rivalry between the Minoans and the Mycenaeans culminated in the destruction of the Minoan culture and the installation of a Mycenaean *wanax* at Knossos. The competition between the mainland and Crete resumed and Crete was defeated again about 50 years later; after 1370 it was no longer mentioned in Egyptian records. Throughout Godart stresses the hypothetical nature of his views and on occasion presents alternative possibilities. In a future edition Godart will have to take into account work on the different dates for various archives at Knossos by Jan Driessen (*An Early Destruction in the Mycenaean Palace at Knossos,* Acta Archaeologica Lovaniensia Monographiae 2 [Leuven 1990]).

The second part, entitled «Derrière le rideau», examines the various as yet undeciphered scripts: Cretan Hieroglyphic, Linear A, and the Bronze Age Cypriot scripts. Included is a discussion of the Phaistos disc. Godart gives a compact and detailed description of each script furnishing such information as the number of texts, a catalogue of the types of inscriptions and the total number of signs in the extant corpus, as well as analyzing the problems associated with its decipherment. He pays particular attention to the origin and development of Cretan Hieroglyphic and Linear A, their differences and similarities, and what their respective roles in Minoan society might have been.

The last section of the book sketches the possible economic and social parallels between Mycenaean and Sumerian civilizations. It also includes a brief analysis of Mycenaean society in the light of Dumézil's work on Indo-European cultures.

In general Godart gives fair summaries of topics that often are controversial among specialists. He is usually careful to make clear when he is expressing his own opinion as opposed to scholarly consensus. The need for clarity when writing for a general, non-specialist audience often makes it impossible to investigate and thoroughly present all sides of a difficult issue. But even if one does not agree with Godart's theories, his book remains useful and enjoyable. I make only two suggestions for improvement: (1) add to the bibliography the standard editions of the

Linear B tablets and other basic reference tools such as the *Index Généraux du Linéaire B*; (2) include fuller explanations of more actual Linear B texts. An expanded discussion of KN Fp 1, for example, which is briefly translated and mentioned only to show Mycenaean parallels with Near Eastern texts, could very effectively illustrate many points Godart raises about tablet formulae and format, the ties between Mycenaean and later Greek culture, and Minoan influence on Mycenaean culture and writing.

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Scavi a Nerokouro, Kydonias I. Richerche greco-italiane in Creta occidentale (Incunabula Graeca, 91), Rome 1989. Pp. 339, fully illustrated.

This quality volume, with contributions by A. Kanta, L. Rochetti, L. Vagnetti, A. Christopoulou, I. Tzedakis, V. Francavglia, L. Godart, D. Monna and P. Signanini, forms the first of a series of monographs centring on the Greek-Italian Excavations of the site of Nerokouro, one of the rare Minoan settlements excavated up to this date in the hinterland of Khania in West Crete. It may be regarded as a timely publication, the excavations having taken place between 1976 and 1982, although printer's difficulties and problems with the distribution of the work, still make the publication hard to acquire.

The Nerokouro building —or «villa» as it is called by the authors— received some minor attention by its excavators in the past, such as the report in *SMEA* 19, 1978, pp. 7-10 and a good discussion of its architectural phases and peculiarities was given by I. Tzedakis and S. Chrysoulaki in the *Function of the Minoan Palaces* (1987). The latter report is now substantiated by the presentation of the pottery of the building, which is fully treated by A. Kanta and L. Rochetti. This has to be applauded since there are only a few neopalatial domestic pottery deposits published as complete as the one presented in this study. The dating evidence presented for a MM III habitation of the site is convincing (although I am still puzzled whether the authors mean MM IIIA in Knossian terms or not! and whether this pottery could not be associated with an earlier pre-«villa» occupation of the site). The building is, as so many other Cretan structures, violently destroyed by fire in LM IB. Perhaps the authors could have stressed the presence of a goblet fragment as well as a complete alabastron (which, if it is Cretan, as the authors seem to believe, is the oldest example on the island) in the destruction deposits, since they could have some historical importance. May these be regarded as indicating a slightly later destruction date for the Nerokouro complex than we are made to believe? If so, this may agree with recently collected evidence from the other extreme of the island, at Palaikastro.

Except for the Neolithic and Minoan pottery, we are also offered a concise overview of the stratigraphy, the chronology and the economy of the site together with some historical conclusions. We have to await the publication of the architecture as well as the other remains found at Nerokouro to form a definite view whether this building is really an isolated independent structure, a 'villa' or 'countryhouse'. The present reviewer doubts that such buildings existed in Neopalatial times. Interesting is the observation that, in its final, LM IB, phase, the building lost its former glory and was transformed into some kind of industrial estate, as is the case at Vathypetro. Perhaps these two sites are not unique: as has recently been suggested for Knossos by C. F. Macdonald, the palace in LM IB seems to be in a state of repair. Elsewhere, such as in the palaces of Malia and Phaistos, the same may be true, which would explain the absence of prestige goods. Could it be that Crete, after the Santorini eruption, was indeed trying to recover and concentrating on food production, an easy prey for marauding Mainlanders?

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