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K.-E. SJÖQUIST & P. ÅSTRÖM: *Knossos: Keepers and Kneaders* with an appendix by J.-P. Olivier (*SIMA* Pocket-Book 82), Göteborg, Paul Åströms Förlag 1991, pp. 160, 36 figs.

J. T. HOOKER intro., *Reading the Past: Ancient Writing from Cuneiform to the Alphabet,* Berkeley, University of California Press/British Museum 1991, pp. 384, 268 illustrations. $29.95 US.

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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$^{M?\ or\ F?}$ (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 socio-political 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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