of synharmony, the key to Maya writing.

J. E. S. Thompson, an eminent Maya scholar, has rejected Knorosov's claim with the following statement (*Maya Hieroglyphic Writing*, University of Oklahoma Press, ed. 3, 1971, p. vi):

> With a phonetic system, as with breaking a code, the rate of decipherment accelerates with each newly established reading. It is now nineteen years since it was announced with such a fanfare of the trumpets of tabarded heralds of the U.S.S.R. that after nearly a century of abortive bourgeois effort, the problem had been solved by this Marxist-Leninist approach. I would gladly make a pilgrimage to Marx's grave in Highgate Cemetery to give thanks, were that really so. Alas! The first flow of alleged decipherments has not swollen to a river, as it should with the successful solving of a phonetic system; it has long since dried up.

Despite Thompson's criticism, Lounsbury offers a phonetic reading of a common hieroglyphic prefix, the "Ben-Ich" glyph, based to a certain extent on Knorosov's approach. This "Ben-Ich" prefix is composed of two elements, which Thompson has read as *Ah*, or lord. Lounsbury maintains that the *Ah* applies only to the Ben element, and he reads the second element, Ich, as *po*. Lounsbury bases this *po* reading on (i) the doubled prefix for the month Pop, which was recorded by the 16th-century bishop of Yucatan, D. Landa, and (ii) Knorosov's reading of *mo* or *(o)m* for a dotted circle around a point. When the Ich element appears inside the dotted circle, Lounsbury reads it as *pom*, the Maya word for "copal" or ball of incense. Lounsbury thus reads the Ben-Ich elements as *Ah po* or *Ah Pop*, which we know was a title of rank among various Maya groups, but so was *Ahau*. Accepting Thompson's reading of *Ah* for both elements resolves the problem of the flexible ordering of the two elements, because both Ben-Ich and Ich-Ben occur.

Lounsbury is in error when he claims (p. 136) that the earliest occurrences of the Ben-Ich prefix are in the 6th century A.D. at Tikal. There are earlier occurrences at the same site on Stela 31 (A.D. 445), where the word order is Ich-Ben and if we phonetically read it as *po ah* it makes no sense. Lounsbury does keep the door open for *Ah po(p)* or *Ahau*. Lounsbury's argument is extremely persuasive in spite of its problems, but the extent of phoneticism will remain controversial.

A new direction of Maya hieroglyphic research is suggested by T. Proskouriakoff in "The *hand-grasping-fish* and associated glyphs on Classic Maya monuments." Her topic involves studying those hieroglyphs which accompany representations of ritual acts. For instance, Proskouriakoff shows that the *hand-grasping-fish* glyph is associated with depictions of women performing the bloodletting rite on lintels at the site of Yaxchilán and that the *shell-fist* glyph is used with posthumous records. Proskouriakoff suggests that if some temples had a funerary purpose and were dedicated to departed lords, this might explain the fact that the *hand-grasping-fish* glyph occurs on lintels and not on stelae in the Petén. After having directed us to the historical approach, Proskouriakoff is now showing us how the same methodology—studying glyphs in conjunction with scenes—can provide information on ritual.

D. H. Kelley and K. A. Kerr in "Mayan astronomy and astronomical glyphs" return to the relationships among history, cosmology, and astronomy. After much recent emphasis on the historical approach, this attention to astronomy is welcome. While the dates the Maya recorded have historical importance, showing for example the accession to the throne, it remains for us to determine whether the same dates might have been "beneficent" with special attributes in Maya cosmology.

G. Kubler in "The clauses of Classic Maya inscriptions" makes three statements that are likely to create some controversy: (i) that the historical texts of various sites are abbreviated memory aids; (ii) that the compact notational system appears to be more ideographic than phonetic; and (iii) that the original purpose of the Maya scholars who wrote the inscriptions was to "make the meaning clear to the farmer from the fields regardless of dialect, all the while loading the statement with esoteric meaning for the learned few" (p. 162). It seems very unlikely to me that the average farmer could "read" the inscriptions. It is perhaps because he could not that the information provided in the texts is usually replicated in the scene, and if the farmer ever saw these inscriptions (or was allowed in the main plaza of the site at all) he would probably comprehend only the scene.

Clearly the extent of phoneticism in pre-Columbian writing still represents a frontier for research.

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A Shift in the Archeology of Britain

British Prehistory. A New Outline. COLIN RENFREW, Ed. Noyes, Park Ridge, N.J., 1975. xiv, 348 pp., illus. $20.

This book has a simple and straightforward aim: to take stock of the changes in our view of British prehistory that have occurred in the 30 years since the last attempt to survey the subject (V. G. Childe's *Prehistoric Communities of the British Isles*, Chambers, London, ed. 2, 1947) was made (p. xi) and "to examine and summarise the existing evidence" (p. xiii). Renfrew opens with a chapter that skillfully outlines the changes, and there follows a chronological succession of chapters, from the Paleolithic to the Iron Age, all by researchers of prominence and repute in their fields. Bearing in mind that this book will have a readership ranging from practicing professionals to interested amateurs, it is remarkable how well the authors have blended introductory survey information with some quite detailed argument. Of course, there are issues that any reviewer could debate, but none so badly handled as to detract seriously from the general success that the book deserves.

There is one major interpretative theme that does deserve discussion, however, for it appears in nearly every chapter. This is the shift away from diffusion toward internal development as an explanatory mechanism for cultural changes. The dramatic lengthening of the Neolithic is due to the establishment of an increasingly coherent radiocarbon chronology, and this alone has been responsible for most of the reevaluation in this period. But for later periods the changes brought by radiocarbon age determination have been much less, and reconsideration of the material evidence, together with reexamination of classificatory schemes, has been more influential. The earlier stages of this general trend were summarized (and taken further) by Grahame Clark ("The invasion hypothesis in British archaeology," *Antiquity* **40**, No. 159, p. 172) in 1966, and the movement has been sustained since. However, despite accumulating evidence pointing toward a greater degree of insular continuity, two things emerge from this book: that substantial intrusive elements are still recognized, however reluctantly, and that these postulated intrusive elements, immigrations and invasions, seem to become more frequent the later in time we get.

Many of the formerly postulated intrusions of the Neolithic are dispensed with now and, particularly for the multiplicity of megalithic tombs and cairns, well-substantiated sequences of internal development now replace the successive waves of migrants and invaders each with their own tomb preferences. But still, for example, Smith (p. 126) and Henshall (p. 152) cannot escape the Continental paral-

lels for the passage-grave series. Neither author explicitly admits intrusion from the Continent, but the (current) European chronology for passage-graves does not suggest that the British and Irish examples were the earliest, and a Continental origin in fact remains probable. The "Beaker" phase remains unavoidably attributable to intrusions from the Continent, and to some now seems an even more complex welter of intrusions than formerly! Burgess concludes that there was limited immigration from Brittany in the Early Bronze Age (p. 187), and he cautiously suggests further limited immigration from northern France at the beginning of the Middle Bronze Age (p. 217). Around 1100 B.C. he suggests an incursion of "Urn-Field" warriors from the Continent (p. 207), and in the 7th century B.C. an incursion of raiding/invading Hallstatt warriors (pp. 211–213). Burgess is not enthusiastic about explanations based on invasions, but feels that the available evidence supports these interpretations. For the Iron Age (chapter 6) Cunliffe provides a quick introduction to the historical assumptions that first furnished the "invasion model" for later British prehistory, and to its application to the Iron Age from the 1890's to the 1960's. He appears even less enthusiastic about invasions than Burgess. Nevertheless we have references to La Tène incursions in the 5th to 4th centuries B.C. (p. 255) and to the Belgic invasions in the 2nd century B.C. (p. 257). These last are referred to by Caesar and are not denied by the archeological evidence. Thus, for all the right, proper, and usually justified reaction against the former almost universal application of the "invasion hypothesis," it stubbornly remains, however its eminence is attenuated.

Why should this historically derived model appear more frequently justifiable and more difficult to avoid as we move later in time? Some maintain this to be a reflection of ethnographic reality in that later prehistoric groups, being, it can be argued, larger and more organized politically, had more incentive to migrate and were better able to exploit conquered territory (and peoples?). This viewpoint is certainly plausible and deserves respect. But it also can be argued that, as the archeological record approximates more and more closely to historically known societies, it can be interpreted in some respects with rather less ambiguity. Thus, in the 1st millennium B.C., the prehistoric archeology of Britain takes on an increasingly familiar aspect. Hill-forts, bronze and iron weaponry in increasingly effective and diverse forms, and linguistic reconstructions indicative of population movements, not to mention the earlier classical references to incursions of northern "barbarians" into the Mediterranean world, all promote an interpretative climate in which invasions are indeed plausible agents of cultural change. With far less adequate historical or ethnographic models for comparison, the far less familiar Britain of the 4th and 3rd millennia B.C. may appear less amenable to the "invasion hypothesis" partly because it is, simply, less familiar.

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Prehistory in France

France before the Romans. STUART PIGGOTT, GLYN DANIEL, and CHARLES MCBURNEY, Eds. Noyes, Park Ridge, N.J., 1975. 240 pp., illus. $28.

This synthesis for students and scholars of the archeological data for the proto- and prehistory of France is marked by four chapters translated from the French that provide English readers with the rich detail of French archeology. Contributions by eight authors are arranged chronologically into seven chapters that begin with the Lower Paleolithic and end with Roman Gaul. Editorial comments by Charles McBurney are incorporated as footnotes into two chapters, and comments by the other two editors form a summary chapter; these provide additional views, for example on the origin of the Upper Paleolithic Solutrean beyond France and the astronomical significance of Brittany megaliths.

The editors identify the book as culture history with an emphasis on chronicle, and make allowances for sparse explanation. As a chronicle of material culture, it stands as a welcome reference; but, with reconstructions of past lifeways barely discernible and explanations for culture change weak, the book is not good culture history. Stratigraphy, chronology, and typological relationships characterize the three chapters that cover the period ending in 4000 B.C. The treatment of later prehistory, likewise, focuses largely on the careful correlation in time and space of such objects as Urnfield poppyhead pins, details of funerary customs, and ceramic changes for the purpose of identifying regional and temporal divisions, and not on the socioeconomic and demographic processes that underlie the sequences.

Data for reconstructions are incomplete, admittedly, but the reader is too often left with disconnected house counts, population estimates, and seemingly endless details about funerary architecture and grave goods which, as Jacques Briard assures, say much about social structure, but which as presented permit only the most obvious and general conclusions about social stratification during the Bronze Age. For the Lower Paleolithic, McBurney fails even to mention the Acheulian base camp of Terra Amata at Nice, complete with superimposed seasonal occupation floors, shelters, and a large inventory of organic remains (see H. de Lumley, "A Paleolithic camp at Nice," *Sci. Am.* **220**, No. 5, 42 [1969]). There is also no serious attempt to discuss the subsistence-settlement system of the Mousterian, and the behavioral implications of the functional-statistical approach are lost by being bracketed between more lengthy descriptions of the typological-statistical and diachronic approaches. While technological and typological characteristics of specific Upper Paleolithic cultures are presented in detail, those of habitation, subsistence, and the like are summarily treated for this stage.

More pronounced than reconstructions are the major modes of explanation of culture change used: (i) population displacement; (ii) a biological model for industrial change; (iii) environmental conditions; and (iv) socioeconomic processes. McBurney uses the first to explain the abrupt break between the Mousterian and Upper Paleolithic; whereas Denise de Sonneville-Bordes uses the second to argue for an indigenous development of the Upper Paleolithic from the Mousterian in France, and to reaffirm her view of a genetic link between the temporally separated Perigordian I and Perigordian II on the basis of similar techniques for backing blades. The second mode is also evident in Max Escalon de Fonton's tracing of the genetic links through industrial mutations between the Azilian, Azilio-Sauveterrian, Sauveterrian, and Tardenoisian. The biological model has been discredited elsewhere (see S. R. Binford, "Early Upper Pleistocene adaptations in the Levant," *Am. Anthropol.* **70**, 707 [1968]).

The third mode is derived from Grahame Clark's study (*Prehistoric Europe: The Economic Basis*, Methuen, 1952), which first viewed European cultural changes as adaptations to dynamic post-Pleistocene environmental conditions. It is most fully developed by Escalon de Fonton to explain the indigenous development of a pastoral economy during the Mesolithic Castelnovian and Early Neolithic Cardial sequence (6000–4000 B.C.) in Provence as a response to desiccation; and by Gerard Bailloud, who relates the cultural uniformity in the French Midi and cultural diversity in north France during the Middle Neolithic, and the reverse condition during the Late Neolithic, to temperature and hu-