user about their assumptions and shortcomings, and provides a set of reasonable warnings about their use. While many archaeologists will continue to ignore these well-meaning warnings, they can no longer easily justify these lapses in practice since this book provides a useful and accessible treatment of these problems. Second, his treatment of correspondence analysis---a method little seen in the Anglo-American quantitative literature and which deals primarily with count data---is clear and is likely to lead to its greater use. Third, the bibliography is excellent, and consists of an exhaustive listing of archaeological applications of these methods. Finally, the author is always ready to (gently) criticize archaeological practice. He does so, though, in a constructive manner, and his background lends credence to his critique.

There are some aspects of the book, however, with which I have some concerns. Perhaps the most serious of these is Baxter's advocacy of data exploration. In essence, he advocates the use of these methods as a means by which archaeologists can search for hidden, or implicit, structure in their data. Although he recognizes the statistical dangers of this approach (thus his strong critique of cluster and factor analysis and preference for principal components and correspondence analysis), he never acknowledges the archaeological limitations of this highly inductive perspective on data analysis. Historically, some of the greatest abuses in the use of multivariate methods in archaeology have been due to this style of inductive data dredging, and it is unfortunate to see this approach perpetuated.

This probably helps to explain why he does not discuss at any length the relationship between theory, problem, data, and method. While he does have some discussion of data types and aspects of practice (pp. 12–24), this is mostly descriptive as to what has been done, and not why it is or should be so. Indeed, Baxter is somewhat bemused by the lengths to which some archaeologists discuss "foundational" issues in print (pp. 223–224), and humorously notes that statisticians, according to one of his mentors, worry about such issues in the "privacy of their bathtubs" (p. 223). Maintaining the metaphor, archaeologists air (or wash) their dirty laundry in public precisely because as archaeologists we are forced to worry about the anthropological significance of our interpretations. Doing statistics or any other sort of quantitative analysis is meaningless unless there is some assurance that the problem has been framed in a useful way, and the data available are congruent with that framing. Despite his good intentions, Baxter remains a statistician, and thus his book is best seen as a statistics text with many useful archaeological illustrations of the use of these methods.

If this limitation is kept in view, I nevertheless find this text to be very useful. It is written at a level that most archaeologists will find comprehensible, and it is a good companion to Shennan's book. Indeed, the two can be used profitably in tandem. I intend to use Baxter's book in my graduate course in quantitative methods, but buttressed with supporting materials that discuss those "foundational" issues he has eschewed.

Ancient American Inscriptions: Plow Marks or History. WILLIAM R. MCGLONE, PHILLIP M. LEONARD, JAMES L. GUTHRIE, ROLLIN W. GILLESPIE, and JAMES P. WHITTALL, JR. Early Sites Research Society, Sutton, Massachusetts, 1993. xvi + 415 pp., figures, tables, references, index. $19.95 (paper).

Reviewed by Bradley T. Lepper, Ohio Historical Society—Newark Works.

This book is one of the more sophisticated attempts to present a case for Old World peoples in Precolumbian America. The argument, here as elsewhere, rests almost entirely on so called epigraphic evidence, i.e., "purported inscriptions in Old World scripts found in the Americas" (p. 384). The authors clearly recognize many of the problems that have plagued epigraphic research in the past and offer a number of suggestions which, if followed, would greatly improve the quality of future efforts (p. 36).

Claims of evidence for various pre-Columbian Old World cultures in America are not new. Many are outright frauds, others are misunderstandings, still others are insufficiently reported to evaluate. Only the Norse site of L'Anse aux Meadows in Newfoundland has demonstrated a limited episode of contact prior to the landfalls of Columbus in A.D. 1492. Occasionally, archaeologists such as Stephen Williams and Kenneth Feder have addressed these claims, but, more often, the claims simply are dismissed as pseudoscience. McGlone et al. argue, not terribly persuasively, that this is unfair and set out to present a balanced summary of the evidence in support of Precolumbian contacts (pp. 63, 329).

The late Barry Fell is the father of modern claims for ancient Old World inscriptions in America. McGlone et al. pay tribute to Fell's contribution (p. 37), but recognize that "most of Fell's work" is characterized by extreme claims "without solid back-up data or well-ordered argumentation" (p. 308). Unfortunately, these same flaws mar their own research.

One of the key localities McGlone et al. present as having yielded "valid epigraphic data" (p. 138) is a series of caves in the panhandle of Oklahoma. They describe a particular panel of petroglyphs that includes figures

whom the authors interpret as the Egyptian god Anubis and a "Sun God." They identify the "Sun God," a figure with upraised hands and rays emanating from the head, as "Harpocrates, Baal, Isis, Apollo, Sol Invictus,...[or] Mithras" (p. 167) after "considering a broad spectrum of mythologies and religions" (p. 167)—except, apparently, any Native American religion.

McGlone et al. review the evidence for Ogam inscriptions in America. Ogam is an obscure early Irish alphabet composed of parallel linear markings. They present a number of useful criteria for evaluating Ogam inscriptions (p. 89), but they fail to appreciate the limitations of their data. They recognize that there is a continuum of parallel markings, beginning with some which clearly are not Ogam (p. 117) and some which the authors think might be but probably aren't (p. 218), to a few which the authors accept as true Ogam (p. 231). The significance of this continuum, which eludes McGlone et al., is that if one examines enough random parallel linear markings, some eventually will be found that fortuitously correspond to some sort of Ogam inscription, especially when the procedures used in the "translation" of the controversial vowelless American Ogam allow such extreme latitude (p. 124).

Although McGlone et al. have heard of Occam's Razor (pp. 94, 179), it is evident that they do not know how to wield it. They note in passing that "figures with upraised hands are carved on rocks throughout the world" (p. 179) and acknowledge that "vertical parallel markings" on rock "appear on a worldwide basis" (p. 112). Yet, when confronted with these phenomena in America, they seek only Old World analogs with which to interpret them. This line of argument is inherently racist, despite the authors anticipation and vehement denial of that charge (pp. 236--238).

McGlone et al. accept that the absence of artifacts attributable to the authors of the various alleged Old World inscriptions has been the principal factor in the rejection of epigraphic data by archaeologists (p. 238). But, McGlone et al. propose that only "small groups of explorers" are represented (p. 239). Therefore, so they argue, few artifacts could be expected (pp. 239--241). They also point out that no archaeologist has looked for artifacts at their rock-writing sites. These are specious arguments. The alleged visitors from the Old World (including, if we accept all of the "valid epigraphic data" put forward by these authors, "small groups" of Celts, Egyptians, Arabs, and others) didn't fall out of the sky. They had to get to the New World from somewhere and they had to have left some sort of archaeological record along the way. The absence of such a record for any Precolumbian Old World group is a compelling reason for the authors to consider alternative explanations for the petroglyphs, explanations which build on the observed similarities with indigenous Plains Indian rock art (p. 179).

McGlone et al. repeatedly call for archaeologists to take up the challenge of the epigraphic data (e.g., pp. xv, 234), but the data they present do not warrant the effort. In fact, archaeologists have studied some of the sites they champion. William Godfrey conducted excavations at the Newport Tower and reported the results of his work in the pages of this journal in 1951. He was able to demonstrate conclusively that the structure was built as a colonial windmill, but McGlone et al. cavalierly assert that the Newport Tower may have been built in the fourteenth century by a presumably small group of Knights Templar (p. 336).

McGlone et al. do not understand the strengths and weaknesses of archaeological data. They do not understand how science is done. Instead of recognizing that their extraordinary claims require extraordinary evidence, they opine that "pioneer thinkers" (such as themselves) should not be held even to ordinary standards of evidence (p. 378). They claim that there is some sort of conspiracy among archaeologists to suppress or even destroy evidence for Precolumbian Old World civilizations in America (pp. 97, 157, 326). This book is not an honest effort to bridge the gap between amateur epigraphers and professional archaeologists. There are some positive aspects to the book, but it falls far short of making a provocative case for Precolumbian Old World inscriptions at New World sites.

BOOK NOTE

Middle Paleolithic Assemblage and Settlement Variability in West-Central Jordan. JAMES M. POTTER. Anthropological Research Papers No. 45. Department of Anthropology, Arizona State University, Tempe, 1993. v + 59 pp., figures, tables, bibliography. $10.00 (paper).

Reviewed by Anthony E. Marks, Southern Methodist University.

In spite of the title, this slim volume presents data on only two middle Paleolithic sites from the Wadi Hasa in southern Jordan. These data are mainly limited to various metric and nonmetric attributes of the two lithic assemblages. One, WHS 621, belongs to the Late Levantine Mousterian (Tabun B type), and the other, WHS 634 (Ain Difla), falls within the Early Levantine Mousterian (Tabun D type). They are presented within a framework of testing hypotheses of degrees of mobility, as seen through "curation" vs. "expedience," raw material availability, and site function. Each factor is discussed very briefly, relative to recent Middle Paleolithic