Binford, however, argues that because skeletal-part frequencies do not seem to vary significantly across levels and because he is interested in "macroscale" patterning, a fauna spanning nearly 100,000 years can be treated as if it represented a single depositional event.

The absence of stratigraphic detail harms Binford's analysis. As he notes in a critique of G. Isaac's home-base model of early hominid organization, "The positioning of the system in space ensures that the content of sites, *particularly stratified sites*, will appear variable . . . between occupational episodes" (p. 199; emphasis mine). Yet Binford attempts to show that scavenging was characteristic of the entire Klasies Middle Stone Age by showing that there is evidence for such behavior in the fauna as a whole. Without his evidence arrayed stratigraphically, his argument is not convincing. There are, after all, only some 60 hack marks involved, and without stratigraphic data we do not know if these were confined to a single level or if they occur throughout the Middle Stone Age strata. Although gnawing is far more common, lack of stratigraphic information prevents us from judging whether or not hack marks and gnawing covary through time. That the Middle Stone Age was characterized by scavenging is simply not established.

That Binford ignores stratigraphy in his basic analysis is the more remarkable in that he also examines shifts in the relative frequency of hunting and scavenging through time. Lacking a stratigraphic analysis of his own data, Binford turns to a very different measure of changing subsistence tactics: the animals themselves. Large mammals become evidence of scavenging, small ones of hunting, and changing frequencies of the two, drawn from Klein's work, are held to reflect changing frequencies of hunting and scavenging by the Middle Stone Age occupants of Cave 1. From this, Binford concludes not only that Klein's attempts to reconstruct past environments using the Cave 1 mammals are misdirected but also that hunting becomes more, and scavenging less, important through the sequence. This is a long leap from the argument that some of the Cave 1 mammals were scavenged, the large mammals themselves now becoming the evidence for scavenging. The circularity could have been avoided had the Klasies fauna not been treated quite so much as if it were a fully modern one.

Though it is difficult to agree with Binford that he has placed our view of the Middle Stone Age in a "dynamic mode," it is certainly true that *Faunal Remains from Klasies River Mouth* is an important book. The flawed analysis aside, Binford makes an intriguing case for scavenging and presents a method for detecting scavenging that is superior to anything that has come before. More important, he provides a way of thinking about the Middle Stone Age, and the Middle Paleolithic, whose value far eclipses problems in the analysis. This is a provocative book (and a number of my colleagues have been provoked) and must be carefully read by archeologists regardless of their chronological interest. It is certain to affect the way we think about the past, and certain to generate much new knowledge not only about the past but also about how the archeological record is to be approached.

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Pleistocene Extinction Reexamined

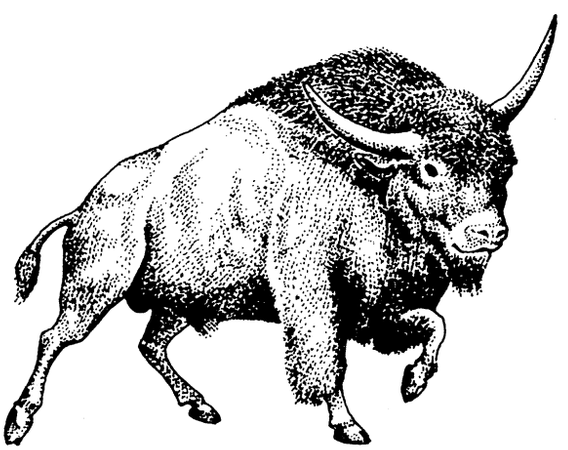
Quaternary Extinctions. A Prehistoric Revolution. PAUL S. MARTIN and RICHARD G. KLEIN, Eds. University of Arizona Press, Tucson, 1984. x, 892 pp., illus. $65.

Why did so many animals become extinct near the end of the last glacial period? Were they killed off by human hunters, or did they die out because of climatic change? Scientists in many disciplines have grappled with these questions for more than a century, and the problem has taken on a sense of urgency as the rate of extinction has accelerated in modern times.

Unlike the acknowledged parent book, *Pleistocene Extinctions: The Search for a Cause* (P. S. Martin and H. E. Wright, Jr., Eds., Yale University Press, 1967), which was an International Union for Quaternary Research proceedings volume, *Quaternary Extinctions* was born of informal manuscript requests "without the benefit of any preceding symposium or conference" (p. x). Forty-seven contributors have provided 38 chapters grouped into seven sections that are introduced by brief editorial comments: Historical Background and the Beasts Themselves (three chapters); A Close Look at Significant Sites (five chapters); The Theoretical Marketplace: Geologic-Climatic Models (seven chapters); The Theoretical Marketplace: Cultural Models (six chapters); Asia and Africa: Modest Losses (five chapters); Australia, New Zealand, and the Island Pacific: Severe Losses (nine chapters); and An Overview (three chapters).

Many of the observations that might be made in a review of the book are already provided in the three overview chapters, each of which is an excellent synthesis. For example, L. G. Marshall (chapter 36) tabulates the viewpoints of the contributors, finding that 11 of the papers attribute extinction directly or indirectly to human hunting or other activities, 10 attribute extinctions directly to climatic change, and the others that seek to explain extinction describe some combination of factors. The contributors might seem rather evenly divided between artificial and natural causes of extinction, but if the problem could be solved by a show of hands we would not need a book of 900 pages. Indeed, Marshall concludes that extinction is a "miasmatic problem" (p. 803) and that it is unlikely that a global explanation can be valid for all landmasses.

D. K. Grayson makes two of the finest contributions to this volume. The first is an outstanding historical review (chapter



"*Bison latifrons*, the extinct long-horned bison of the North American late Pleistocene." [From E. Anderson, "Who's who in the Pleistocene: a mammalian bestiary," in *Quaternary Extinctions*]

1) in which we learn the long histories of many currently proposed explanations for extinction, including the concept of changing climatic equability (seasonal contrasts). Edouard Lartet first described variations in seasonality in 1867 (*Ann. Sci. Nat. Zool. Paléontol.* ser 5, **8**, 157–194), and the idea was never again discussed at length until D. I. Axelrod published a seminal paper a century later (*Univ. Calif. Publ. Geol. Sci.* **74**, 1–42 [1967]) and brought the concept to its current prominence (see chapters in part 3). Grayson also contributes to the overview section (chapter 37) with an insightful analysis of the structure of the debate between champions of human overkill and supporters of climatic models. He finds that current climatic hypotheses "stand on firm ground as regards their ability to be profitably tested" (p. 820) whereas the overkill hypothesis is "burdened by auxiliary hypotheses that protect it from falsification" (p. 821).

The last paper (chapter 38), by J. M. Diamond, is a fascinating study of historic extinctions and provides generalizations concerning proneness to extinction that could have been helpful to the reader if placed in the opening section of the book. Diamond shows that even some recent extinctions are not understood and that it may never be possible to pin down the causes of prehistoric extinctions. However, he makes some important observations concerning the structure of the fossil record, the need for species-level (rather than genus-level) analysis, and the lessons to be learned from the fossil records of islands. For example, if there were no waves of extinction in the late Pleistocene of New Zealand and Madagascar, it would be hard to attribute continental extinctions to climatic change alone. Here is fertile ground for further research.

There are several respects in which the book might have been improved by discussion in a prior symposium. For example, the interpretation of radiocarbon dates and their meaning for the larger problem receive varied treatment in different papers. Chapters 8 and 19 focus specifically on dates that are taken to indicate the last appearances of species or genera, whereas N. K. Vereshchagin and G. F. Baryshnikov (chapter 22) state that "the latest known record of an extinct species does not record its final extinction, but rather the continued presence of a relatively large population" (p. 483). In the chapters on New Zealand, M. M. Trotter and B. McCulloch (chapter 32) prefer dates on collagen and marine shell over those on charcoal (p. 718), but A. Anderson (chapter 33) prefers the charcoal dates (p. 733). Since chronology is crucial to the demonstration that two events, say extinction and the arrival of humans, are coincident, I would have preferred to see the dates used as a basis for inferring the age of each event with a discussion focused on the reliability of the inference rather than on technical uncertainties of the radiocarbon method.

Another worthwhile symposium theme would have been a comparison of the Sangamon (or any other) interglaciation and the Holocene. R. D. Guthrie (chapter 13; see also chapters 6 and 11) argues strongly that the Holocene is unlike any previous interglacial, whereas P. S. Martin declares that "the only significant difference between the transition to the Sangamon and the transition to the Holocene is the presence of early hunters in North America" (p. 367; see also chapter 16).

The issue of visibility in the fossil record could have been on the symposium agenda, because opinions vary widely in the book. Lack of visibility is one of the auxiliary hypotheses that buttresses the overkill hypothesis, but J. E. Guilday (p. 256) notes that it is "unanswerable and untestable," R. G. Klein (chapter 25) seems to assume that human-linked extinction will be visible in the fossil record, and one of the clearest cases of overkill (the New Zealand moas) is highly visible (chapters 32 through 34).

These matters show merely that more work remains to be done on this fascinating and important subject, and Martin and Klein are to be congratulated on the publication of an attractive, readable compendium. *Quaternary Extinctions* is markedly improved over its parent and reflects the benefit of nearly two decades of vigorous work by numerous researchers. Many of the authors draw heavily upon B. Kurtén and E. Anderson's recent synthesis *Pleistocene Mammals of North America* (Columbia University Press, 1980), a work that clearly aided Anderson's compilation of the bestiary included in this volume (chapter 2, from which hominids are paradoxically omitted). Better geographic coverage is seen in the papers on Africa, China, Australia, New Zealand, and the island Pacific, and better topical coverage is shown by chapters on birds, plants, and longer time spans. Improved theoretical background is reflected in many of the papers. This book should be read by paleobiologists, biologists, wildlife managers, ecologists, archeologists, and anyone concerned about the ongoing extinction of plants and animals.

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The State of Ecology

Ecological Communities. Conceptual Issues and the Evidence. DONALD R. STRONG, JR., DANIEL SIMBERLOFF, LAWRENCE G. ABELE, and ANNE B. THISTLE, Eds. Princeton University Press, Princeton, N.J., 1984. xiv, 614 pp., illus. $60; paper, $22.50. From a symposium, Wakulla Springs, Fla., March 1981.

A New Ecology. Novel Approaches to Interactive Systems. PETER W. PRICE, C. N. SLOBODCHIKOFF, and WILLIAM S. GAUD, Eds. Wiley-Interscience, New York, 1984. xii, 515 pp., illus. $59.95. From a conference, Flagstaff, Ariz., Aug. 1982.

Whatever the fate of the theories and hypotheses expanded, exposed, or exorcised in these two volumes, it is likely—as May suggests in his overview chapter in *Ecological Communities*—that the publication of the books will come to be recognized as marking an important stage in the development of ecological theory. That the theoretical core of the subject is still young was brought home to me a few months ago when I saw Charles Elton and G. Evelyn Hutchinson talking together in an Oxford street: virtually all the concepts currently under debate have emerged in their professional lifetimes, often originating from one or the other of them. But, as May also points out, controversy about the related theories of the role of competition in communities and the role of density-dependence in natural populations has been a recurring feature. Does this mean that, in contrast to, say, nuclear physicists, ecologists have totally failed to develop an agreed-upon core of theory? Or if, as I believe to be the case, the conceptual core is merely smaller and relatively less well tested, is this due to some characteristic of the subject and its students, rather than to its youth and comparative underprovision with research funds? Some chapters in both