Scientific Archeology

Models in Archaeology. DAVID L. CLARKE, Ed. Methuen, London, 1973 (U.S. distributor, Barnes and Noble [Harper and Row], New York). xxiv, 1056 pp., illus. $62.50.

The 26 essays in this volume are examples of the recent trend in archeology frequently referred to as the new archeology. The principal characteristic of the new archeology is a full commitment to the methods of science. In broad terms, this commitment involves active theorizing, careful formulation and empirical testing of hypotheses, and explanation of archeological data by means of explicit covering generalizations; it is to be understood as a response to an earlier state of affairs in which the prevailing temper was expressed by remarks on the inherent deficiencies of the archeological record, the need for more evidence and less theorizing, the basically unpredictable nature of human behavior, and the essentially humanistic and historical character of the archeological discipline. The jacket blurb describes the transformation as "struggling from intuitive and aesthetic obscurantism towards a more disciplined and integrated approach." Whether or not these are completely accurate characterizations of the earlier state of archeology, especially prehistoric archeology, is not critical here. The point is rather to sketch the background for the fiercely scientific (as opposed to amiably historical) tone of the essays.

As the title of the book indicates, the concept of model and the activity referred to by some of the authors as modeling are thought to be the central features of the new kind of archeology exhibited by the essays. Several of the authors discuss the concept of a model; in particular, the opening essay by the editor contains an extensive discussion with the conclusion that models are "pieces of machinery that relate observations to theoretical ideas" but that there is so much variation in form and application that it is unprofitable to attempt a precise definition. None of the authors refer to R. B. Braithwaite's spirited essay in *Logic, Methodology and Philosophy of Science* (Stanford University Press, 1960) on models in the empirical sciences, in which, among other observations, he points out that in psychology and the social sciences "model" is often used merely as a synonym for a formalized or semiformalized theory. I take it that the authors do not in general have a deep philosophical interest in the concept and that it is probably true that in most of the situations in which the word is used it could be replaced with greater accuracy by other terms—"theory," "hypothesis," "analogue," "interpretation," "example," and "representation" come to mind. This vague usage is common in the social sciences. The archeologists seem to have been influenced strongly by geographers, from whom they have also borrowed a number of other techniques and concepts, particularly those associated with locational theory. I do not believe, however, that the situation calls for anything more than very mild deriding of overenthusiastic jargonizing; the jargon does not prevent me from admiring the imaginative and technically impressive accomplishments of the authors.

Nine of the essays are devoted principally to general treatments of topics. D. L. Clarke's introductory essay provides a broad discussion of the archeological enterprise in terms of models and paradigms. "Paradigm" in the sense of a broad and pervasive concept governing perception of problems and research objectives is taken from Thomas Kuhn, whose views on the nature of scientific change have been a fundamental stimulus (paradigmatic, one might say) for the new archeologists. Clarke's list of basic paradigms in the new archeology includes the morphological paradigm, the anthropological paradigm, the ecological paradigm, and the geographical paradigm. In my summary terms, these paradigms refer to detailed study of the nature, associations, and correlations of the attributes exhibited by the artifacts and features of archeological assemblages; ethnologically guided inferences on the pattern of human behavior responsible for the form-locus relationships of artifacts; interpretation of past communities as elements in ecological systems; and study of the spatial distributions of communities (and also of elements within communities) to detect patterns of areal organization of prehistoric activities. This list is not an exhaustive description of the activities of the new archeologists. Chronology and cultural evolution are still among the basic organizing concepts, and failure to make this point clear is an irritating feature of some recent writing. The creative novelty lies, it seems to me, primarily in willingness to face up to the mind-stretching terrors of advanced quantitative methods, to the serious study of ecology, and to the use of any other techniques that promise to improve our understanding of past human behavior. The result is a noticeable improvement in our ability to define that which is dated and that which evolves. The new archeologists are attempting to increase our powers of generalization on and explanation of past human behavior by any means that come to hand, and they do not accept any self-imposed limits on the increase.

The general treatments include essays by J. N. Hill and by Hill and R. K. Evans on, respectively, the character of the current "methodological debate" in archeology and an important aspect of the debate, the problem of classification and typology. In both essays, the essential role of active theorizing in scientific progress is heavily emphasized. This emphasis leads to preoccupation with the activities of the theorist at the expense of the empirical aspect of research, sometimes causing one to wonder whatever became of the artifacts. But the major point is a good one—the principal enemy is passive inductivism. S. G. H. Daniels provides an informative discussion of research design with particular stress on techniques for controlling observer bias in both field and laboratory procedures. The essay of J. E. Doran on computer models is a timely contribution in view of the perhaps immoderately passionate involvement of the new archeologists with the computer. Doran is a computer specialist with a good knowledge of archeological problems and methods. His conclusion is surprising: archeology is essentially a nonnumerical subject and the digital computer is also essentially nonnumerical, a situation offering a promising field of cooperation between archeologist and computer scientist. Other discussions in my class of general treatments are somewhat more narrowly focused: G. L. Cowgill and Leroy Johnson deal with seriation techniques, J. Litvak King and R. Garcia Moll with set theory models (in archeologically more familiar terms, with comparison of entities in terms of attribute presence or absence). In the final essay, R. P. Chaney discusses theoretical statements in archeology and anthropology, beginning with the gloomy assertion that a condition of "extreme theoretical myopia" prevails; his general statements preface a discussion of cross-cultural survey methods and problems. Chaney's paper does not seem to articulate well with the rest of the volume.

The remaining essays are by no means devoid of general interest, but they are also examples of application of explicit theory to archeological problems. I can mention only a few highlights to illustrate something of the range of problems and results. Two papers, those of L. R. Binford and G. Ll. Isaac, are concerned with the 98 percent of culture history embraced in the Lower and Middle Paleolithic periods. Both authors emphasize the intractable character of the assemblages from these periods when explanatory concepts drawn from more recent prehistory are applied. Briefly, archeologists usually explain differences between assemblages (if environmental circumstances are a constant) as products of separation in time or space or both—as inverse functions of the degree of communication between the people who produced the assemblages. Close similarities are correspondingly taken to mean a high degree of communication. But the Lower and Middle Paleolithic periods exhibit substantial and consistent differences between assemblages that are not explicable by time-space separation. Binford argues that these differing assemblages are to be explained as activity-specific loci; the various types of assemblages represent the appropriate combinations of flint tools for particular kinds of activities such as preparation of vegetable foods or butchering of animals. In effect, there was essentially one broad and very slowly evolving cultural tradition manifested only in specialized facies, in ad hoc tool kits reflecting spatially and seasonally distinctive subsistence activities. Isaac is more cautious, pointing to more or less random "cultural drift" and other factors as possible explanation. He also emphasizes difficulty in interpretation because of the wide scattering in space and time of the Lower Paleolithic sites. The two authors agree on the inadequacy of the culture tradition concept customarily applied to Upper Paleolithic and more recent data; with the advent of the Upper Paleolithic (and biologically modern man) some 40,000 years ago, a new and familiar order of cultural behavior appears. The other papers are less sweeping in subject, but they illustrate the methodological versatility and theoretical liveliness of current archeology. Pervading concepts are "action archeology" (detailed ethnographic studies designed to illuminate archeological data), assemblages as manifestations of spatially organized cultural systems (in medieval England as well as in the prehistoric Great Basin of the United States), and assemblages as elements in a thermodynamic system (ecology in the carefully used sense of the term).

I conclude that the basic claims of the new archeology are justified by the quality of the work exhibited here. There is indeed notable progress in our ability to explain past human behavior, and the progress is the result of theoretical originality, careful formulation of hypotheses, and the incomparably better field and laboratory research resulting from explicit hypotheses. There are some undesirable side effects, of course: one can with some justification poke fun at jargon, pretentious and inappropriate appeals to philosophy and logic complete with misspelled technical terms ("ideographic" for "idiographic"), and excessive enthusiasm for currently fashionable ideas (surely there is less to locational analysis than meets the eye). But there will be a sobering-up process. There is plenty of solid matter underneath the froth.

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Transition Zone

Bridge and Barrier. The Natural and Cultural History of Torres Strait. Proceedings of a symposium, Canberra, Australia, Dec. 1971. D. WALKER, Ed. Australian National University Press, Canberra, 1973. xviii, 438 pp., illus. Paper, U.S.$7.50. Research School of Pacific Studies Publication BG/3.

Because of its unique biota, stemming from its protracted period of isolation, the Australian biogeographic region has long fascinated biologists. Of only slightly less interest are the rather marked biological differences between its two major subregions, continental Australia and insular New Guinea. These differences are great in the case of plants and insects, those of New Guinea being strongly Oriental in character, whereas in mammals and birds an endemic Australo-Papuan fauna of monotremes, marsupials, birds of paradise, honey eaters, mound-building birds, and parrots is shared by the two regions.

In 1971 a symposium was convened at the Australian National University to investigate the significance of Torres Strait, the 150- to 500-kilometer-wide, 13-meter-deep water barrier that separates Australia and New Guinea, relative to the biotic differences, and similarities, between mainland Australia and insular New Guinea. The resultant volume consists of 21 chapters devoted to the paleogeography, geomorphology, late Quaternary and contemporary climates, vegetation communities, plant distribution patterns, insects, lower vertebrates, mammals, birds, and human linguistic and culture patterns of the Torres Strait area. The emphasis is specifically on the Strait rather than on the Australian and New Guinea biotas as a whole. Several authors, however, provide data of the wider type. The book merits full praise as an in-depth appraisal of a fascinating biogeographic and cultural transition zone.

The Torres Strait water barrier apparently came into existence only in late geological times, possibly middle or late Pleistocene, and probably in association with downfaulting that also produced the Gulf of Carpentaria. Prior to this a much smaller New Guinea (roughly equivalent to the southern half) was broadly joined to Australia and formed the leading edge of the Australian continental plate. This plate, sea-floor-spreading data suggest, only reached its present proximity to Asia in the Miocene. Subsequent to its original formation the Torres Strait seaway was interrupted with each of the major drops in sea level. These drops, which some authorities are now suggesting amounted to as much as 180 meters, were more than sufficient to bring Australia and New Guinea again into extensive contact, and to extend Australia far to the northwest. What was the climate like during these times of contact? The evidence is that cooler sea surface temperatures, cooler trade winds, and the greatly extended land surface made northern Australia significantly drier than at present; thus it would have supported tropical savanna such as occurs in the interior of northern Australia today (P. J. Webster and N. A. Streten). If these deductions are correct, then, there would not have been any accelerated southward movement of the New Guinea lowland rain forests at such times.

New Guinea and Australia have long been independent, major centers of radiation. Author after author, however, brings out how very differently adapted the two biotas are. The former,