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Rosalind L. Hunter-Anderson
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NEW STUDIES IN ARCHAEOLOGY

Series editors
Colin Renfrew
Jeremy Sabloff

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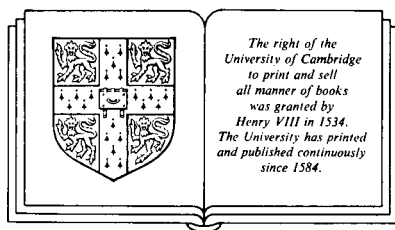
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ROSALIND L. HUNTER-ANDERSON *University of Guam*

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FOREWORD

‘We will contribute to the development of general anthropological theory’ was a polemical rallying cry of the ‘new archaeology’ at the time of its inception in the early 1960s (see Binford and Binford 1968; Binford 1972). Although great lip-service was paid to this goal and some notable attempts were made to fulfill it (see, for example, Renfrew 1972; Plog 1974), much of the initial optimism about theory building was soon lost. As Rosalind Hunter-Anderson argues in the pages that follow, most of the original enthusiasm for theory building has been channelled into so-called ‘hypothesis testing’ in which attempts to explain the archaeological record use, in a circular fashion, observations of the record as part of the ‘tests.’ This pattern of ‘fitting’ models to archaeological data has impeded the development of legitimate hypothesis testing.

Recently, there have been new calls for a return to the concerns of the heady days of the 1960s when theory building was considered paramount (see Moore and Keene 1983). Yet these calls see the ‘tyranny of methodology’ as the principal stumbling-block preventing the growth of theory, although it would actually appear that a lack of productive methodologies, as well as no clear understanding of their roles, has impeded past attempts at theory building. Nevertheless, whatever the causes of archaeologists going astray, the field must once again be set on a positive path or paths. Since archaeologists seem most responsive to arguments by example, what are obviously needed are good examples. We believe that this book fits the bill.

While Dr Hunter-Anderson’s arguments and polemic are certain to be controversial, since her critical eye is an unsparing one, she has the courage to ‘put her money where her mouth is’ by demonstrating what she believes to be a practical path to theory building. Potential critics will not find the author hiding behind curtains of verbiage! In addition to her clear, straightforward presentation, Dr Hunter-Anderson’s study has the merit of showing how pre-1960 archaeological reports can be put to good use in modern analyses. Whether or not Dr Hunter-Anderson’s attempt at theory building is successful, her book should serve as a productive example of *how* archaeologists might profitably proceed. At the least, it should stimulate further attempts of like kind. If it does, then one of our own goals for the New Studies in Archaeology series will have been reached.

JEREMY SABLOFF
COLIN RENFREW

PREFACE

On the geologically ancient Southwestern landscape of muted browns and greens, the torrential rains of July and August always produce a short-lived mosaic of bright flowers – red, orange, yellow, purple. Seeing for the first time this stunning array of annuals along the roadsides of central Arizona, I felt somehow akin to those opportunists taking advantage of the rare moisture and sustained warmth. Summer is also the time when archaeologists can be found in various parts of the Southwest, intent on their specialized purposes while the weather and other schedules permit.

It was the summer of 1968. We were a small group of undergraduates from various universities, along with members of the staff of the Chicago Field Museum's Archaeological Expedition, encamped for the summer in Vernon. It was one of Director Paul Martin's last field trips to the Southwest, and his young graduate assistants (Mark Leone, Craig Morris, Fred Plog, Ezra Zubrow) had planned much of the research. The New Archaeology was fast becoming a reality; as guest speakers we had Leslie White, Thomas Kuhn, James Hill. For contrast we had Emil Haury and Watson Smith. In the background loomed the large frame of Martin himself, who had taken an abrupt turn toward the future in sponsoring such a grouping after a solid and productive career as a culture historian. For the three U.C.L.A. students (myself, Michael Schiffer and Charles Vanasse) this expedition offered especially exciting prospects. After two years of classroom exposure to the new ideas we were going to act on them in the field and produce Great Works!

The student research papers we wrote that summer are rightfully moldering in boxes, where such tentative efforts belong. Nonetheless since then several of the group have continued to experiment with ideas and techniques in the Southwestern region and elsewhere. Innovative analytical approaches distinguish the research begun by graduate students at the University of Chicago who had worked with Martin at Vernon (Longacre, Hill, Leone, Zubrow, Plog) as well as that of their students who are now teaching and carrying out research programs of their own. I continued at U.C.L.A. for graduate studies, later transferring to the University of New Mexico.

The divergence in academic pathways between myself and the others who spent an archaeological summer in Vernon led to differences in field experiences and in research focus as well. In spite of living in the heart of 'Indian country' in New Mexico while attending the University at Albuquerque, I became a generalist rather than a Southwestern specialist. Others became experts in pottery design analysis or Pueblo architecture; I worked on human space use and hunter-gatherers. Like many

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archaeology students in the 1970s, I worked on contract projects, for example at Zuni and in the Rio Puerco and Rio Grande drainages, finding 'cultural resource management' money more plentiful than research grants. These projects were not ideal archaeological training but they were valuable in other ways. As a principal investigator I often had to determine whether what we had found was 'significant.' Although this term can be a conceptual catch-all reflecting many different priorities in historic preservation, I took it to mean anthropological significance. Each final report was an exercise in which comprehensive statements of this sort were made, in respect to various sets of data.

When it came time for a doctoral thesis, all my best efforts to do an ethno-archaeological study were thwarted by circumstances seemingly beyond anyone's control. I fell back on Paul Martin's published work. The site reports contained systematic descriptions of artifactual assemblages and architectural features which could be re-analyzed and puzzled over. Here again was the challenge to make anthropological sense out of a set of data, this time a large one spanning several hundred years of prehistory in a large and contrastive geographic region. This book is based on the dissertation that resulted, revised after over two years of ethno-archaeological fieldwork among the Yapese in the western Pacific.

Like other archaeologists who have been privileged to live among a non-industrialized and non-commercialized people, I too noted the factors that contribute to patterns of artifactual deposition at a site. But far more interesting to me was how people cope with the demands made upon them by the culture into which they have been born. Direct ethnographic observation was a powerful antidote to the dull poison of social scientific abstraction of the ethnological literature. I learned, for instance, that the Yapese chiefs participate in competitive exchanges of food and valuables because they must. It is a burden of office, so to speak, and has nothing to do with personal ambition or an insistent desire for prestige. Personal ambition on Yap, at least in the past, could get one assassinated.

I also learned that exotic goods (large disks of aragonite and a rare type of pearl shell) were brought over the open ocean into Yap from Palau, hundreds of miles to the Southwest. These non-local items had not arrived by 'trade' but through the mechanism of formal permission. Yapese themselves quarried the aragonite and dived for the pearl according to agreements between chiefs in the two islands. Other exotic goods arrived in Yap yearly as part of a tribute system marking sociopolitical ties between one particular district on Yap and its atoll dependencies. The tribute items (mats, woven cloth, coconut oil and rope, various shells) were redistributed in Yap along strictly predetermined lines of sociopolitical association. Return gifts marking the superior/inferior relationship between the Yapese chiefs and their outer island dependents were duly made. Later they were redistributed along predetermined sociopolitical lines in the atolls. No 'trade' was involved, only obligations fulfilled. So when I returned to the States and found that Southwestern archaeologists were imagining a prehistoric past full of ambitious individuals propelling their aboriginal culture into a hierarchical chiefdom, and of later vast trade networks of local and exotic goods managed by a jealous and competitive elite whose excessive

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greed caused a terrible environmental degradation – ‘supported by the archaeological record’ consisting of some non-local objects, different sizes of pitstructures within villages, and evenly spaced settlements – I knew I was back in the non-real world. My first response was amusement; a more considered one has been incorporated into the Introduction.

But in the heat of debate over these points one can lose sight of the basic agreement within the New Archaeological camp: there is much more to be learned from studying prehistoric cultural remains than had been thought by the culture historians. We are not limited by the ‘incompleteness’ of the archaeological record; it is just another empirical domain with its own properties and regularities, very different but no less full than the ethnographic record. We are not data-poor ethnographers of the past; we are anthropologists who study adaptive processes over long time ranges. And if we are lucky we will have the opportunity to make some ethnographic observations from an archaeological perspective. My own experience in this area convinced me that archaeologists need to be extremely selective when borrowing from other disciplines either analytical techniques or theoretical concepts. For the most part we will have to invent our own, and this will be our contribution to general anthropological theory.

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To my husband, Yigal Zan, go my unreserved thanks for years of intellectual and emotional support, culminating in this work. Several of the points made in the Introduction are a product of his insight as someone from another discipline in need of a paradigmatic revolution like that begun in archaeology some two decades ago.