THE ARCHAEOLOGY OF HISTORY
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ABSTRACT

The definition of Historical Archaeology continues to vex practitioners and to arouse passionate, not to say dogmatic, contention. I suggest that Historical Archaeology should be defined broadly; that it can make contributions to general issues relevant to the study of complex societies; and that historical archaeologists must think comparatively in order to make the most of our discipline. In response to Mark Leone’s (1979) challenge that Historical Archaeology does not have a question to ask, I propose two related questions, one methodological and the other theoretical, that reflect my fundamental definition of Historical Archaeology as the Archaeology of History.

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This essay is a preliminary formulation of several lines of thought about the nature of historical archaeology, in theory and in its current state. I have, in previous papers (Wesler 1985, 1991), tried to challenge our collective concept of what historical archaeology is. I continue to challenge it because I think we tend to define the field too narrowly, and that we all specialize too much.

In one paper (Wesler 1991; cf Wesler 1994), I tried to apply several methods developed in historical archaeology to a prehistoric data set, my Mississippian assemblages from the Wickliffe Mounds. My reasoning was that Mississippian society is the most complex society in prehistoric Eastern North America, and that some of the techniques and methods from historical archaeology ought to be useful, in general, on cases other than Euroamerican sites. I adapted methods of pattern recognition, formula dating, mean ceramic dating, and socioeconomic indexing to the Mississippian data. Success was mixed at best, but I thought the attempt, and some thoughts on why the failures occurred, were interesting enough to present for discussion.

I sent a version of the paper to a major journal, which rejected it, partly because of real problems with the paper. But partly, it was rejected because of the reaction of a historical archaeologist. The reviewer objected strongly to my statement that “historical archaeology will contribute most lastingly if it is seen as a subset of the archaeology of complex societies, and not as a discipline distinct from prehistory.” Elsewhere in the paper, I wrote: "Historical archaeology is a particularly rich subset of the archaeology of complex societies. It is not a separate discipline... to create a unified archaeology of complex societies, and a set of methods and theories that provide comparability across wide ranges of pre-, proto-, and historic sites, the methods of prehistory and historical archaeology must be integrated” (same conclusion as Wesler 1991).

I saw these statements as a plea for a holistic archaeology, for a broad vision of what we do, and for a recognition that historical archaeology has something to contribute to a wider set of questions and issues. The historical archaeologist reviewer reacted with umbrage.

“I do not think that Mr. Wesler has demonstrated this point in any way that would begin to make me redefine what I do as a subset of prehistory.” That is not what I wrote.
“When I see someone attempt to redefine historical archaeology or confine it in any way, it makes me nervous.” How does calling for a broader perspective “confine” historical archaeology? The reviewer cites Cary Carson and Garry Wheeler Stone as historical archaeologists who did not come from anthropology, and complains that I am trying to deny them access to the field and restrict it to anthropologists. How does my suggestion that historical archaeology can contribute to broader issues than American history, devalue their contributions?

The reviewer's position seems to be that historical archaeology is what familiar and congenial practitioners do, and no other definition can be allowed. I see a frightening narrowness of vision here. It is an unusually defensive version, but it reflects a number of definitions that boil down to this: historical archaeology is about European history in the last five centuries. Period. Non-Europeans who are assimilated or impacted by European culture can be slipped in the back door, but only if they have the right kind of artifacts in their sites.

There are various forms of this definition. Classic statements include Noel Hume's (1964) and Harrington's (1955) identification of the field as handmaiden to history, to Dollar's (1968) definition of historical archaeology as architectural in focus and reconstructive in scope, to Deetz's (1977) archaeology of the European expansion. I had always thought the most restrictive positions were answered by Cleland and Fitting's (1968) paper placing historical archaeology in the realm of anthropological archaeology, designed to ask questions beyond the details of historical places, and that there was room for everybody's perspectives. My reviewer suggests otherwise.

A more recent anonymous reviewer of another version of the paper suggested that I was out of touch, and that the best recent writing on historical archaeology had established that we are doing the archaeology of capitalism. Mark Leone's work has led the way into this version of the definition: “Historical archaeology is the archaeology of the modern period, of the expansion of Europe. And the engine that drove that expansion was capitalism.” He did specify that ‘the study of capitalism is a central focus of historical archaeology” (School of American Research 1994:20), but my second reviewer seemed to think it was now the correct focus.

I am not satisfied with these definitions. I think that historical archaeology ought to be broadly defined as the archaeology of people who can also be studied through documents. I see historical archaeology as an opportunity to compare two mostly independent data sets, archaeological and documentary (including oral), to test hypotheses and to complement each other for a fuller picture of the past. In effect, I see historical archaeology at its broadest as the archaeology of history.

There are, of course, others who see this point. Schuyler (1978) defined historical archaeology this way, including Islamic, Classical, Biblical, Chinese, and others. Kardulias (1994) recently published a paper in which he compared Classical archaeology to historical archaeology, and Houston (1989) recently suggested that Mayan archaeology is coming close to being a historical archaeology.

It seems to me that the methodology of comparing documentary to archaeological data has common threads, whether the documents are cuneiform, hieroglyphic, or alphabetic, whether Sumerian, Roman, medieval Celtic, or colonial American. There are similar conceptual problems, and on the broadest level of anthropological and historical inquiry, comparable questions to be asked.

I can cite an example that I like to use in class. It is a case study in battlefield archaeology. It involves an army of a colonial power, a unit of the most powerful and best equipped army in the world at that time, on an expedition beyond its colonial frontiers. Its mission was to round up and
punish enemies that were considered ill-equipped, racially and intellectually inferior savages. It met the forces of the “savages” and was wiped out. Historical sources left a great deal of confusion about what happened. Archaeology on the battlefield clarified a number of points. The so-called savages were as well armed as the army, and better organized than the army—and its historians—were prepared to allow. Archaeology identified previously unsuspected tactical positions, clarified the course of the battle, and went a long way towards explaining the army's disaster. Archaeology offered an entirely new, and unprejudiced, view of that battle.

Historical archaeologists should recognize this scenario. It is the battle of the Teutoberg Forest in A.D. 9, when Quintillius Varus led three Roman legions to disaster at the hands of the Germans (Dornberg 1992). You were thinking of some guy named Custer, maybe?

I think that the parallels are striking, especially as case studies in the way archaeology can illuminate historical documentation. Why do we not think comparatively often enough?

Mark Leone has commented on historical archaeology many times. In the 1970s, he was critical, looking for the direction that he has apparently found in the study of capitalism. Two of his comments have stayed with me (Leone 1979). He spoke of a sense of incompleteness in archaeology—in my interpretation, a sense that we are not doing enough, not tackling hard enough questions. He also suggested that historical archaeology does not really have a question to answer. What do we want to know that cannot be answered by historians?

I would like to propose a question, one that reflects my definition of historical archaeology in its broadest sense as the archaeology of history, of historical peoples. Actually, it breaks down into two questions, one theoretical, the other methodological.

My theoretical question is this: what is the effect of literacy on human society?

My methodological question is: what is the effect of a literary record on archaeology?

I think the answer to the second question is obvious, and that we ought to be dissatisfied with it. Historical records have overshadowed archaeological data. Too many archaeologists have been content to see archaeology as the adjunct to history, as the handmaiden to history. At least in theory, this position has been roundly attacked in American historical archaeology, and new techniques and methods have been developed. But we have all seen too many projects that are site specific, descriptive exercises; too many projects that end up as illustrations of history, despite our best intentions. Even many of our innovations, like South’s (1977) pattern recognition, are applied in rote and uncreative ways, in what Leone (1979) called “sterile science” (cf. South 1988). We still see far too little synthesis that adds substantially to what the historians have told us.

It is even worse in other historical archaeologies. Look at Mesopotamian archaeology. How much theory comes out of that field? According to one synthesist, “The archaeologist's contribution toward the elucidation of the Mesopotamian past bears primarily on that crucial millennium or more which preceded the earliest written documentation...” “...archaeologists prefer digging for monuments to spending time in following the endless walls of cities... or disentangling the network of crooked streets in a residential section...” (Oppenheim 1977: 10, 126). The clearly expressed view of Mesopotamian archaeology is that it serves to recover texts, which is where the real information comes from. There are counter examples, but they are not the rule, and the rule is an indictment of archaeology. In Egyptology, Classical and Biblical archaeology, archaeology illustrates or goes its separate way; it rarely serves as a critical historical archaeology, despite a potential I see as crying for attention.

As to the theoretical question, what is the effect of literacy on human society, there is a small but interesting literature on literacy (e.g. Goody 1986). It ranges across disciplinary
boundaries. For instance, in psychology, “the invention of literacy has been suggested to have had profound historical cognitive effects... human intellect cannot be separated from the technologies (e.g. writing, speech, numerical systems) invented to extend cognitive processes.” There is even some evidence suggestive that there are differences in hemispheric lateralization and neuronal connections in the brains of literate versus non-literate people (Rogoff 1981:280-282). This is not a matter of intelligence or ability, but a difference in how we conceptualize the world, organize it, and think about it. If literacy makes a difference in individuals, how does it affect whole societies?

We have properly abandoned a simplistic equation of literacy with “civilization” or state societies. As Goody (1986:91) notes, “Historically the association may be accidental, since, in effect, the first we know of states comes from the written record.” Nonetheless, the adoption of literacy affected individuals psychologically, and societies structurally; and we have only the vaguest notions of how. This is, in my view, a subtle and fundamental question. Can we create an archaeology of history that grapples with subtle and fundamental questions?

Why must archaeologists grapple with these questions, and not historians? Because the contrastive case is nonliterate societies--prehistory--and archaeologists can compare the two. Historians cannot.

I think that a study of European Medieval Archaeology can be used as an extended example and a test case for widening my perspectives. I have recently read through the whole series of the journal Medieval Archaeology. I am interested in European Medieval archaeology as a potential bridging case between our American historical archaeology and my Mississippian data set: European Medieval society is Mississippian in time period, and is the ancestral cultural tradition to most American historical societies.

I found that Medieval archaeology is caught in the same particularistic, architectural, antiquantitative focus that South (e.g. 1977) attacked for us in the 1960s and 70s. The situation has changed little since White (1970:3) wrote that medieval studies are “word-bound, almost entirely focused on extant documents.”

My reading of the journal found essentially no research design, no quantification, and only rare discussion of field methods. Volume I of Medieval Archaeology was published in 1957. The first “methods” section I found was in 1964 (Addyman 1964). The excavators shovel skimmed and trowelled, but the author made no mention of using screens. The next such section was in 1969, and was the same (Hurst and Hurst 1969). The first and so far only discussion of “sampling design” appeared in 1982 (Astill and Lobb 1982). The authors evaluated “sieving” and suggested that screens made the excavators too careless about finding things in situ. The first, and only, solid article based on quantification of animal remains appeared in 1988 (Gilchrist 1988). On the whole, this is archaeology as illustration, not a historical archaeology at all.

And yet, Medieval Europe as background to European colonization is crucial to our study, even in its narrower sense. Some of our colleagues, of course, have recognized this. I think one of the more intriguing points of Deetz’s (1977) In Small Things Forgotten is that the shift from a medieval to a modern world view occurred largely during the eighteenth century. Studies of medieval foodways, among others, provided the background for that study. But the background studies were done for American historical archaeology, not by medieval archaeologists, and they were done with historical sources, not archaeological data.

So, the relevance of Medieval archaeology to American historical archaeology is clear. How can it be compared to Mississippian society?
The knee-jerk reaction is, well, it can't. Mississippian is Neolithic. Medieval has steel, wagons, ships, draft animals. Mississippian is chiefdoms. Medieval is state societies with market economies. No comparison. It doesn't work.

Really? Consider the burial statistics from a couple of Medieval British cemeteries (Steane 1984:94-95). At Wharram Percy, the evidence is of an average life expectancy of 35 for males, 31 for females; less than 10% lived past 50. Males averaged 5’6” tall. At St. Helens-on-the-Walls, York, average heights are 5'6" for men, 5'2" for women; 9% lived past 60, while 27% died as children (and infants were under-represented).

These statistics sound very Mississippian to me. So the British had steel, wagons, draft animals--how much difference did that make to the average guy trying to scratch a living in the field?

How about the way a house was organized and equipped? or a village? This is the scale at which the average person lived his or her life. But to compare them, we have to compare a historical database in Britain to an archaeological one in the Mississippi Valley. That is what does not work.

John Steane's (1984:28) summary of Medieval archaeology in England and Wales states that “the Early Middle Ages in England saw a hierarchical society emerge, composed of classes each of which performed set functions in human affairs...” His statement is based on historical and architectural data. How do we trace intravillage socioeconomic distinctions via archaeological patterning? How do they compare to patterned distinctions in the development of ranking in Mississippian society? We do not know--we do not have the archaeological database in Britain, and we do not have the methods to compare the archaeological data. Historical archaeology is developing those methods, such as pattern recognition and socioeconomic indexing. Can we generalize them?

The point of all this returns to my position that historical archaeology has much to contribute to broad issues. Historical archaeology is the archaeology of literate societies. It is the archaeology of societies utilizing, and influenced by, literacy. The archaeology of history offers us the opportunity to build models informed by documentary and archaeological data, to test hypotheses and methods, to range analytically across complex societies and to provide a basis for comparing literate to nonliterate cultures. I do not think that we can allow the documentary record to dominate the archaeological, thus stifling the creativity of our methodologies. I do not think that we can allow historical archaeology to turn inward upon itself, never looking beyond the temporal and geographical boundaries of European cultures of the last five hundred years.

I am not saying that any of us is wrong to concentrate on historic sites in North America. Nor am I saying that scholars from disciplines other than anthropology can or should be excluded. I am saying that historical archaeology is a broad and inclusive field, and once in a while, it will do us good to think of it that way.

Note
1. Although I appreciate the integrity of this reviewer in signing the comments, the review of course was not meant for publication. I will therefore not identify the reviewer.

REFERENCES
Addyman, P. V. 1964 A Dark-Age settlement at Maxey, Northants. Medieval Archaeology 8:20-73.
Cleland, C. E., and J. E. Fitting  
Deetz, J. F.  
Dollar, Clyde D.  
Dornberg, John  
Gilchrist, Roberta  
Goody, Jack  
1986   The Logic of Writing and the Organization of Society.  Cambridge University Press.
Harrington, J. C.  
Houston, Stephen D,  
Hurst, Gillian, and John G. Hurst  
Kardulias, P. Nick  
Leone, M. P.  
1979   Comments on Method and Theory in Historical Archaeology by Stanley South and on In Small Things Forgotten by James Deetz.  Paper presented at the Society for Historical Archaeology, Nashville.
Noel Hume, Ivor  
Oppenheim, A. Leo  
Rogoff, Barbara  
School of American Research  
Schuyler, Robert L.  
South, Stanley
Steane, J. M.
Wesler, Kit W.
White, Lynn