ARCHAEOLOGY AND TRADITION AT WICKLIFFE MOUNDS

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ABSTRACT

A recent issue of *American Antiquity* presented two articles about the utility of Native American traditions in archaeological interpretation. Ronald Mason argued that oral tradition is irrelevant to a scientific archaeology. In contrast, Roger Echo-Hawk argued that some traditions offer historical insight into a past as remote as the terminal Pleistocene. I consider these two positions via interpretations of Wickliffe Mounds and Mississippian research issues in the light of Chickasaw oral traditions regarding craft specialization, village landscape, and artistic motifs. Oral tradition may be useful to a scientific archaeology if elements can be recast as testable hypotheses, but even if they cannot, traditions offer a perspective that archaeologists may find interesting.

Last year, an issue of American Antiquity presented two articles about the relationship between archaeology and Native American oral tradition. While the two authors, Ronald Mason and Roger Echo-Hawk, actually had some area of agreement, the effect of their papers was one of diametrically opposed arguments: Mason concluded that there was little if any room for oral tradition in a scientific archaeology, while Echo-Hawk affirmed the essential historicity of fundamental elements of current bodies of tradition. Having made a few Native American friends in my work at Wickliffe Mounds, and learned some interesting bits from their traditions, I would like to consider the two positions and how tradition and archaeology might interact in the interpretation of this Mississippian site.

Mason began his discussion by noting the distinction between oral history and oral tradition. Oral history is a recollection of first hand experience. As such, oral history reaches no further into the past than the memory of the oldest living informant.

Oral tradition, on the other hand, is information handed down from previous generations, "believed by their narrators to be more or less faithful renderings of the older happenings to which they refer" (Mason 2000). In traditional Native American culture, oral tradition is respected as a "legitimate repository of knowledge about the past" (Mason 2000), although scholars of oral tradition recognize numerous ways in which any given story might be changed, abbreviated, exaggerated, or recast for deliberate effect. Oral tradition is a
living part of a culture, and as such, serves the needs of society today, however much or little it reflects the needs and experiences of past situations.

Mason considers a number of arguments for and against using oral history in archaeological interpretation. Archaeologists or other students of the past who promote the reasons for using oral tradition accept either the legitimacy of oral tradition as history or the illegitimacy of science as a world view—saying, in essence, that science is an ethnocentric western view of the world, and is no more valid than any other belief system.

Mason rejects these positions. His reasons against using oral tradition include the proven lack of trustworthiness of oral tradition as history, the embeddedness of oral tradition in living culture, and an explicit denial that science is merely another belief system. He argues that science is a hypothesis-testing body of knowledge that rejects ideas that are disproven. Oral tradition, in contrast, is "impervious to external challenge." Those who believe implicitly in oral tradition take an essentially fundamentalist view, and do not allow for any evidence that does not validate their history.

Mason concludes that oral tradition is not reliable for chronology, and is simply a different kind of information, effectively impossible to correlate with a scientific archaeology. "Some... efforts at using tradition have proven worthwhile, more seem
trivial or even worthless, while most are of indeterminate value because of problems of testing."

Mason cautions that a scholar who tries to use oral tradition must evaluate the source critically and avoid picking and choosing those elements that are useful while ignoring those that don't fit the preferred scenario. In other words, we must use oral tradition as a systematic data base, reliable or unreliable in sum, and not in convenient pieces. In this perspective, the problems of oral tradition in archaeology seem insurmountable, and Mason refuses to endorse any such effort. "Oral traditions are more often than not roadblocks than bridges to archaeologists aspiring to know 'what happened in history.'"

Roger Echo-Hawk (2000), on the other hand, affirms that "oral traditions provide a viable source of information about historical settings dating far back in time." He notes, as Mason would agree, that many uses of archaeological and traditional data do not follow the strictures of scientific evidence: legal arguments, for example, or NAGPRA proceedings follow different rules of evidence and may weigh tradition against archaeology without prejudice against either. Echo-Hawk also recognizes the potential for flawed or even bogus traditions, and proposes three tests that traditions must meet before they may be utilized in reconstructions of the past. Most important to me is his Test 3: a tradition "must be supported or verified through evidence gathered from independent, nonverbal
It seems to me that Test 3 undermines his whole stance that oral traditions may be reliable in the first place. More than that, his examples fail to refute Mason's dismissal of traditions as dubiously reliable and their use as unsystematic.

I'll pick only one of Echo-Hawk's examples. He suggests that Arikara origin myths contain actual cultural memories of the crossing of Beringia in the late Pleistocene.

According to these myths, the Arikara began their journey in a dark, cold place. They passed several barriers before arriving in their historic homeland, including a body of water, described in one version as "wide, thick ice and deep water," a great forest, and a deep canyon, these barriers "appearing in variable order, with different descriptions." Echo-Hawk's order implies strongly that these barriers may be equated with the Bering Strait, the northwestern forests, and the Grand Canyon. (I'm simplifying considerably here).

Echo-Hawk interprets the dark place of origin as Beringia. "I view the theme of emergence from an underworld/region of darkness, as a distorted remembrance of Beringia and the Arctic Circle... The historical specificity of a land of lingering darkness as a place of origin... preserve[s] memories of ancient Beringia..."

This interpretation might work for me if we assume that the Arikara ancestors experienced Beringia only in winter. In the
summer, Beringia would have been a land of lingering light. It seems to me that Echo-Hawk's interpretation tells us more about his own stereotype of Arctic climate than it does about the Arikara past.

In sum, his examples strike me as exactly the kind of cherry-picking, seat-of-the-pants interpretation that led Mason to reject oral tradition.

Echo-Hawk concludes, "Archaeology is inherently multidisciplinary, so the study of oral literature should exist as one more realm of legitimate inquiry... to add oral information to our models of human history." His call on multi-disciplinary inclusion notwithstanding, I do not find his argument compelling at all.

And yet, it seems to me that Mason also misses a point. In his call for the exclusion of oral tradition from a scientific archaeology, he does not seem to allow for any possibility that we may use tradition as a source of testable hypotheses.

We've all, I think, had the lecture--if not given the lecture--about archaeology as science: hypothesis, test implication, test, confirm or disconfirm, revise and retest, etc. However, what we often neglect to discuss is that hypotheses can come from anywhere: deduction from a general proposition, induction from known data, daydream, nightmare, drug-induced hallucination--it doesn't matter, as long as it is testable and we are prepared to reject the hypothesis if the data don't support it. Thus--oral tradition
may potentially be a source of testable hypotheses, and as testable hypothesis, may be incorporated into scientific archaeology.

I'd like to consider a couple of ways in which these ideas about oral tradition play out in my long-term thinking about Wickliffe Mounds.

Two summers ago, we had two interns at Wickliffe who were also Chickasaw tribal members. One, LaDonna Brown, had been raised in Oklahoma in a very traditional society. I started thinking about the issues of tradition and science in archaeology one day when she picked up a bone fishhook from the Wickliffe collections and began telling me that, traditionally, fishhooks were made by a specific clan.

This idea gave me a whole new perspective on a long-running debate about Mississippian societies and about chiefdom-level societies in general: whether these societies supported specialists. Jon Muller, for example, has argued numerous times that there are specialized sites, such as his Great Salt Spring in Southern Illinois, but that there were no craft specialists in Mississippian society. That is, there was no one who made his or her living by making certain kinds of artifacts, such as shell beads or fine pottery, supported by surplus production of the community in order to produce particular goods for general or elite consumption. Most of us, I think, expect that certain individuals were better at certain crafts than others, and maybe a skilled knapper made a few extra Ramey
knives to trade for a fine potter’s best effigy. But on the whole, most Mississippian households were self-sufficient for crafts, trade items, and food production and preparation, including the chiefs’ households. In the absence of a market economy or true coercive power, which we don’t see in Mississippian society, there were no occupational specialists.

Other archaeologists (e.g. Yerkes 1991) do see evidence for craft specialization, in fluorspar and shell bead production, shell carving, flint knapping, or other items. One version of the argument holds that some artifacts were produced for elite exchange, and that the craftspeople were attached to the chief’s household and were supported by the chief out of surplus amassed through tribute and tithing.

These two positions have become two dialectically opposed arguments. It is no coincidence that both are based on our experience in a capitalist, market-based economy: either there is specialization, or there isn’t.

LaDonna Brown’s story provides a third perspective, based on an entirely different social ethic. As I understand it, one clan had a ceremonial or social responsibility to make fishhooks for the rest of the tribe. Any person could have the tools or skill to make a fishhook, and probably did if none of the hook-maker clan was around. But in the normal course of life, a person needing a fishhook went to a member of the proper clan to obtain fishhooks. They didn’t
make their living that way, but they had the right and the responsibility to do it. This is a whole different way of looking at specialization, which can be found nowhere in the archaeological literature.

As a second example, consider the woodpecker gorget motif. This is a Cox style gorget that was found at Wickliffe in the 1930s (it disappeared before we took over the site). The Cox style has the four woodpecker swastika motif, the looped square, and the cross-and-circle inside. There are a number of examples with the same set of motifs.

James Brown interprets this motif in a particular way. To him, "the pileated woodpecker... represented warriors... [and] was conventionally associated with the war hatchet that was shown in images with chiefly identifications" (Brown 1991:533). “The looped square frame is a transformation of the litter...” (Brown 1991:532) on which chiefs were carried as a sign of their status.

LaDonna Brown, never having read James Brown’s work or anything citing it, took one look at the gorget design and recognized the looped square as the path that traditional dancers take around the square ground. If you follow the lines, you’ll see that each corner loops around and comes back over; the lines form a continuous path, not the crossed handles of a carried platform. It seems to me that the dance-path interpretation is a lot less labored than the litter idea, makes sense, and has the advantage of coming from a perspective
inside a descendant culture.

There’s yet another idea that I’ve been playing with, one that LaDonna and I came up with cooperatively while talking about clans and mounds. To explain this, I have to digress a little about the Wickliffe map and Mound E.

Here is the original map of the Wickliffe site, Loughridge’s 1888 sketch. You’ll note the two platform mounds and a set of smaller mounds, including one off in the northeast associated with an embankment. The embankment is not visible now, but I suspect it marked the edge of the village, and possibly had a palisade on top of it. Most of the small mounds, in fact, are hard to find today due to excavations, plowing, and other disturbing factors.

Note the small mound in the northwest. In 1932, Fain King and the Alabama Museum of Natural History investigators put three tests in this vicinity, labeling the tests E1, E2 and E3. King later dug Mound E with a grid that covered nearly a hundred-foot square. I spent two summers testing in this area in 1988 and 1989, found one disturbance that I think was one of the 1932 tests, but was unable to find a large backfilled excavation. After that, I dismissed the idea that Mound E was up there. King’s Mound E excavation, I think, actually was in the area south of Highway 51, now thoroughly destroyed.

Looking back at the Loughridge map, I also noted that Mound F, which was directly west of Mound A, the bigger platform, is not
depicted. I therefore concluded that Loughridge had mismarked the small mound in the northwest, when it actually was Mound F, which should have been on the ridge extension farther south.

I am re-thinking the idea that Loughridge misplaced Mound F. Looking at the bluff edge where Mound F should be, I see an area that seems to be drawn as brushy. If Mound F was as small as the other mounds, and the area was brushy, Loughridge might well have missed it. On the other hand, since he obviously knew what he was looking at in the open areas, there is no reason to doubt his powers of observation in the northwest. I think now that it is very likely that there was a mound in the northwest and that he marked it correctly, but missed Mound F due to brush. I still think King dug his Mound E south of the road, but that’s another story.

This all becomes relevant to today’s topic because one day LaDonna Brown and I were discussing the clan structure of the Southeastern tribes. She pointed out that there are seven basic clans in the Southeast; they now have subdivisions and intricate relationships, but all relate back to seven basic clans. One of those clans, she said, was traditionally the one from which the chiefs came—the hereditary elite that fits our notion of chiefdom societies and, particularly, Mississippian societies.

Now we look back at the Wickliffe map. We have a central precinct, the two platforms and Mound D (drawn as a pair on Loughridge’s map) defining the plaza. This would be the chiefly
mound area. Then we have one, two, three, four, five, six mounds forming a near circle around the center. Seven mound areas for the seven clans.

It would be very interesting to locate accurately all six of the outlying mounds, to see if they do indeed form a circle—and then to do a test to find out what’s at the center point.

If you look at Melvin Fowler’s Atlas of Cahokia, you’ll see that Cahokia is set up the same way. There’s the central plaza precinct with Monks Mound, and there are six subsidiary plazas clustered closely around the center. Seven plazas, seven clans.

You should already be squirming with the primary objection to using these traditional data as legitimate interpretations of the Wickliffe landscape, fishhook specialization, and gorget motif: they are not testable. I can’t see how to make them testable hypotheses.

Two of the ideas--the fishhook clan and the clan mounds--require creating test implications that identify clan occupations. I don’t know how to do that. I’ve looked at the distribution of ceramic effigies as potential clan markers, but I don’t see any clustering, any pattern. In fact, since the clans were exogamous, clan members would be thoroughly mixed among the households of the village, and I would not expect any sort of symbol clustering. Fishhook manufacturing debris unfortunately is too infrequent to look for clustering in it, at least until we have a thorough analysis of all of the faunal remains. And how would we test for a looped square
as the representation of the dance path? I don’t believe that the litter idea has been logically tested, either, but that’s not an argument for the other idea.

I have, therefore, to agree with Mason: for a truly scientific, hypothetic-deductive strictly logical archaeology, these interpretations based on traditions are not useful. They cannot be tested; they cannot be formally verified, or more formally speaking, cannot be disconfirmed. They are unverifiable hypotheses, which have no place in a scientific explanation.

But I don’t think we can simply ignore them, either. They offer new perspectives that we--being mainly from a western, capitalist, educated culture and world view--have not thought of; perspectives that at best make a lot of sense, and at least are provocative and intriguing. I can’t offer these ideas as firm conclusions. But for my own understanding of the site and the people who created it, for less formal interpretation to museum visitors of all cultures and backgrounds, and for sheer emotional satisfaction that Native American voices can be heard teaching us about a Native American village, I think these traditions contribute a richer, more vibrant, more culturally grounded, and more humanistic picture of a Mississippian town. For these reasons, I will try to keep speculation, interpretation, and scientific explanation explicit in their own domains, while I celebrate the survival of traditions that link us to the Mississippian ancestors.
References

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