



Comments on Espenshade's A River of Doubt: Marked Colonoware, Underwater Sampling, and Questions of Inference

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Comments on Espenshade's *A River of Doubt: Marked Colonoware, Underwater Sampling, and Questions of Inference*

By Leland Ferguson

I agree with Christopher Espenshade: I did not *prove* "an association of marked Colonoware bowls with riverside ritual in 18th century South Carolina;" that is, unless your requirements for proof are low. I did, however, strongly suggest that to have been the case. In "The Cross is a Magic Sign" (1999), I presented the data on marked vessels I had available in the late 1980s and early 90s, and in *Uncommon Ground* (1992), I wrote a fictional account of a low country African throwing a marked bowl into a river.

An archaeological association of marked and ring-based bowls with South Carolina low country rivers is definite -- the artifacts have been found along river bottoms. Nevertheless, my tentative interpretation of the ritual use of these is an hypothesis: It was based on a small sample of archaeological data, and my reading of history and ethnography, from both the Americas and West Africa. I believe it has strong merit, and I encourage other archaeologists to more rigorously test this hypothesis and to expand our knowledge of early African-American history and culture.

Logical scholarship -- social science in our case -- has harsh rules. We must first use our observations and imaginations to create likely stories. Then, we must employ those same observations and add more data in our best attempts to prove those favored stories wrong! Our method requires that we frequently drop anchor in Espenshade's *river of doubt*, testing and retesting the validity of our hypotheses. Those we can't prove wrong, we accept as true, tentatively.[1]

Both imagination and doubt are essential components of the process. Espenshade finds my work, and especially interpretations of that work, heavy on imagination and light on doubt. He makes a legitimate, though arguable, point. In turn, I find Espenshade's critique excessively weighted toward doubt and lacking imagination. His firmly stated conclusion that "these marked bowls *are* simple refuse" (emphasis added) seems unnecessarily negative and founded, itself, on no more than untested, casual observation. There is no wiggle room: he says the marked bowls are rubbish and offers nothing more. Of course, here, I think Espenshade, like those of us he criticizes, overstates himself. I trust he would agree that some of the marked vessels *could* have been used in ritual, at the riverside or elsewhere. How could anyone say otherwise? The question is how much? Based on methodological parsimony, his hypothesis is not very many, perhaps none.

I too have used parsimony in interpretation of colonoware, with surprising results. In *Uncommon Ground*, I suggested that the vast majority of colonoware was probably used for preparing West African-style meals -- jars for cooking and bowls for serving. This was an interpretation that I considered parsimonious. But, what is parsimonious from one cultural perspective may not be from another. Later, when I showed pictures of the material to West Africans in Sierra Leone, most responded that the vessels looked to them

like "medicine pots" rather than cooking and serving vessels; another West African suggestion was that the small vessels were used for ablution, including feminine hygiene. I certainly don't know if this is the case, but what I do know from these informants is that what I considered parsimonious was an ethnocentric notion.[2]

Below, I respond point-by-point to Espenshade's problems with the waterside ritual argument:

1. *The argument is based overwhelmingly on materials from sports divers.*

The term "sports divers" is Espenshade's, not mine. I wrote that the collections were donated by "nonprofessional archaeological divers." This was a carefully conceived, if rhetorically awkward, term intended to include professional treasure hunters as well as sports or hobby divers. The terminology has a slight relevance to the issue as explained in my next response.

2. *The argument ignores that sports divers collect a whole range of domestic refuse from the rivers, not just marked bowls.*

I was well aware of the variety of materials coming from underwater, but I did not ignore this variety to bolster my argument. At the time of my writing historical archaeologists had found tens of thousands of sherds of colonoware on terrestrial sites -- excavations at Yaughan and Curriboo alone produced more than 18,000 thousand sherds, *one* of which was marked. Relatively, the amount of material coming from the rivers was quite small, and there was no indication that divers were intentionally biasing their collections (Ferguson 1999: 128). Also, more vessels with ring bases were coming from the rivers than from sites on land. Thus, it appeared that there was a significant association between marked vessels, bowls with ring bases, and waterways. As Espenshade states, the rivers were so dark that divers usually picked up items by feel, then culled above water. At the time, there was no relic market for colonoware, so treasure hunters had little interest these plain bowls and jars. Hobby divers seemed more interested in fancy-looking artifacts plain colonoware, marked or not. Divers were recovering far fewer artifacts than terrestrial archaeologists, yet they were recovering a much larger proportion of marked bowls.

True, I did not include all of the other material in my discussion. It was obvious that in the rivers adjacent to plantations, ferries, and bridges that there was a great variety of items to be found in the rivers. It is possible that many, or most, of the marked pieces found their way beneath the water from erosion or intentional dumping of trash in the river. As Espenshade argues, the issue clearly calls for a careful study of site formation processes, as well as spatial and temporal variation.

Bakongo ritual involving the cosmogram is not limited to bodies of water. Because most of the marked vessels at the time I was writing came from underwater, and because historians and ethnographers had emphasized water, I placed emphasis on the water side aspect of ritual. However, the vertical line in the cosmogram represents the pathway from the living world to the world of the dead, underground. Most all underground is moist and watery.

Thus, although passing through a body of water is considered the quickest way to connect with the underworld, connections can be made through almost any opening in the earth. Thus, if a vessel were used in a related ritual it might be put in a trash pit, down a well, beneath tree roots, in a privy, or any other hole, as well as through water. Or, it may not have been necessary to put the marked artifact in any of these kinds of places to have the magic work.

3. *The argument is overly broad in considering any crossed line design similar to a cosmogram.*

The pattern we perceived in the late 1970s and early 1980s was one of marks in the center of colonoware vessels, either on the inside or outside and put on the vessel either before or after firing. I say "we perceived," because many people were looking over the collections in those days at the South Carolina Institute of Archaeology and Anthropology, and most were commenting on the marked vessels. Occasionally, other scratches and lines were observed, but they were not perceived as a pattern. Of course, this does not mean that there is not another pattern, just that we didn't recognize another regularity.

Espenshade is wrong in his assertion that I considered *any* crossed line design to be part of the pattern. Category I of the three categories I described in the 1999 paper were not included in the analysis, because I believed they were not part of the pattern. This category included four marked pieces, two with initials and two with painted crossed lines that appeared to be part of a larger floral design.

4. *The argument generally ignores well-dated, marked bowls from terrestrial contexts.*

When I originally wrote "The Cross is a Magic Sign," I did not ignore any well-dated vessels, or those from terrestrial contexts. Had I known of any such vessels I certainly would have used them. Some later data from the Bonny Shore site was included, and I thanked Espenshade for providing that information (1999: 127). Nevertheless, he appropriately calls me on not updating the paper.

My paper was written and submitted for publication in 1989. I anticipated that it would be published within a couple of years, and that it would be the basis for a section on marked vessels in *Uncommon Ground*, published in 1992. However, *Uncommon Ground* was written after the 1989 meeting and published seven years before *I Too, Am America* (1999). In the middle of the 1990s, I requested that my paper be withdrawn from the volume because I knew that more data had been recovered. However, I was assured that the volume was moving along. By that time I was involved with other matters, and I let the paper go to press as it was -- a mistake.[3]

Concerning publication and dissemination, I believe Espenshade again exaggerates when he describes the "myth of the gray literature" as belief "that only books published by major presses *can possibly* inform our discipline" (emphasis added). I'm familiar with complaints that the gray literature is not easily accessible, but I don't know anyone who

subscribes to Espenshade's extreme position. Surely, Espenshade, himself, would consider this hyperbole. Nevertheless, I do believe we have a problem with publication.

For the most part, the gray literature serves as a kind of localized primary source, not a means of wide-spread dissemination. On the other hand, edited volumes in trade and university presses don't do such a good job with dissemination either. They come out no faster than the slowest person involved, and the final decision for publication is not in the hands of archaeologists. Without question, our professional journals are the best means for timely dissemination, controlled and refereed by archaeologists. Neither Espenshade nor I have taken appropriate advantage of this valuable venue in publishing on colonoware. Of course, the online *African Diaspora Archaeology Newsletter* is another valuable means of distributing information to other archaeologists.

Espenshade's complaint that cultural resource management reports were not utilized in Singleton's (1999) volume appears unfounded. Throughout the bibliography are various management reports and manuscripts listed as "on file" at various institutions. William Kelso and associates, alone, account for five of these. If by CRM, Espenshade means only reports by contract archaeologists, there are fewer. However, there are several references to published articles by contract archaeologists based on their management reports; articles by Ronald Anthony, Patrick Garrow and Thomas Wheaton are examples.

5. Most of the vessels considered in the argument lack solid chronological control.

This is true. Most of the vessels used in my analysis came from underwater contexts and were not well dated. Twenty to thirty years ago, excavations were being conducted on a number of 18th and early 19th century plantation sites, and archaeologists were recovering an astonishing amount of colonoware. It appeared from what we saw at the time that the material was most popular in colonial and early post-colonial times and that it diminished in the 19th century. The marked vessels in my sample may well have come from the early 19th century, or even the second quarter of that century.[4] But, it appeared more likely that they were earlier.

In considering a later dating for the marked vessels, Espenshade writes that "perhaps the cross was reborn and recontextualized in Gullah Christianity, rather than having direct continuity with African cosmograms." This could be. I would never underestimate how complicated the history of southern America might be, especially the history of enslaved African-Americans. In the 1999 paper (p. 124) I wrote that "In addition to Bakongo and other African beliefs, plantation people must have known, in varying degrees, of American Indian cosmology, Islam, and Christianity."

In his conclusion, Espenshade writes kind words about my contribution to the archaeology of early African-Americans; I appreciate this sentiment but would emphasize that I was not alone. Subsequently, he says that "unfortunately, some of [Ferguson's] arguments require revision in light of broader data." I do not view this as unfortunate at all. In fact, I am disappointed that more of my early work, and that of others, has not been revised and expanded with new, imaginative and well-tested interpretations of early African-American

lifeways; and I am pleased Espenshade has refocused attention on these intriguing marked artifacts. He and I both know of data on marked vessels that is not readily available to students and other professionals. Perhaps we can find a way to make all this data more readily available to others. Then, more scholars could join the hunt for the meanings scratched on these vessels.

Notes

[1]. Einstein demonstrated Newton's elegant laws did not always hold true, then, recently, Einstein's *constant* speed of light has been clocked as low as 38 m.p.h.! After the slow-down, Harvard researchers actually stopped the light, transformed it to matter, and later regenerated the light from the matter. As the Apostle Paul observed, we "see through a glass darkly."

[2]. Soon, I will submit my most recent paper on medicine and colonoware to the *African Diaspora Archaeology Newsletter* for publication.

[3]. There is an accounting error in my 1999 paper. On page 121, I state that I have recorded 28 marked vessels or vessel fragments from South Carolina collections, referring to Table 6.1. But, there are only 27 listed in the table.

[4]. I know of unsubstantiated reports that it was being made in the 20th century.

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