The Dominance of the Document and the Arrogance of the Artifact: A Prehistorian's View

Larry J. Zimmerman

As a prehistorian I have a fundamental bias toward the artifact—the wrought—and that colors my view of historical archaeology and its use of documents—the written. Each time I have practiced historical archaeology in my career, it was largely by accident. In these projects, an excavation of outbuildings at an 1880s house (Lass 1979) and field survey with the Northern Cheyenne regarding the Dull Knife Outbreak of 1879 (McDonald, et al 1991), consideration of relationships between documentary and material were only afterthoughts, but nevertheless proved to be challenging. In the Austin-Whittemore House project, every document indicated an elite, wealthy family living on the frontier, but the artifacts contradicted the written in every way. They pointed to a largely utilitarian, simple existence similar to those less well off who lived nearby. For the Dull Knife Outbreak, document and artifact relationships were especially important because there was conflict between two very different types of documentation, military records and Northern Cheyenne oral tradition. Artifacts resolved the conflict, at least from the viewpoint of the Cheyenne.

For me, there is no fundamental difference between the epistemologies—how we know what we know—of prehistoric and historic archaeology. Yet most historical archaeologists apparently think there is. In discussions about their dissertations with Allyson Brooks and Beth Prine this past year, and reading historical archaeology papers in preparation for the session on which this volume is based, I have been truly puzzled. I told Allyson that my title would be the title it is now, but besides the alliteration, I did not exactly know why I chose the word "arrogance." As the first papers came to me, I thought I might change the phrase to "Absence of the Artifact," for it seemed to me that that must be what historical archaeology had become.

Frankly, on first reading, many of the papers were terribly annoying. With few exceptions—Martin Hall and his gables, Yentsch's beads, Bell's pipe and some others—the written seemed absolutely pre-eminent in the majority of papers. They seemed like weird histories, not archaeology at all. Certainly some like Kirch did demonstrate the importance of complementary sources, but most just raised old ghosts for me.

The ghosts, if a gendered stereotypical vision may be pardoned here, were in a knock-down, drag-out, hair-pulling fight. One was Noel-Hume's "handmaiden to history"—proper, virtuous, vacuous, but literate and, I imagine, rather thin—adding a bit of color to the life of her lady from the Castle of the Big Event and Important Personages. The other was what Jim Deetz might consider to be a "scrub woman"—ill-kempt, dirty, street-wise, illiterate and, I imagine, rather robust—making her own action in the quest to understand the big picture and meaning of life for the forgotten everyperson. These ghosts apparently still haunt most historical archaeologists. At the risk of repeating what many others have written, history is not

Larry J. Zimmerman, Archaeology Laboratory, University of South Dakota, Vermillion, SD 57069

archaeology and archaeology is not history nor is it history's handmaiden. Nor, I must emphasize, is it some weird hybrid. One important goal of archaeology, as several of the authors in these papers reiterate, can be to seek normative human behavior, basing interpretation of these norms on material remains. Important events and great people are, with rare exception, the focus of history which uses documentary evidence almost exclusively.

With these images and issues deranging my thoughts on the papers, I began to pester Allyson Brooks to throw in some slides of maps, bottles and tin cans for her SHA presentation. As I did this, I began to realize why I had initially chosen the phrase "arrogance of the artifact." The artifact is the more powerful source of information when it comes to accomplishing archaeology's goals, and the historical archaeologist must remember that. To follow D'Agostino's comment (this volume), archaeology seeks "to go beyond writing site reports to interpret culture and actually write ethnographies..." and to look beyond the site to local, regional and world-wide levels, or as Deetz and White say in their paper, to relate the data, both written and wrought, to the larger context with vertical rather than horizontal linkages. Archaeology generalizes, history particularizes and that gives each its power. For archaeology, the artifact is the more potent tool because it is more removed from us than the document. In the artifact the idiosyncrasies, gender, class or other characteristics of the maker may be utterly absent, forcing the researcher to seek commonalities and meanings by comparisons to other objects. From these comparisons archaeologists interpret the norms in the everyday life of common people. Comparison forces a search for vertical connections from which contexts are derived. The wrought contributes some of the distance needed from the subject to help us objectify it (that is, to be objective). This is certainly what Deetz (1993:12) must mean when he writes that material culture provides a point of departure (from history?).

The written is profoundly seductive. As a case in point, several authors in this volume start with the written and then move to the wrought. The prehistorian in me objects to this, but I understand the temptation. Archaeologists are literate people, in Walter Ong's (1982) sense of the word, and we thus recognize the power of the document to transcend time. We are fooled by it into a false "present" that seems to allow us to get close to the subject of our research, perhaps even to see clearly the individual behind the document. In Johannes Fabian's (1983) terms, we make the document (and the individual) co-eval to us, that is to say, we bring it casually into our own time and therefore cannot easily objectify it. Seeing beyond the document becomes difficult, pushing us toward subjectivity and historical particularism thereby deflecting us from normative assessments.

Post-processual quests for meaning and intent exacerbate this problem. To address the emic in the archaeological record we erroneously fall into the trap of thinking that the written is equivalent to an ethnographer's interview (cf. D'Agostino quoted above). The problem, of course, is that the ethnographer is on hand for the interview and sets its agenda. It is a context of orality, set in the present, where the ethnographer is able to ask for clarification and can provide first-hand contexts of emotion or other conditions. Readers of documents cannot do so. Their context is literate and set in the past (or future). Therefore, all usages of the document becomes interpretation.[1]

Rather than for us to attempt to address questions of emic (and etic) in the archaeological record, we might more productively follow F. Allan Hanson's (1975) related

notions of intentional versus implicational meaning. Intentional meaning is what someone had in mind when doing something; implicational meaning is an assessed result of that action, most accessible to an outsider, the researcher. The written absolutely may allow us to assess someone's stated intention, but it can be profoundly and purposely misleading. Intention, by definition, cannot be found in the wrought. The implicational meaning is always an interpretation based on broader social, spatial or temporal context and can be found in both the written and the wrought.

What this all boils down to is that historical archaeologists must remember that the written is also the wrought. Documents are artifacts, something apparently easily forgotten by historical archaeologists (and historians, by the way!). When people create and then tell an oral history over generations; when an artist makes a petroglyph; when a clerk prepares an inventory; when someone publishes a newspaper account; they have an underlying intention. That intention is to somehow modify the perceptions and behaviors of those around them, both contemporaneously and perhaps even well into the future. True intentions may be unfathomable.

At the same time, each document also has implications for its surroundings. As Brooks, following Moreland, ably demonstrates in her paper, the written word is a form of material culture that is in a recursive relationship with human behavior. Documents are the products of human action having the power to transform social organization and the cultural landscape. Her mining journals and newspapers are compelling examples.

If archaeologists do not understand these differences in meaning, they will likely treat the written in too cavalier a fashion. They will assume too much. Because the language or medium, though not precisely that of contemporary usage, is about the same as that of the archaeologist, they might be tempted to assume the context and meanings are the same. Intellectually archaeologists know this may or may not be true, but they get sloppy. Historical archaeologists using documents suffer the same problems as prehistorians who, in trying to interpret function or meaning of an artifact, are willing to rely on fuzzy ethnographic analogies.

This means that the complementary nature of written and wrought can actually be fallacious, or at very least, it may be very difficult to shift between them. This is what I see in Mark Hall's fascinating paper on Viking ironworking. I think his assessment is probably correct. Although there is a real possibility of reconstructing various social aspects of ironworking, the connections are not entirely clear to me. What are the implicational meanings for the material culture if the process described were not used literally? I am sure that Beth Prine is really onto something with her interest in frontier community structure, but what are its markers in the material record? As for Hall, he will be troubled by the old bugaboo of the "historicity" of oral tradition. Casella has an equally daunting problem. Her intuition seems reasonable, but there is a very big methodological jump from Victorian gender roles, described in secondary sources, to prison architecture, even with intervening letters from a superintendent that seem to contextualize prison layout. Are there intervening or overarching complexes of meaning such as that of the panopticon (Leone 1992), which in the case of women's prisons may actually have been contravened?

In part, what I recommend that historical archaeologists try to do to make the written and wrought complementary is to treat the written first as an artifact before any of its meanings are assessed or interpreted. At the risk of sounding like a closet processualist, archaeologists ought to be reasonably explicit about the steps they take. I do not have many of the answers about how to do this, but at very least most should be able to classify the types of documents they are working with. D'Agostino does this very well in her overview and I appreciate her table giving the correlates between household inventories and the archaeological record. Winer's superb classification of painted, poetic Eastern Cape landscapes carefully examines the contexts of each type of landscape, but says little about their material implications. At the same time, her work raises another old ghost, that of the mental template.

Each document has a morphology that is constrained by the physical properties of the medium used, the images of proper form in the mind of the creator and the intended audience. In landscape painting, style, perspective and technique put boundaries on expressive content, just as poetic structure may constrain word choice. As Bragdon certainly recognizes, the medium of the petroglyph constrains content and is indexical, symbolic jargon. For prisons, available space, materials, skilled labor and finances may dictate architectural style. The form of the inventory sheet may literally dictate the inventory. An analogy from our own work as archaeologists might be the descriptive field of an 80 column punch card from the 1970s limiting descriptors in our artifact catalogs, versus the nearly freeform databases now used. Historiographers speak of these problems with documents, but do not seem to be aware of morphological constraints and their impacts on both creators and perceivers of documents, either cross-culturally or cross-temporally.

Documents have a morphology just as other material culture does. The written has dimensions that can be measured, and those dimensions affect content. Form and content are inextricably linked and archaeologists should analyze both, as well as the linkage between them. They may develop a better vision of mental template if they do so.

The "opposite" of the mental template is the archaeological construct which recognizes that the analytical scheme used by the archaeologist may or may not have any relationship to the creator of the artifact or its social context. In post-processual approaches this concept is a given, but when archaeologists analyze the written, constructs are more difficult to accept because the artifact's creator seems so "close" to us. The archaeological construct, however, is more "honest" in that it makes not suppositions about intentional meaning and uses the construct only to seek the implicational.

Certainly there are more things archaeologists can do to treat the written as the wrought, but they are even more poorly formulated than what I have outlined here.[2] I must reiterate that I believe historical archaeologists place too great a reliance on documents for interpretation of intentional meaning without giving adequate consideration to the implicational circumstances available from the documents if they first dealt with them as artifacts. Archaeologists do this because they have accepted that there actually is interplay between written and wrought without much examination of crucial issues. Complementarity of the document and artifact may not be so easy as we assume.

This leads me to a final point: this whole process has a great power to generate what Kenneth Hoover (Hoover 1992:6) calls socially "compelling knowledge." Hoover contends that as societies function, they tend to rely on highly subjective accounts of life. These have limited utility and often can cause problems because they "form a poor basis for the development of common understanding and action." Non-critical uses of documents can contribute directly to these problems. If social knowledge is to be useful, "it must be communicable, valid and compelling." Do archaeologists understand this of their own work? If archaeologists wish to work in broader contexts, as many of the papers in this volume demand, do they recognize the potential impact of their work on contemporary situations? If they provide knowledge that is not communicable, valid and compelling, they risk contributing to contemporary social problems, and not helping with solutions. This is an excruciating problem for Hall's study and Franklin's slave quarter proposal, but it is equally so for all the rest. I am more immediately worried about the problem for Anderson and Kirch in their work on Hawaii, but by extension this also applies to Purser and her studies of the post-colonial Pacific Rim.

The immediacy of this work became abundantly clear when I recently met on O'ahu with a group of Native Hawaiians whose lives are being directly affected by development on the 1200 acre Kawainui Marsh. I also met with landscape architect Robert Herlinger who was trying to understand the impact of the Kawainui developments and make them less destructive to traditional practice. He is trying to fit sacred sites and pre-and post Contact economic and political patterns into his planning. To do this he has developed what is in essence a geographic information system of oral tradition and written documentation for the valley keyed to archaeological survey. The interplay of written and wrought has been anything but easy for Herlinger, but his plans are unquestionably socially compelling knowledge with profound implications for residents.

Another one good example of how all I have written about here plays out is in my colleague Richard Fox's (1993) book *Archaeology, History and Custer's Last Battle*. Fox began with the artifacts found in survey when fire burned the dense grass from the Little Bighorn battlefield. From distribution and type of artifacts he discerned intriguing patterns. He moved to the documents including Indian oral history of the battle, did a formal analysis of them and linked them carefully to the artifacts. He soundly demonstrated that Custer's last battle was largely comprised of tactical and unit disintegration caused by fear, a common battlefield trajectory. He utterly destroyed the myth that the last stand was in any way noble or heroic. He did not know how much his work affected people's lives until he started getting hate mail following publication of a summary article. Even though Fox is a decorated combat veteran, he was accused of revisionism, lack of patriotism and cowardice. He had not considered what destroying a sacred myth might mean to those who believed it.

By writing these things about the papers in this volume, I am certain to have belabored the obvious, and to some extent preached to the choir. Still, I do not think it hurts to be reminded that we are archaeologists, whether prehistoric or historic, and that what we do derives first from the wrought. The artifact lies at the base of our research and provides us with the best (though not the only) information to achieve archaeological research goals. We need to remember that the written is also the wrought and should treat it as such first before we try to derive other kinds of information.

From these comments, you should have figured out who won the fight between the ghosts. The handmaiden just did not stand a chance. The scrub woman came to the fight with better tools and a clearer vision of what she was about.

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Notes

1 See Walter Ong 1982 for clarification of orality versus literacy. The former emphasizes the present; the latter, the past or future.

2 Documents, like other artifacts, also have a taphonomy, for example.

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