

SUPPLEMENTAL COMMENTS

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When we realized a few years ago that the Arkansas project had not been properly published, we wanted to contribute to the High Plains Upper Republican debate by re-interpreting the Wallace complex. We were involved with this project from its beginning: Huffman recorded the Wallace site in 1964 and excavated Trench 1 (Withers 1965); he was part of the three-man team that uncovered House 1 in 1965 (Withers and Huffman 1966); and part of the larger team that further excavated Wallace and other sites in 1966 (Olson 1966). Throughout this time, we considered the Wallace and Hobson sites as Upper Republican: even the original site cards (DU S:9:24 and S:9:22) give that designation. Blakeslee is quite wrong on this point (Blakeslee Supplement 1), and it is unclear why this is even an issue.

At the time of our excavations, the standard theoretical approach was inductivism: archaeological data would speak for itself. Ironically, this is why we did not recognize the rounded corner in House 4A as part of House 1 (cf. Logan Supplement 3). To have made this connection, we would have needed a Plains lodge in mind: instead, we lacked a model altogether. Now we know from philosophers of science that it is not possible to derive answers directly from data. We emphasize this point because the critics seem to underestimate the impact of a traditional approach. Indeed, they criticize us for using a Plains model. Excavation methods can be theory neutral, but the results need to be compared to various models for the 'best fit'. The House 1 complex at the Wallace site does not fit an Apishapa model. Instead, the rounded corner shows that the original 'house' was the kitchen

of a much larger structure. Its size, shape, central fireplace, and substantial walls and roof are more like a Plains-style lodge than anything else. This strong correspondence remains true regardless of any other criticism.

For the 1966 excavations, the Field Director was Alan Olson. With his Southwestern background, none of the rock slabs appeared to him as part of a wall. The only wallstones were those that formed the platform near our projected easterly entrance. Contrary to Blakeslee, the local limestone could yield suitable slabs, so the lack of stone slab walling of any kind is significant. We are well aware, incidentally, that most Upper Republican lodges do not face due east. In our reconstruction, only House 5 has that orientation with respect to true North.

To reconstruct orientation, shape, and other features, we re-evaluated the original field notes, measurements, photos (official and private), plans, and artifacts (housed at DU and with the Wallace family) and revisited the Wallace and Hobson sites several times to check the reconstructions. The dashed and solid lines make clear what we consider secure versus educated estimates. Note that most dashed lines cross unexcavated areas. Our forthcoming geophysical survey should resolve the uncertainty about shape and orientation.

We have continued to build on the Upper Republican interpretation. Once we knew that the reconstructed pot from the rock art chamber was Apishapa, we began a project on interaction. Once we knew the value of different lithic sources, we re-examined the Hobson collection (DU Museum of Anthropology). As noted in the main reply, the Hobson ceramic assemblage contains Upper Republican pottery and hunting equipment (arrowheads and scrapers) made from Black Forest silicified wood (BFSW) which outcrops in the Palmer Divide not far to the north. It is significant that this combination characterizes several Barnes-type sites in the Pinyon Canyon area. The locations of these sites (open upland zone) together

with their content (scrapers and fractured arrowheads) make most sense as summer hunting camps. Barnes pottery, according to Krause (Lindsey and Krause 2007), is a local development from Upper Republican. This analytical conclusion means Upper Republican people lived somewhere in the general area. The BFSW points to the north. Thus Krause's various studies do not undermine our re-interpretation; quite the contrary—settlements such as Wallace and Hobson formed part of the residential base presupposed by the Barnes pottery.

Much criticism is leveled at our ethnohistoric approach. For Logan (Logan Supplement 3), our belief that lodge organization would be conservative is unjustified. Indeed, he offers the opposite as an alternative. Anthropological data, however, support our position. More specifically, Historic Pawnee considered the lodge (and various parts) as sacred space (Murie and Parks 1981) — like a church. The conservative nature of church architecture is too well known to require examples. In any case, it is the application of the lodge model that counts. As we noted, the lodge model makes sense of the House 1 complex.

What is more, the lack of physical evidence for altars in Medicine Creek lodges, for example, does not mean altars were not present. The 1940s excavations, after all, followed the traditional inductivist approach. With a different perspective, O'Brien (1986) recognized an altar at the Witt site, dating to the Smokey Hill phase.

Plains archaeologists have generally ignored O'Brien's work. This indicates that even if the geophysical survey completely supports our re-interpretation, many will continue to reject an ethnohistoric approach. The ramifications of such a rejection are profound. Native American worldviews were so different from those of present day Anglo-Americans that without recourse to some ethnography, archaeologists will impose their own values and beliefs. Roper's Supplement 5 is a case in point. To Roper, because more sites with bird

remains are now on record, the Witt assemblage is no longer unique. 'Uniqueness', however, is not based on numerical frequency, but indigenous attitudes. Most significantly, ethnographic sources do not point to the regular consumption of such birds as woodpeckers, owls, hawks, and eagles. Instead, Plains sources note the repeated use of these birds in ritual contexts.

To Native Americans, economic activities were imbued with religious meaning: economy was not a simple matter of subsistence. Although Roper acknowledges other uses of birds, she emphasizes subsistence, and as a result characterizes Upper Republican people as broad-spectrum hunter-gatherers. Moreover, because Historic Pawnee are not classified as a chiefdom-level organization, our use of terms such as 'chiefs' and 'priests' attributes an unwarranted degree of social complexity to Upper Republican society. Whatever classification system Roper has in mind, it will be based on empirical generalizations (e.g. Service 1963) rather than theoretical principles. This means the classification cannot be used to deduce conclusions about past societies. Instead, it is an empirical question whether Upper Republican people used such social categories as did Historic Pawnee. We believe O'Brien and ourselves demonstrate that they did: the social categories make sense of our respective data. This is not a tautology, as Roper mistakenly believes; the social categories do not have to make sense of these data.

Roper's reference to 'falsification' reflects a narrow stereotype of scientific methodology popular at the beginning of Processual Archaeology. 'Deductive consequences' and 'falsification', however, are problematic concepts in empirical sciences, such as archaeology, because other hypotheses are always hidden between the premise and conclusion (e.g. Laudan 1996). Rather than an emphasis on falsification, or the origins of hypotheses, scientific methodology is concerned with the assessment of competing hypotheses using ampliative criteria, such as the number of data covered, the diversity of data

and the potential to predict new data. Making sense of the data is a fundamental goal in science. Accusations of tautology are red herrings.

In this regard, the concept of a sacred bundle makes more sense of the bird data than does simple subsistence. The ubiquity of bundles throughout the Plains (Zedeño 2008) and their presence in the Cahokia area (Pauketat 2013: chapter 4, 167) at about the same time as the Wallace hamlet increase the probability that sacred bundles were part of the Upper Republican religious repertoire. Roper (Supplement 5), on the other hand, questions whether Upper Republican peoples used them because bundles cannot be reliably inferred from the present physical data. ‘Reliably inferred’, however, derives from an inductivist approach to archaeological interpretation. Bundles cannot be inferred from data; rather, the concept is applied to these data to see if it fits. A bundle model does make sense of the faunal cluster in the Witt’s lodge: one pit yielded the wings of a woodpecker, blue jay, and owl, as well as four quail wings, four un-modified mussel shells, a gar jaw and a turtle carapace (O’Brien 1986: 944). For other reasons (due east alignment, back altar, and semi-cardinal alignment of four center posts), the lodge can be identified with a priest. We agree with O’Brien that the combination of the faunal remains and physical features, in the light of the ethnography, demonstrates the antiquity of some Northern Caddoan cosmological concepts.

Our re-interpretation of the Wallace lodges, with reference to Northern Caddoan cosmology, formed a framework to interpret the rock art chamber below the bluff. We rejected an Apishapa affiliation of the art, not because of content first (contra Blakeslee Supplement 6) but because of our re-identification of the lodges: we fundamentally believe the two go together. The differences in content with Apishapa art (as opposed to technique) provided subsequent support. Engraved images, incidentally, could have been inscribed on the underlying sandstone. Such an engraving site, in fact, is on record a few kilometers upstream (5LA2224).

In conversation with various Plains researchers, we know many remain skeptical of our ritual interpretations of the middens. Researchers should remember that rock art was religious and its production was a ritual act: thus the chamber was ritual space. Furthermore, despite Blakeslee's comments regarding preservation, the middens were markedly different. Huffman and Ireland excavated three trenches, not two. Other than the omission of an arrowhead, our Supplemental Table (Huffman and Earley 2014) presents a true distribution of the artifacts. Trench 1 in the West Unit was almost entirely bone. Here we uncovered at least two thick, more-or-less horizontal levels by trowel. Had there been friable carbonized corn cobs, pottery, daub, or other such artifacts, we would have seen them before they crumbled. Their absence is therefore not due to poor preservation. There was a second trench (Trench 2) in the West Unit; this is the trench that yielded the few potsherds and manos noted by Blakeslee. Only Trench 3 in the East Unit yielded the huge amounts of carbonized corn cobs, pottery, lithics, and daub. This empirical distribution, at the least, represents a contrast between domestic (East Unit midden) and wild (West Unit Trench 1). We believe it also corresponds to the roles of priests/chiefs and doctors.

New interpretations are seldom accepted at first. We hope with time, critics will see the value of our re-interpretation and accept the presence of residential bases, such as Wallace and Hobson, in their understanding of High Plains Upper Republican society.

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