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Reply to Whitley

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WHITLEY suggests that I "misrepresent" both his research and the ethnographic record of California and the Great Basin. Yet Whitley's vigorous responses to my unpublished articles (cited in his comment) has, I feel, presented a rather misleading impression of my research. Whitley claims that my research continues a "hundred-year-long history of implicitly racist attitudes in American archaeology" (Whitley et al. 1999:17) and that I advocate "archaeology for academic Euro-Americans but not Native Americans" (Whitley 2000:31). Given the

central role that ethnography plays in my critical appraisal of Whitley's shamanic interpretation of California and Great Basin rock art, such conclusions are hard to maintain.

Instead, I disagree that Whitley's metaphoric re-analysis of the relevant ethnography demonstrates the visionary basis of rock art imagery in these regions. In my opinion, Whitley's approach pays insufficient attention to negative evidence and uses culturally specific information too broadly as an ethnographic analogy explicating the contexts of historic and prehistoric rock art production. This trait is exemplified by the way Whitley seems to believe that Monache and Yokuts ethnography can provide a template for understanding all California and Great Basin rock art.

I am not sure exactly what Whitley (p. 108) is implying by asserting that I distributed manuscript copies of an earlier version of this paper when it was presented at the 1998 annual meetings of the Society for American Archaeology (SAA). Manuscript copies of the paper were not distributed among the audience attending my session nor to any other individual at the meeting. In any case, the SAA advises participants at its annual meetings to prepare distribution copies for interested scholars, leaving me unclear what the point of his remark is.

Whitley (p. 108) construes my careful consideration of the relevant ethnography as an attempt to deter critical scrutiny of my arguments by providing "long lists of citations." Some might consider thorough referencing as taking account of all the available evidence. Further, Whitley frequently employs thorough referencing to support his own interpretations: a central proposition in one paper (Whitley 1994a:3) is accompanied by 38 citations. That same paper cites 171 references in 30 pages of discussion and notes, while his comment here cites 121 references.

Yet later, Whitley (p. 110) finds my referencing not sufficiently thorough as I supposedly ignore or attempt to "argue away . . . related additional evidence from throughout the far west that directly ties rock art to vision imagery." Bracketing the numer-

ous references to the Columbia Plateau,¹ the sources that I "neglect" are comprised of unpublished articles (including a letter to Whitley) and work published long after the submission of this paper. Of interest is a report elucidating the cultural context of a rock art site important to the Kaibab (Stoffle et al. 2000). Kaibab consultants reported that some white pictographs at this site were associated with the Ghost Dance. Other rock art imagery present was reported to have recorded past events and history (Stoffle et al. 2000:20). The authors argued that this research provides "scientific demonstration of the association between rock art and shamanism," because oral accounts connect the art to the Ghost Dance (Stoffle et al. 2000:21). I shall let others interpret whether this represents "related additional evidence" linking rock art directly to visionary imagery. However, it does imply a neglected narrative function for some rock art.

Is an approach that argues for "a unitary explanation for the rock art of the Far West" (Whitley 1996:28) insensitive to temporal and regional variations (p. 119)? Far western rock art is viewed as reflecting a "widely shared" concern to "portray visionary imagery" irrespective of whether it was made by shamans or puberty initiates (p. 117). Variation in symbolic themes and ideological functions is granted, but these revolve around shamanic themes (*passim*). Although derived from re-analysis of historic ethnographies, Whitley (1996:30) argued that this interpretation is "our best hypothesis" for much, if not all, prehistoric rock art, too. For example, despite identifying a tradition of making rock art at the Cosos for at least 9,000 years,

... we have every reason to assume that even the earliest Coso petroglyphs were shamanistic in origin and that they portrayed visions of the supernatural. (However, it seems unlikely that these early shamans were primarily concerned with weather control.) A shamanistic origin for the art is supported by the earliest dates that we have obtained so far on the Coso petroglyphs. Just like the more recent art and exactly as implied by the ethnographic accounts, our Late Pleistocene dates include a mix of entoptic and representational motifs [Whitley 1998:162].

It is the strenuous rebuttal of alternative interpretations, rather than just the very narrow interpretive focus, that makes Whitley's approach potentially monolithic. For example, Whitley concedes that some rock art sites may have been related to astronomical and cosmological beliefs because "... shamans did maintain sophisticated and detailed astronomical knowledge" (Whitley 1996:192). However, ultimately, archaeoastronomy is found wanting because of its implausibility, its lack of any direct ethnographic support, and its Eurocentrism (Whitley 1996:192). Furthermore, "little understanding is to be gained by turning Native Americans into junior scientists and fitting them into our own twentieth-century mold" (Whitley 1998:137).

A mythological function for Great Basin rock art and much of south-central California is also rejected as a possible explanation, since in trance states Great Basin shamans only encountered spirit-helpers. Shamans therefore could not have depicted mythic beings in their art. Thus, rock art only portrays spirit-helpers and other visionary imagery, establishing the shamanic context of its production (e.g., Whitley 1994a:6-7, 1998:163). Obviously, this argument is predicated on the assumption that shamans exclusively made Great Basin rock art. Therefore, it is possible for other researchers to find Whitley's narrow insistence on a shamanic context for rock art production and the rejection of alternative interpretations to be monolithic. Whitley could perhaps avoid this implication if apparent exceptions to his shamanic interpretation were not revealed as having latent associations with shamanism.

Whitley's reliance on metaphoric re-analysis of California and Great Basin ethnography is a tacit recognition that direct consultant statements asserting that rock was made to portray visionary imagery are rare and restricted to the Monache. However, Whitley's mode of metaphoric analysis has little to recommend itself if it is as flimsy as his discussion of Great Basin doctor rocks (p. 116). Whitley implies that terms such as "medicine rock," "doctor's rock" or "shaman's rock" were widely

used as generic names for rock art sites. This is inaccurate; instead, many referred to certain rocks with petroglyphs and/or pictographs which "seem to have been used more to cure illness and to grant favors rather than for a person to receive doctoring power" (Fowler 1992:178). Currently, four doctor rock localities are known from the western Great Basin (Woody 2000). Wuzzie George attributed the petroglyphs on the famous Schurz doctor rock to Coyote (Wheat 1967:115) when, according to Whitley's reasoning, it would have been more appropriate to attribute it to Water Baby had she wished to metaphorically assert a shamanic context.

Whitley claims that I imply "that massive acculturation has rendered our ethnographic record effectively valueless" (p. 118). What I actually noted was the observation of other researchers that the salvage ethnography conducted during the first half of the twentieth century cannot be assumed to represent an accurate reconstruction of precontact cultural practices (e.g., Fowler et al. 1999:54). On a number of occasions, Whitley (e.g., 1994b:364, 1998:133) has highlighted the deficiencies of early ethnographic investigations of Great Basin religion and shamanism:

But in that much of this [ethnography] was collected under the guise of Julian Steward's (e.g., 1955) cultural ecology, the importance of art, symbolism and ritual are diminished. Using the existing ethnography to define the social context of the art necessarily, therefore, requires a re-analysis of it [Whitley 1994b:364].

It is for these reasons that I find Whitley's approach inconsistent and unconvincing. There seems little room for alternative interpretations and insufficient appreciation of the diversity of both rock art and cultural traditions in California and the Great Basin. I am left to wonder how, using Whitley's approach, archaeologists could ever discover something about rock art not already theorized in his metaphoric re-analysis of ethnography.

NOTE

1. I use the term "far western North America" in the same restrictive sense as Whitley (e.g., 1992:89,

Fig. 1) as a shorthand for California and the Great Basin, where his research is focused. The Columbia Plateau provides the most solid ethnographic support for the shamanic model proposed to date. However, I am not sure that ethnography from this region should be used as an analogy for the contexts of California and Great Basin rock production, either historically or in prehistory.

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