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Response to Quinlan

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I am pleased that Angus Quinlan's article, *The Ventriloquist's Dummy: A Critical Review of Shamanism and Rock Art in Far Western North America*, is finally in print. I first received a manuscript copy of it within a few days of its original presentation at the 1998 Society for American Archaeology meetings, from a colleague who had attended Quin-

lan's session where it was distributed to the audience. Shortly thereafter, I began to hear about it from archaeologists stretching from Berkeley to France who had received unsolicited copies in the mail, with no explanation attached. A number of them called to ask, "What is going on? This guy Quinlan says that you got it all wrong." When I received a review copy of Quinlan's manuscript a year later from the *Journal of California and Great Basin Anthropology*, I quickly replied. Publish it as-is, I responded, but allow me to comment in detail, especially inasmuch as citations to his manuscript had already begun to appear in other researchers' papers, thereby lending implicit legitimacy to his claims, if not implying outright acceptance (e.g., Gilreath 1999; Chippindale et al. 2000). It is a pleasure to finally have an opportunity for a formal response.

To start, it is necessary to clarify the nature and purpose of Quinlan's argument. It operates on the principle that, because science is necessarily conservative, most readers will award more weight to criticism than to original research and, thus, commentaries such as his can get away with implausible claims and misrepresentations of the evidence, because they themselves are rarely scrutinized. Such a position assumes that if long lists of citations are included, no one will bother to check the assertions it makes. And although Quinlan claims that his article is an assessment of the shamanistic rock art interpretation, in fact it is anything but. It is instead simply a protracted and confused effort to disagree with anything and everything I have published, and then some: Quinlan wanders from a discussion of neuropsychology (pp. 92-93), to ethnographic evidence for shamanism (passim), to shamans' sex (p. 102), to Euroamerican acculturation (pp. 96, 101), to Carobeth and George Laird's relationship (p. 100), and so on—leaving me to wonder whether Quinlan would have objected if I had claimed somewhere in one of my papers that the earth revolves around the sun!

Quinlan's article plays on the fact that the ethnographic record contains occasional inconsis-

tencies, because informants (and anthropologists) sometimes disagree. This is no news to anyone who has ever worked with ethnohistorical data or, for that matter, with empirical data of any kind, and there is sometimes room for debate in such cases. There is no room for debate, however, when basic facts and sources—those in the ethnographic record and the statements in my research—are systematically and blatantly misrepresented. Nor can meaningful debate occur when alternative positions are promoted that are so manifestly implausible that they leave the reader wondering whether his point is actually some kind of obscure or perverse joke.

An example of just such a proposition is provided by Quinlan's (p. 96) assertion that:

... references to Joaquin Ayala (a shaman) and Rafael Solares making rock art . . . cannot be used to conclude that all (or even any) Chumash pre-Columbian rock art was shamanistic in character . . . *Solares' shamanistic use of rock art may have more to do with the religious significance of graphic imagery in Roman Catholicism* (emphasis added).

Does Quinlan expect us to believe that his "Catholicism hypothesis" is somehow more likely, factually better supported, and therefore a "more direct, alternative reading" (p. 95) than the shamanistic interpretation? Yet on careful analysis, it is this level of empirical critique, and this kind of inferential logic, that is the entire basis for Quinlan's attempt to discredit my research.

In the following, available space allows me to focus on only a few of the key issues, taken on a regional and topical basis, rather than a point-by-point tabulation and correction of Quinlan's systematic errors. But once Quinlan's central factual misrepresentations and mistakes are outlined—and the nature of his commentary is unmasked—it becomes apparent that shamanism remains the most robust ethnographic interpretation for recent rock art.

KEY FACTUAL MISREPRESENTATIONS I: SOUTH-CENTRAL CALIFORNIA

The central issue in this debate is whether there is ethnographic evidence that shamans created rock

art to depict the visionary imagery of their trances. Quinlan (p. 98) asserts unequivocally that:

Whitley (1994b:83-84, 1994c:3) has only been able to uncover two Monache consultant statements that provide direct evidence that shamans depicted their visionary imagery in rock art (Gifford 1932:52; Driver 1937:126). While these are of relevance to the Monache, they do not apply to the rest of California or the Great Basin.

In this short passage alone, Quinlan's first factual misrepresentation is revealed when it is contrasted with a verbatim passage from Driver (1937: 126) that Quinlan himself cites:

[Monache shamans] painted their "spirits" (anit) on rocks to "show themselves, to let people see what they had done." The spirit must come first in a dream. The informant said he was certain that the Yokuts from the Tule r. north and the other W Mono had the same belief and practice.

This statement unambiguously contradicts Quinlan's claim. Driver stated clearly that the shamanic depiction of visionary imagery was not restricted to the Western Mono, but was also practiced by Yokuts shamans at least from the Tule River north.

Corroboration comes from Julian Steward who, based on Anna H. Gayton's research, wrote: "In Tulare county some of the strange pictographs are believed by the Western Mono and Yokuts, according to Dr. Gayton, to be 'doctors' marks. These may represent shamans' powers although it is unlikely that all were for this purpose" (Steward 1929:226). "Shamans powers," of course, were obtained in the visions that they experienced in trance.¹

How significant is Quinlan's first factual misrepresentation? Cook (1978:91) estimated that the southern Sierra Nevada/San Joaquin Valley region contained about 27% of the total aboriginal Native Californian population, with the Yokuts alone constituting the majority of this figure. Baumhoff (1963) placed their proportion even higher. But by whatever demographic measure, Quinlan's misrepresentation of the Yokuts evidence results in the omission of at least 15% of Native Californian peoples.

Quinlan's second factual misrepresentation—again, just in this first quoted passage—is his assertion that direct evidence for shamanic depictions of visionary imagery is limited to two Western Mono sources that he cites. The Steward reference above is the first example of an omitted source that contradicts Quinlan's claim. The second omission stems from the fact that, in one of the two papers of mine that Quinlan cites in this passage (Whitley 1994b), I identified another Yokuts consultant who stated that rock art was made by shamans, and that it depicts shamans' "dreams"—the gloss used throughout the far west to indicate trances (see Whitley 1998c). I even included photographs of panels that relate to the consultant's commentary.²

In this one example, involving the central issue in the debate, Quinlan has misrepresented what the published ethnographic record states in the clearest and most unambiguous terms, as well as the evidence that I have presented about this record. The result is the omission of evidence concerning a major portion of Native Californian populations.

Quinlan also fails to mention, or attempts to argue away, related additional evidence from throughout the far west that directly ties rock art to visionary imagery—in some cases, that of the shaman himself; in others, puberty initiates or adults during life crises (Whitley 1992, 1994b, 1998c, 2000a, 2000b; Keyser and Whitley 2000). For example, for the Numic (discussed further below), Lowie (1909), Phillips (1986), Hultkrantz (1987), Shimkin (1992), Zedeño et al. (1999), Loendorf and Bullets (2000), and Stoffle et al. (2000) all indicate that rock art depicts shamans' visionary imagery. On the Columbia Plateau, statements tying rock art to visionary imagery have been recorded for the Lillooet (Teit 1906), Nez Percé (Spinden 1908), Shuswap (Teit 1909), Kutenai and Interior Salish (Teit 1918), Coeur d'Alene (Teit 1930), Okanagan (Teit 1930; Lerman 1954; York et al. 1993), and Modoc/Pit River (Benson 1998), which Quinlan omits entirely.

Ethnography and Southern Sierra Nevada Pictographs

That Quinlan makes his argument based on factual misrepresentations of major issues is one matter (see more below). Another matter arises from the way Quinlan has structured his article, which has been to decontextualize, isolate, and contest every issue, bit of evidence, and/or ethnographic statement in support of the shamanistic interpretation, meanwhile never stopping to consider the sum of it all. The result, in some cases, is a false picture of anecdotal and isolated fragments of data; in others, it is logical incoherency. But since scientific research is based on inference to the best hypothesis, using multiple lines of evidence (Kelley and Hanen 1988), I prefer to consider the evidence as a whole. This is particularly necessary in ethnohistorical research where any given source is liable to provide only partial information. To illustrate just one regional example, what then is the ethnographic evidence in support of a shamanistic origin for the recent rock art in the southern Sierra Nevada?

This evidence, summarized in Table 1, demonstrates that: (1) the connection between southern Sierra Nevada pictographs, shamans, and shamanic rituals has been regularly documented for a century and a half; (2) there is consistency in these comments about the origin and function of rock art; (3) the art depicts visionary imagery; and (4) a variety of lines of subsidiary evidence supports this last conclusion, especially independent comments about the depiction of supernatural beings in the art—things seen by shamans during trance. Harrington (1984) and Latta (1977), for example, identified rock art motifs as supernatural beings, and noted the depiction of shamans and their regalia in the motifs. In light of the sum of this evidence, it is quite plausible to interpret historical southern Sierra Nevada rock art sites as having been made and used by shamans; that they depict the visionary imagery of their trances; that they were undertaken for a variety of purposes, including acquisition of

Table 1
ETHNOGRAPHIC DATA ON SOUTHERN SIERRA NEVADA PICTOGRAPHS

Reference	Ethnographic Information
Eccleston 1851	A rock art site called "Great Medicine Rock" was held "in great reverence;" cures were conducted at the site; offerings of small stones were left in front of the site (Crampton 1957:65).
Harrington 1984 [ca. 1914 data]	A rock art site called <i>pusun tinliw</i> (literally "spirit helper cave") includes a painting that depicts a shaman in ritual dress.
Steward 1929:226	"In Tulare county some of the strange pictographs are believed by the Western Mono and Yokuts, according to Dr. Gayton, to be 'doctors' marks. These may represent shamans' powers although it is unlikely that all were for this purpose."
Gayton 1930:391 [1920s data]	Shamans' caches were: "some knoll which was shamans' customary meeting place"; shamans met there with their <i>taiwan</i> (ritual trays), plotted the deaths of other shamans, played eagle-bone whistles and ran around fires, and got magical "shot" from sun.
Gayton 1930:393 [1920s data]	Shaman Wasic provided this description of his cache: "I have lots of money and baskets up at Tawatsanahahi in a big rock like a house; there is a lot of poison there, too."
Gayton 1948a:33 [1920s data]	"As in the foothills, doctors were believed to have private caches where the wealth they accumulated was hidden: such a cache is called <i>tai'wan</i> . . . One of these is located in a little ridge about eight hundred yards north of the Lemoore Rancheria . . . "A <i>taiwan</i> is always guarded by some sort of creature. This one [near Lemoore Rancheria] is inhabited by personified fire."
Gayton 1948a:34 [1920s data]	"This <i>taiwan</i> is an old one; nobody knows what doctor owned it. <i>Poso'o</i> [Tachi shaman Bob Bautista] told J.A. about it and warned her to keep away from it. He said a big sickness (<i>tau'mui</i>) like consumption or pneumonia would 'come out' if any one attempted to disturb the place."
Gayton 1948a:113 [1920s data]	"The [shaman's] cache would be in a cliff or rock pile; cracks indicated the door, which opened at the owner's command. The rocks were usually painted; in fact, any rock with pictographs was thought to be a cache."
Gayton 1948a:113 [1920s data]	Shamans' caches were called <i>pa'čki</i> [color 'red'], where wealth and talismans were kept. One cache is in Drum Valley; another at Hoganu [both are rock art sites]. A large cache near Gutnumi (Terminus Beach) was called <i>Čoiš,šiu</i> , and was "marked with pictographs" [this site is Bell Bluff, CA-TUL-2; name confirmed by Latta 1977:185].
Gayton 1948a:113 [1920s data]	"S.G. said there were several inner chambers 'each as big as a house' and that they were filled with native treasures . . . S.G. expressed apprehension lest the hill might some day be dynamited, for if so, 'all kinds of bad diseases would fly out over the country.'"
Gayton 1948a:113 [1920s data]	"M.P. had a frightening experience there . . . The rocks of the bluff are painted with pictographs, she said, but she could not accompany me there."
Gayton 1948a:113 [1920s data]	Uncle of M.P. warned her of a "beautiful weird dog there which came out of the rocks to attack investigators . . . the dog which guarded the cache had a snake's body and human hands for feet." M.P. went there and saw a ghost that "look[ed] like fire."
Gayton 1948a:113 [1920s data]	Common term for shaman's cache is <i>Čoiš,šu</i> , "dog place" [i.e., "spirit helper place"]; but "correct name is <i>pački</i> " [confirmed by Latta 1977:600].

Table 1 (continued)
ETHNOGRAPHIC DATA ON SOUTHERN SIERRA NEVADA PICTOGRAPHS

Reference	Ethnographic Information
Gayton 1948b:168-169 [1920s data]	Shamanism “ran in families,” and the son would usually inherit his father’s wealth and cache (pa’čki). If there was no son, the pa’čki was “lost forever.” The pački “was always located on a hill, with a rock as a door. There was no mark of ownership. After the shaman’s son had received his power, he could enter the pački at will.” The son danced outside and called the name of his father/grandfather, and it opened. “Within it were baskets, money, feathered ornaments, skins—all valuable things.” The son could take what he wanted from the left side. Things on the right belonged to the most ancient ancestors and were too powerful. “They served as guardians of the place, for their power would kill anyone who touched them.”
Gayton 1948b:168-169 [1920s data]	The power seeker awoke after dreaming and went into the hills “to dream some more.” When the man had dreamed sufficiently, he went to a cache where his spirit helpers opened the door, and stayed inside four days. No one knew where he was while he was in the pački. “The animals [i.e., spirits] told him what to do,” gave him songs, danced with him, and gave him regalia. He remained in the hills for two more days (a total of six), went home, then did his debut dance, where he cured a patient.
Gayton 1948b:207 [1920s data]	According to one informant, only rain shamans had shaman’s caches. A rain shaman “kept his outfit within a large rock,” which the shaman could open but otherwise could not be seen. “Each doctor had his own place.”
Gifford 1932:51	A rock art site in Hooker Cove was made by a shaman known as Sigurup.
Gifford 1932:52	A pictograph four miles east of Fuller’s Meadow depicted a man’s dream.
Driver 1937:86	Eight consultants reported that pictographs were made by recent humans; six consultants reported that pictographs were made by shamans; other consultants denied that rock art was made by shamans or “anybody.”
Driver 1937:126	Description of the making of pictographs: “Specifically [made by] doctors, po’hage. They painted their ‘spirits’ (anit) on rocks to ‘show themselves, to let people see what they had done.’ The spirit must come first in a dream. The informant said he was certain that the Yokuts from Tule r. north and the other W Mono had the same belief and practice.”
Aginsky 1943:426	Two consultants reported that pictographs were made by shamans; two consultants denied it.
Latta 1977:199 [1930s data]	A CA-TUL-19 pictograph depicts the painting of <i>soksouh</i> , a supernatural spirit.
Latta 1977:600 [1930s data]	Rock art sites have been located at permanent villages or at locations of ceremonies. “Tribal equipment” (jimsonweed mortars and ceremonial costumes) was concealed nearby; sites are tripne, i.e., supernaturally potent.
Latta 1977:601 [1930s data]	Informants identified a shaman’s ritual tray and “mythological characters” in the rock art.
Whitley 1994b, 1996, 2000a	Information from a modern Wukchumni consultant indicates that pictographs are “doctors’ marks”; a rock art panel was described as a “shaman’s dream”; some motifs portray Blue Heron (shaman) dancers, and some sites are still used for nonshamanic curing rituals.

power, curing, and sorcery; and that sites were also used by nonshamans for other ritual purposes, especially curing and praying.

Certainly there are informants who, in the ethnographic record, denied that rock art was made by shamans, but these same consultants also denied that rock art was "made by anybody" (e.g., Driver 1937:86). Quinlan knows about this contradictory rock art evidence and can cite this literature precisely because, in my publications, I have been diligent to include references and citations to all of the ethnographic evidence about rock art. But more to the point, does a consultant's denial indicate that this rock art is necessarily prehistoric and predates ethnographic southern Sierra Nevada culture, or does it instead suggest that these consultants simply were not comfortable discussing rock art with an anthropologist, especially when the sites are associated with major historical villages (e.g., Latta 1977:600) and substantial additional rock art ethnography exists (e.g., Gayton 1948a, 1948b)?

Anyone who has used Culture Element Distribution (CED) studies knows that occasional contradictory data of this kind are common. In Driver (1937), for example, 30% of his Yokuts informants denied the use of bedrock mortars. Are we then to conclude, following Quinlan's logic, that the Yokuts did not use bedrock mortars? Obviously, intelligent, critical use of such data would suggest otherwise.

I encourage readers to examine the data in Table 1 in light of Quinlan's (p. 100) claim that "in general, a shamanistic context for the production and functions of rock art is denied." For those interested, I suggest that they check the original sources referenced in Table 1. I also encourage readers to consider the possibility that the general pattern suggested in Table 1 might likewise apply to the adjacent Chumash, about whom we have fewer data, inasmuch as Gayton (1935), Applegate (1978), and others have identified cultural similarities between them, the Yokuts, and other groups in south-central California. I further urge readers to decide whether a shamanistic origin for ethnographic Chumash rock art, along general southern

Sierra Nevada lines, is a plausible interpretation or, instead, whether the evidence supports the theory that "Solares' shamanistic use of rock art may have more to do with the religious significance of graphic imagery in Roman Catholicism," which Quinlan (p. 96) proposes as a more direct reading of the ethnographic evidence.

KEY FACTUAL MISREPRESENTATIONS II: GREAT BASIN

Substantial factual errors and misrepresentations also occur in Quinlan's discussion of the Great Basin evidence, where his objections to shamanism are based primarily on two issues. The first involves the dismissal of Carobeth Laird's (1984:302) argument that consultants speaking Uto-Aztecan languages claimed that rock art was made by various spirits rather than by shamans as a way of avoiding a taboo against naming these dead individuals (p. 100). With Laird, I have argued that these spirits were shamans' helpers and, following Forde (1931), Gayton (1948a, 1948b), Applegate (1978), Siskin (1983), Hultkrantz (1987), and others, have stated that the actions of a shaman and his helpers were indistinguishable. Thus, rock art said to be made by a spirit was simply a metaphoric way of claiming that the art was made by a shaman (see also Loendorf 1999).

Quinlan's argument here is based primarily on a single and relatively insignificant issue: ethnographic statements from a few consultants who attributed the origin of rock art to Coyote. Quinlan (p. 99) states that "Since Coyote did not serve as a shaman's spirit-helper among the Northern Paiute (Park 1938:19), such statements cannot be construed as a metaphor that malicious shamans made rock art." Further, because Coyote was a mythic actor, Quinlan then concludes that Great Basin rock art is necessarily ancient rather than potentially recent in age and beyond the reach of ethnographic information and interpretation. Hence, he claims that, "The few metaphoric statements that supernatural entities made rock art . . . seem to assert the timeless-

ness of the rock art—it had always been there, made in mythological time” (p. 102).

There are numerous problems with this argument, some involving factual errors and misrepresentations while others are more conceptual in nature. For example, substantial ethnographic data indicate that Coyote could be a spirit-helper, contrary to the single source that Quinlan cites. This was documented by Gifford (1932), Voegelin (1938), Harris (1940), Steward (1941), Gayton (1948a, 1948b), Applegate (1978), Smith (1978), Harrington (1984), Hultkrantz (1987), and Fowler (1992), as well as elsewhere in Park (1938), proving that there is occasional contradictory data, even in a single source. Moreover, Steward (1941), Smith (1978), Laird (1984), and Zigmond (1986) even identified the particular shamanistic powers that Coyote imparted—sorcery or bewitching power—while Harris (1940:60) stated that “One who had Coyote power could break the common tabus since he, like Coyote, was recognized as a licentious lawless being.”

But Coyote’s shamanic function is a minor side issue.³ Much more importantly, most Uto-Aztecan consultants stated that rock art was made not by Coyote, but by other kinds of spirits (e.g., Lowie 1924; Kelly 1932; Driver 1937; Voegelin 1938; Stewart 1941, 1942; Steward 1943b; Laird 1976, 1984; Zigmond 1977, 1986; Brooks et al. 1979; Irwin 1980; Sutton 1982; Hultkrantz 1987). Water (or Rock) Baby is the most commonly cited of these, and this spirit was widely identified as a shaman’s helper (e.g., Chalfant 1933; Park 1938; Steward 1941; Whiting 1950; Downs 1961; Miller 1983; Siskin 1983). Fowler (1992:180), for example, stated that “water baby was considered to be essential to effective doctoring.” Despite Quinlan’s (p. 99) attempt to phrase this problem in terms of the minor question of whether Coyote was or was not a spirit-helper, most of the references to rock art made by spirits still pertain to those that are directly known to have served as shamans’ helpers.

Moreover, some rock art is clearly historical in age and cannot be “mythic” in origin. Historical

motifs, for example, have been recorded at a variety of sites (e.g., Benton 1976; Brook et al. 1977; Garfinkel 1978; Bard 1979; Whitley 1982; also see Schiffman et al. 1982). Equally importantly, numerous consultants claimed that rock art continued to be made into recent times (Gifford 1932; Chalfant 1933; Steward 1933; Driver 1937; Stewart 1942; Aginsky 1943; Malouf 1974; Riddell 1978; Brooks et al. 1979; Phillips 1986; Cole 1990; Zedeño et al. 1999; Loendorf and Bullets 2000; Stoffle et al. 2000).

An additional problem with Quinlan’s claim that rock art is mythic in age is conceptual and results from a pernicious form of Eurocentrism. Anthropologist Robert Layton has addressed this problem with respect to Australian aboriginal ethnography and rock art. Attributions of rock art to mythic or religious spirits “must be understood within an Aboriginal ontology in which people are, or can become, the incarnation of ancestral heroes” (Layton 1992:13), paralleling my argument concerning the nature of the relationship of a shaman and his spirit-helpers. Furthermore, Layton (1992: 13) discredited the false “archaeological inference . . . that the less Aboriginal people have to say about a body of rock art, the further removed in time its execution must lie.”

It is thus not surprising that the origin of rock art was attributed to spirits in various parts of far western North America, not just in the Great Basin. Consider, for example, the following description of the making of pictographs at The Dalles on the Columbia River:

One night a medicine man of the Wishram mixed some paint made from roots. Then an unseen power guided his hand and his brush across the stone . . . The man could not see what he was painting. He worked hard, so that his work would be done before morning came . . . Later he was found, in a trance, at the foot of a rock. A ghostly eye was looking down at him and on the people who came to him. All knew that *Tahmahnawis* [powers, spirits] had painted it [Ranck 1926:1].

As this statement makes quite clear, the idea that both a shaman (in a visionary trance) and a spirit

could be involved in the making of rock art was not a contradiction in Native American terms.

Finally, proof that Numic peoples did not consider rock art to pertain to the mythological past is found in their ethnography. Consider, for example, Hultkrantz's (1987:49) observation that: "Some Shoshoni who approach the rock drawing places in winter time may hear the spirits working them." Similarly, Irwin (1980:32) noted that: "Indians hear pounding on rocks as spirits make fresh petroglyphs; the rock writings are continuously being made." And Fowler et al. (1995:55) recorded that:

Maturango Peak [Argus Range, part of the Coso petroglyph locality] is a very spiritual place, a source of supernatural power . . . It is also a place where petroglyphs are known to occur, with new ones added by spiritual forces all the time.

As these statements make abundantly clear, Native American consultants themselves did not believe that rock art was timeless, as "it had always been there, made in mythological time" (p. 102). They instead explicitly and clearly identified it as an ongoing phenomenon, which was to them no contradiction to their idea that it resulted from spiritual forces.

Numic Vision Questing

Quinlan's second main contention about the shamanistic interpretation of Numic rock art seems to be the assertion that Numic shamans did not normally conduct vision quests (during which, I have inferred, they made rock art). Quinlan (p. 95) states that, "The purposeful pursuit of shamanistic powers ("vision questing") appears to have been largely unknown in the Great Basin and not universal in California (Park 1938:110, 113, 119)." Later, Quinlan (p. 99) contradicts himself:

In the Great Basin during the nineteenth century, the Northern Shoshoni (Lowie (1909:223-224, 1924:295-296; Voget 1984:302), Eastern Shoshoni (Shimkin 1953:409; Trenholm and Carley 1964:40; Hultkrantz 1987:52-54), and Gosiute (Malouf 1974:81) acquired shamanistic powers by sleeping overnight at a rock art locale.

How is it that Quinlan can assert at one point that vision questing was "largely unknown" in the Great Basin when, subsequently, he cites seven Numic references documenting its practice *at rock art sites*? Moreover, Quinlan admits only part of the evidence demonstrating that Great Basin vision questing was a widespread shamanic practice (e.g., see Kelly 1936; Steward 1938, 1941, 1943a; Harris 1940; Laird 1974; Riddell 1978; Hultkrantz 1961, 1986a; Fowler and Liljebblad 1986; Kelly and Fowler 1986; Liljebblad 1986; Fowler 1992; Shimkin 1992; Loendorf and Bullets 2000).

This internal contradiction is bad enough. More disturbing is Quinlan's use of citations from Park's (1938) monograph as support for his claim that Basin vision questing was "largely unknown." This is what Park actually said:

In addition to those who receive shamanistic power from involuntary dreams, there are individuals whose desire for power is sufficiently strong to cause them to undertake a voluntary quest. There are certain places, such as caves in mountains, where power can be acquired. Eight or ten such places are known today, and perhaps formerly there were still others which are now forgotten [Park 1938:26].

The Paviotso quest is quite simple. The person seeking shamanic power, usually a man, stays in a cave for a single night [Park 1938:110].

[Basin vision questing] has been reported for the Surprise Valley Paiute, the Lehmi, Wind River, Seed Eater, Salmon Eater, and White Knives bands of Shoshoni, at least for the Chemehuevi band of the Southern Paiute, with a strong suggestion that the Northern Ute also seek powers in mountains [Park 1938:118].

Nowhere in this did Park (1938) remotely imply that the vision quest was "largely unknown," as Quinlan falsely implies. Park stated just the opposite, even identifying specific Northern Paiute, Shoshone, and Southern Paiute bands where the practice had been documented. This is confirmed by Hultkrantz (1986a:54), who stated that:

The dream and vision-quest complex is clearly the most diffused and intense religious pattern in aboriginal North America . . . it is this pattern which

primarily inspires American Indian identifications of rock drawings.

It is then not surprising that I have been able to document over two dozen ethnographically described vision quest sites, along with an even greater number of verbal accounts of visionary hallucinations (Whitley 2000a, 2000c), despite Quinlan's (p. 98) additional claim that "dream experiences were often primarily *auditory* experiences" (emphasis in original).⁴

Furthermore, there are substantial additional data tying Numic rock art to shamanism that Quinlan has omitted in his effort to focus on the question of Coyote's place as a spirit-helper. Consider, for example:

1. The creation of Numic rock art by shamans directly documented by Riddell (1978), Brooks et al. (1979), Phillips (1986), and Cole (1990).

2. The depiction of visionary imagery in the art noted by Lowie (1909), Phillips (1986), Hultkrantz (1987), Shimkin (1992), Zedeño et al. (1999), and Stoffle et al. (2000). Note here that, citing Hultkrantz (1987) as an authority, Quinlan (p. 99) claims that the Eastern Shoshone "generally denied that shamans made rock art." This is a misleading semantical ruse based on the Shoshone attribution of the art to spirits. What Hultkrantz (1987:50) actually said, in a long discussion of shamanic vision questing and rock art, can be summarized by his caption for a photo of a Dinwoody petroglyph: "Spiritual power radiates like electricity from *pandzoavits* (depicted here in a rock drawing), the dangerous, mysterious ogre and visionary spirit of the Shoshoni."

3. The use of the term *pohaghani*, "house of supernatural power," as the Numic term for "rock art site," according to Malouf (1974), Shimkin (1992), and Loendorf and Bullets (2000), thus paralleling *pohagunt*, "[man] having supernatural power," for "shaman."

4. More generally, the terms "medicine rock," "doctor's rock," or "shaman's rock" used as generic names for rock art sites by consultants of Steward (1943a), Heizer and Baumhoff (1962), Wheat

(1967), Grosscup (1974), Liljeblad (1986), Hultkrantz (1987), and Fowler (1992). If Numic rock art was not shamanic but instead "timeless" and "mythological," as Quinlan asserts, then why did Numic peoples widely refer to the sites as "doctor's rocks?"

KEY FACTUAL MISREPRESENTATIONS III: MY RESEARCH

Perhaps the worst example of Quinlan's systematic factual misrepresentations involves the way he characterizes my interpretations and conclusions—because an honest critique necessarily must be accurate in its portrayal of the subject that it purports to assess. Consider then the following statements he uses to portray my interpretations:

One could rightly question whether a single explanation of all California and Great Basin rock art seems plausible . . . The basis of all historic and prehistoric rock art production is argued to have been a "vision quest," with rock art made by shamans after trance states to depict their vision imagery [p. 95].

. . . the ethnographies of far western North America provide scant support for a shamanistic interpretation of all prehistoric and historic rock art in this area [p. 102].

The implication of this assertion is clear. According to Quinlan, in my publications I have argued for a "single explanation of all California and Great Basin rock art." Citing one ethnographic example which counters my putative claim for a "single explanation," Quinlan (p. 96) points out that: "In central California, certain forms of rock art were associated with fertility rites." The implication here is also straightforward. Since the totality of my interpretation solely concerns shamanistic vision questing, I have overlooked the well-documented evidence concerning pit and groove petroglyphs—and this omission on my part proves my general contention false.

Let us look at what I have actually said, in contrast to the way that Quinlan has misleadingly portrayed my research:⁵

Although we lack ethnographic evidence for the making of cupules in southern California . . . Our best hypothesis . . . is that they were made by non-shamans in what can best be termed "folk uses" of rock art sites [Whitley 1996:95-96].

[I]t is nonetheless true that a handful of empirical observations that have been made at a few sites during the solstice, combined with the fact that Native American shamans did maintain sophisticated and detailed astronomical knowledge . . . suggest a relationship between some of the art and Native cosmological and astronomical beliefs [Whitley 1996:192].

[W]e are uncertain as to what cultural group made these unusual motifs and why they may have been produced [Whitley 1996:193].

The maze-style sites, like the Hemet maze, were formal, open public displays, differing greatly from the secretive, idiosyncratic art of the recent shamans, and even the hurried and repetitive art of the ritual initiates . . . emphasizing that some aspects of far-western North American rock art are not, and may never be, unraveled [Whitley 1996: 195].

Although not rock art in the sense used here, cupules were made during rituals, and they reflect some of the same general symbolism and beliefs as the petroglyphs, even though they weren't made by shamans and do not depict visionary images [Whitley 1998a:24].

Much of the rock art of Native California was associated with shamans and their altered states of consciousness experiences. Yet this was not invariably the case, as the Earth Figure Tradition illustrates. Considerable ethnographic information exists concerning one of the two variants of this tradition: the geoglyphs found along the Colorado River. These geoglyphs were used in public rituals that, while led by shamans, were concerned with reenacting the mythic creation of the world [Whitley 2000a:94].

Pit and Groove Tradition art neither was made by shamans nor involved altered states of consciousness and visionary imagery [Whitley 2000a:98].

Even Quinlan's tacit characterization of my shamanistic interpretation of pictographs and petroglyphs, *sensu strictu*, as arguing for a single function and purpose, along with his claim that I have ignored regional variation, are misleading. I have instead pointed out that:

[A]mong different cultural groups this art was made by different social groups and for different

reasons, even though the intent to portray visionary imagery was widely shared. In southwestern California and along the Colorado River . . . the ethnographic record indicates that rock art was made both by shamans and by puberty initiates; on the Modoc Plateau it was made by these two social groups and by adults during life-crisis rituals; whereas in the intervening areas of south-central California and the Great Basin it appears to have been the exclusive work of shamans . . . the ethnography indicates that the purpose behind a shaman's entering a trance and subsequently portraying visionary imagery could also vary . . . The depiction of visionary imagery, in other words, is the unifying characteristic of Native Californian rock art, even though the creators of this imagery, its specific meaning, and its intended symbolic and ideological functions varied considerably [Whitley 1998c:24-25].

Additional misrepresentations occur with Quinlan's characterization of my interpretations as "deeply ahistorical," and the claim that I argue for "a single function and interpretation" which lasted for thousands of years (p. 101). In fact, I have discussed the issue of temporal change at great length, precisely in an effort to identify those aspects of the symbolism, meaning, and use of rock art that have remained constant, and those that have changed (e.g., Whitley 1994a, 2000d, 2000e; Whitley et al. 1999a, 1999b). Consider, for example, my following comment:

Judging from a systematic examination of the amount of revarnishing present in [Coso] human figure and bighorn sheep petroglyphs . . . it appears that bighorn sheep and rain shamans became more common in the last ~1000 years and that the making of petroglyphs increased during this time . . . The appearance of the patterned body figures appears to correlate with the appearance of the first Numic bands and headmen in the Great Basin [Whitley 1998b:161].

As should then be clear, since 1994 I have been discussing the evidence in rock art for ritual intensification over time, how this correlates with other changes in the archaeological record as a whole, and how this may reflect changing sociopolitical organization and ideological belief systems.

Indeed, the illogic of Quinlan's claim—in essence that I have ignored chronology and its impli-

cations—is emphasized when it is recalled that a major portion of my research over the last two decades has precisely concerned rock art dating. I have published 10 articles solely on rock art chronology and dating techniques. How, then, can my research be honestly characterized as “ahistorical”?

I could cite additional examples of Quinlan’s misrepresentations of my research,⁶ but the point now should be obvious. Not only has Quinlan systematically misrepresented the ethnographic evidence, he has also misrepresented my published interpretations of it.

THE BIG PICTURE QUESTION: ETHNOGRAPHIC CONTINUITY

Quinlan’s article is founded on a fundamental logical contradiction, inasmuch as he argues first that his analysis of the ethnographic evidence demonstrates that “a shamanistic context for the production and functions of rock art is denied” (p. 100). Second, Quinlan implies that this same ethnographic record concerns cultures that were so heavily impacted by Euroamerican contact that they can shed little or no light on precontact indigenous cultural patterns (p. 101). How can Quinlan claim on the one hand to disprove the shamanistic interpretation with the ethnographic record, yet at the same time imply that massive acculturation has rendered it useless? If the ethnographic record is no reflection of traditional lifeways, then what is the point of debating its details at all?

Despite this profound logical incoherency, Quinlan’s implied claim that massive acculturation has rendered our ethnographic record effectively valueless is a serious charge. If true, the careers of the likes of Alfred Kroeber, Julian Steward, and Robert Lowie, as well as numerous other American anthropologists and a current generation of researchers, have been entirely misdirected. Have literally dozens of us been intellectually confused and misled in our research, as Quinlan’s argument implies?

Two general points need to be made concerning

the evidence for ethnographic continuity, specifically here with respect to religious beliefs. For the sake of brevity, I consider only the Numic case, although similar arguments can be made and data cited for other regions where my ethnographic research has primarily focused (e.g., Forde 1931; Gayton 1948a, 1948b; Stewart 1974; see also Keyser and Whitley 2000; Whitley 2000e).

The first point is that my research approach has necessarily emphasized broad comparative studies, inasmuch as it consists largely of the analysis of previously collected data. Opler (1940:153) outlined the justification for such an approach: “Recent investigations throughout the Basin area have demonstrated conclusively that Shoshonean-speaking peoples possess a common core of similar conceptions, social organization, myths and beliefs.” The widely shared nature of Numic religious beliefs and practices is also emphasized by other ethnographers (e.g., Park 1938; Steward 1943b; Miller 1983, 1985). The continuing relevance of such an approach is acknowledged by the organization of Volume 11 of the *Handbook of North American Indians* (d’Azevedo 1986), where “Mythology and Religious Concepts,” for example, is treated as a single topic (Hultkrantz 1986b). Reflecting these widespread cultural traditions, I have accordingly emphasized that my Great Basin interpretations pertain to Numic rock art, not Shoshone or Northern Paiute or Southern Paiute art alone.

I have thus looked to a broad range of ethnographic literature to interpret the petroglyphs of the Coso Range, where the majority of my Basin work has concentrated, not just the Panamint Shoshone, who were one of the groups occupying this region.⁷ The intellectual validity of such an approach, with respect to the Cosos per se, has been corroborated by the existence of accounts indicating that the Cosos were jointly used by all Numic language groups for ritual purposes, including one shaman coming from as far away as Fort Duchesne, Utah (Whitley et al. 1999a). Similar evidence supports this same approach in other regions, and it is commonly the basis for much ethnohistorical research

(e.g., Applegate 1978; Hultkrantz 1986a).

Does the broad comparative approach have the potential to overlook certain kinds of variability? Yes, of course it does. This is precisely why I have emphasized the need to look for regional and functional variations, now that the broad synthesis has been defined (Whitley 1996:192-193).

Second, consider Quinlan's claim that acculturation has rendered ethnographic information on Native American religious beliefs and practices highly suspect, if not valueless: "... ethnographies collected in the first half of this [i.e., the twentieth] century in California and the Great Basin cannot be assumed to be representative of aboriginal cultural practices prior to Euroamerican colonization" (p. 101). But instead of assuming continuity and representativeness, this topic was in fact discussed, in some cases in detail, by many of the anthropologists who worked in the region—and whose comments Quinlan largely omits. Table 2 summarizes the evidence for continuity in Numic shamanism—the fact that traditional religious practices were ongoing and healthy into the first half of the twentieth century, despite changes in other aspects of culture, as these anthropologists made clear. Julian Steward, who spent a considerable part of his career studying the causes and consequences of culture change, stated this point quite clearly:

[Acculturation] has not wiped out all Indian practices. Acculturation has consisted primarily of modifications of those patterns necessary to adjust to the rural white culture . . . The Shoshoni retain, however, many practices and beliefs pertaining to kinship relations, child-rearing, shamanism, supernatural power and magic (Steward 1955:58).

Euroamerican contact certainly devastated Native America and, in some areas, literally destroyed all vestiges of traditional culture. But this did not happen everywhere to the same degree or effect, as Quinlan essentially asserts. Nor are acculturative processes equivalent in all aspects of a given culture as Sahlins (1985), in accordance with Steward (1955), has noted. Even in those portions of the far west where missionization was earliest, and most

intensive, the destruction of indigenous traditions was not complete.

That religion frequently shows great resistance to change and is deeply involved in the social structure as well as the psychological patterns of a culture has been repeatedly demonstrated in anthropology. This is also true for the Luiseño. A rich body of the central characteristics of the old native religion has persisted in spite of missionary influences for nearly two centuries . . . Many features of the old Luiseño culture have been preserved [White 1976:355].

No one, in other words, has uncritically "*assumed* [that the ethnographies are] representative of aboriginal cultural practices prior to Euroamerican colonization" (p. 101, emphasis added). The anthropologists that collected the ethnographic data have, by and large, shown that certain kinds of data *are* representative, meanwhile pointing to those areas of culture or regions where change eradicated traditional practices and knowledge. And I have paid careful heed to emphasize those regions in my research where cultural continuity was greatest (see Whitley 1994d, 2000a, 2000e).

The error here, then, is not in my putatively mindless assumption that postcontact ethnographies everywhere reflect precontact lifeways. The error instead is Quinlan's repeated effort to force an argument onto data that simply will not fit.

Nowhere is this better shown than in Quinlan's most astonishing internal contradiction, which concerns the effects of Catholic missionization. Quinlan (p. 100) at once cites Catholic missionization as a major force of disruption to traditional cultures, which certainly was often the case. Yet Quinlan (p. 96) also cites this same Catholicism as a likely impetus for historical rock art, known to have been made by a shaman. The implication of these two points is clear. In some cases, Catholicism *destroyed* native cultures, including their rock art traditions. But in others—where Quinlan needs to argue away the unambiguous and direct evidence for shamanistic rock art—Catholicism *caused* Indian shamans to make rock art.

Table 2
CONTINUITY IN NUMIC RELIGIOUS PRACTICES

Reference	Ethnographic Information
Euler 1966	From 1776 to the 1930s, Southern Paiute culture was fairly stable through historic period, with shamanism constant.
Kelly 1939	In the 1930s, shamanism was still vital in Southern Paiute life.
Kelly 1932:67	In the 1930s, Northern Paiute shamanism was still "in vogue" in Surprise Valley.
Chalfant 1933	In the 1930s, Owens Valley Paiute shamanism still persists.
Harris 1940	In the 1930s, Western Shoshone shamanism has been least affected by change, because it was least tied to economy.
Park 1938:12	In the 1930s, Northern Paiute economic life and material culture has been affected by reservation life but "many social customs and religious beliefs and practices persist. Shamans are still common, and curing performances are frequent."
Steward 1941	In the 1930s, the Western Shoshone still retain their traditional knowledge, in that shamanism, mythology, family life, and subsistence (pinyon and seed) gathering are still intact.
Whiting 1950	In the 1930s, several Northern Paiute shamans are still active in the Burns, Oregon, area.
Fowler and Liljeblad 1986:452	In the 1960s, "Jimmy George, a singing shaman . . . was active as a healer in Nev. and Calif. until his death in 1969."

CONCLUSIONS

The above discussion has highlighted a few of the key factual errors, misrepresentations of the evidence, omitted data, internal contradictions, and examples of illogic upon which Quinlan has built his argument. Quinlan asserts, for example, that the direct evidence for the depiction of shamans' visionary imagery is limited to the Western Mono, yet the precise source that Quinlan cites to justify his claim stated that it was a regional pattern also practiced by the Yokuts (see Driver 1937:126). Quinlan claims that vision questing was "largely unknown" by the Numic, but the very monograph he lists to support his contention provides detailed evidence contradicting this statement (see Park 1938). Then there is the independent confirming evidence—such as the numerous discussions of the shamanic use of and association with rock art sites in the southern Sierra Nevada (Table 1)—which Quinlan effectively ignores. And these concern

only some of those factual problems that space has allowed me to identify and correct.

There is a troubling side to Quinlan's article because these problems are not minor typographical errors, occasional omissions of a source or a reference, confusions about page numbers, nor one-off analytical lapses, any of which are annoying but are common enough. Nor do these errors concern gray areas in the literature, places where the ethnographic data are contradictory and where there may be room for differing interpretations. Nor are these irregularities simply cases of inflated or overstated claims, attributable to enthusiasm for a preferred theory. Quinlan's errors instead concern the central issues in this debate; his misrepresentations involve the key ethnographic evidence that addresses these points; his factual irregularities are numerous and systematic; and his omissions involve the majority of the corroborating data.

Certainly at debate here is an ethnographic interpretation and such is, by definition, a question of

interpretation and meaning, not in all cases one of unambiguous facts. Compounding this circumstance is the fact that the ethnographic record in some cases includes contradictory data, where there may be room for argument. This is why critical thinking, common sense, attention to the plausibility of an argument, and a thorough review of all the evidence is required to make such interpretations. After all, while it is true that some Native American consultants claimed that shamans did not make rock art (which I have pointed out in my publications; see Whitley 2000a), these same individuals likewise denied that “anybody” made the art, even though obviously someone did (see Driver 1937). Other informants also claimed that they did not use bed-rock or portable stone mortars, did not wear leather breechclouts, and did not use hopper baskets, yet in each case we know that these responses were simply wrong.

It is because of problems such as these that plausible ethnographic interpretation must be based on the recognition and simultaneous evaluation of three kinds of evidence: affirmative data which confirm an interpretation; contradictory or falsifying data; and negative evidence (the absence of information about a topic). The construction of an empirically plausible interpretation requires the careful consideration of each kind of data; it cannot be based on one type alone. In each case, the significance of the data must be evaluated carefully. A decision must be made whether the data are valid or instead reflect potential mistranslations, refusals to discuss a topic, failures to record information, lack of specific knowledge of the part of the consultant, and so on. Because of these potential problems, multiple lines of confirming evidence concerning each type of data are keys to the inferential process.

Ethnographers were tacitly aware of these potential problems. For example, Driver (1937:1) stated that his CED data were “obviously ancillary to full-length individual accounts of culture.” My approach to interpretation has always involved an awareness of the limitations of the ethnographic

record, and a careful effort to use this record within these limitations. I discount the negative statements of Driver’s consultants who claimed that shamans did not make rock art, for example, for two reasons. These same individuals also responded that the art was not made by “anybody,” suggesting a reticence to discuss the topic, and substantial and more detailed evidence from numerous consultants (collected by various anthropologists) contradicts their abbreviated CED responses. After all, Driver (1937:126) did not choose to highlight and quote one consultant’s statement—which supports the shamanistic interpretation—because he thought it was *wrong*, but because he believed it was *right*. In contrast, I have accepted Stewart’s (1942:333) and Riddell’s (1978:84) negative arguments that Numic girls did not make rock art during puberty rituals, despite the commonness of this practice in nearby southwestern California and the Columbia Plateau. This is because I have been unable to find any evidence directly contradicting this conclusion or other evidence, however circumstantial, which would support such a practice.

In addition to its numerous factual misrepresentations, Quinlan’s argument instead is predicated on selectively emphasizing only the evidence that suits his contentions, without any regard for its potential verity (or lack thereof), its plausibility in light of independent data, or even the logic of the inferences that result. For example, Great Basin rock art is mythic in origin, he claims; thus, it is neither shamanic nor recent, nor is it interpretable using ethnography because a couple of consultants stated that it was made by Coyote. But what about the 14 sources (cited above) stating that it was made by recent/contemporary peoples? What about the historical motifs at some sites? What about the numerous statements that shamans used the sites for vision quests? What about widespread use of the term “doctor’s rock” for the sites? And, of course, what about the fact that consultants acknowledged that spirits were continuously making rock art, as well as Coyote’s occasional function as a shaman’s spirit helper? Perhaps even more to the point, how

can the omission of all of this evidence result in a more direct reading of the ethnography, as Quinlan claims to present?

The point then is that individual ethnographic statements can be taken out of context and used to build an argument putatively contradicting *any* anthropological interpretation, at least if there is no concern with the plausibility of what results. Hence, Quinlan can argue that Chumash rock art, known to have been painted by a historical shaman, may have had its impetus from Catholicism rather than traditional shamanistic practices. But this proves nothing other than the contrarian nature of the debater and, when combined with systematic factual misrepresentations, raises concerns about the real motivation behind such a critique.

If Quinlan wishes to continue this debate, accordingly, I then request some direct responses to the questions that his use of the ethnographic evidence raises, not a "shifting of the target" to other issues or topics in an effort to divert attention from the problems that I have outlined in his critique of my research. In addition to the queries raised throughout my response, these are as follows.

First, I have identified Quinlan's misrepresentation of Driver's (1937:126) clear statements concerning the shamanic depiction of visionary imagery by both the Western Mono and the Yokuts, as well as the other evidence confirming this fact, which Quinlan omitted in his article. In light of this correction, does Quinlan continue to maintain that in south-central California the painting of visionary imagery by shamans was restricted to the Western Mono alone, or will he now acknowledge that it was instead a regional practice?

Second, in light of the numerous different kinds and amounts of evidence demonstrating that Numic peoples perceived the creation of rock art as an ongoing phenomenon, does Quinlan continue to insist that they instead thought it as "mythic, timeless"? If so, on what evidential basis?

Third, Quinlan (p. 99) has asserted that "The Yokuts (Gayton 1948a, 1948b) and Eastern Shoshone (Hultkrantz 1987) both generally denied that

shamans made rock art." Where specifically in these two cited references did these ethnographers' consultants "deny" that shamans made rock art; that is, where did they state explicitly that the art was not made by shamans, and using what precise words? And if their respective consultants in fact "generally denied" that rock art was made by shamans, then why did Hultkrantz (1986a) and Gayton (as cited in Steward 1929:226) elsewhere imply or state precisely the opposite?

Finally, Quinlan (p. 102) concludes his article by asserting unequivocally that, in "the ethnographies of far western North America . . . [a]ccounts that actually link the production of rock art with shamanism are limited to a handful of Californian groups." In light of this sweeping claim, I must ask how Quinlan justifies the omission of the ethnographic references to shamanic rock art on the Columbia Plateau, especially inasmuch as a portion of this region extends into California (i.e., the Klamath Basin; see Walker 1998) and has been included in my research (e.g., Whitley 1994b, 1998c, 2000a). Moreover, the one rock art book with a Native American author is titled *They Write Their Dreams on the Rocks Forever: Rock Writings in the Stein River Valley of British Columbia* (York et al. 1993), and it is only one of a number of Plateau references that provide further direct evidence of shamanic rock art (see Keyser and Whitley 2000). In light of these overlooked data, does Quinlan continue to support his argument that shamanic rock art in the far west was restricted to a handful of California groups? Or is this instead just another example of a false claim based not on a "more direct reading" of the ethnography, but instead on a superficial, confused, and subjective examination of it?

NOTES

1. As I have noted previously (e.g., Whitley 1996, 1998c, 2000a), Yokuts shamans made rock art to depict their spirit helpers, to illustrate themselves in their "man of power" form, to show dangerous supernatural beings, as part of sorcery rituals, and for additional shamanistic purposes. This is in agreement with Stew-

ard's (1929:226) comment that the pictographs are not exclusively depictions of spirit helpers.

2. In the article that Quinlan cites, I explain why this consultant, who is a Yokuts tribal member, can be inferred to retain traditional knowledge about rock art (Whitley 1994b:92-93). Not only do his statements agree with those of Driver, Aginsky, Steward, Gayton, and others, but the consultant has also corrected a factual error in the published ethnographic literature. This concerns the identification of the *Huhuna* shaman-dancer. The published record suggests that these are Loon dancers; the consultant claims that they are Blue Heron dancers. As it turns out, Loons do not occur in California, although Blue Herons are commonly misidentified as Loons.

3. As a general rule, Coyote was considered a spirit helper for sorcerers rather than curing shamans. It is thus likely that the minor contradiction in the ethnographic literature on this point stems from whether consultants and/or ethnographers considered sorcerers as shamans or, instead, a distinct category of supernatural actor. I have sometimes made this distinction myself. But the important point here is that there was general agreement that Coyote was a spirit helper.

4. There has never been any doubt that there is a strong auditory component to altered states experiences, especially in Numic vision quests, and I have discussed the aural component of hallucinations in some detail (Whitley 1994c:26-27). But Quinlan's implication that visual hallucinations either did not occur at all or were not considered important is contradicted by descriptions of visions that Park himself and other Basin ethnographers transcribed (e.g., Park 1938:26-27; Steward 1941:258), as well as the ethnography of surrounding regions (e.g., Irwin 1996). Moreover, Quinlan's assertion is contradicted by over 100 years of clinical research in neuropsychology, inasmuch as this demonstrates, beyond any doubt, that vision is the dominant sensory modality (Kosslyn and Koenig 1992).

5. In certain previous papers, I have defined "rock art" as pictographs and petroglyphs, thereby excluding cupules and earth figures, and I have argued that the first two classes are related to *shamanistic* beliefs and practices (which includes art made by nonshamans). But as Quinlan makes clear, his definition includes these other forms as well. In order to avoid meaningless definitional debate, throughout my response I therefore employ Quinlan's preferred and more wide-ranging definition.

6. I also have not had the space to outline the numerous similar errors, misrepresentations, and logical contradictions in Quinlan's (pp. 96-97) discussion of puberty initiation rock art in southwestern California. Suffice it to note that his confused and error-filled argument on this topic, like much of the rest of his arti-

cle, is founded on a false assumption: That because ethnographers stated that puberty initiations were conducted to secure "obedience to social and cultural norms" (p. 97), this somehow precludes the possibility that they were shamanistic. Apparently, in Quinlan's view, shamanism cannot have a social function. Quinlan would benefit here by consulting Driver's (1969) introduction to North American ethnography, where the place of shamanistic initiations in the inculcation of social norms is discussed at length; Irwin (1996), among others, also discussed this topic. Driver (1969: 350) further explained that: "In southern California [puberty initiations] . . . the central theme was the obtaining of contact with the supernatural through the medium of a narcotic plant . . . The hallucinations consisted principally of visions of animals . . . Such animals became the lifelong spirit helpers of the boys."

Likewise, I have not had a chance to respond to his critique of Lewis-Williams and Dowson's (1988) neuropsychological model (pp. 92-93). On this topic, I note solely that the model Quinlan claims to be critiquing has little recognizable relationship to the neuropsychological model which these two archaeologists have developed and published, and which I have read. Insofar as I can tell, Quinlan's discussion of their research only serves to demonstrate one of Lewis-Williams and Dowson's (1988) underlying propositions: All humans, cross-culturally, are capable of hallucinating.

7. In a recent paper, Hedges (2000) publicly and quite pointedly criticized me for committing the putatively comical error of confusing the ethnographic record of surrounding Numic groups with that of the Coso Range. Familiarity with Numic linguistics, tribal and band names, methods of ethnohistorical research, and the Numic ethnographic record, however, would clarify Hedges's confusion. First, as recorded by Chalfant (1933), Laird (1976), Brooks et al. (1979), and others, the Panamint-Coso *region* was widely recognized as an extensive sacred landscape, and was used intertribally. Speaking of the Southern Paiute, Laird (1976) described it as *Tiwiin'arivipi*, "Mythic Country, Storied Land," where the myths begin and end. Fowler et al. (1995:49) noted in a specific case that, "Coso Hot Springs, sacred to Shoshone and other neighboring peoples, including Owens Valley Paiute, is widely regarded as the most powerful of many healing springs in the west." Second, Steward's (1933, 1938) and Grosscup's (1977) maps of territorial boundaries in this region agree that Southern Paiute-speaking peoples occupied the Panamint Valley as far north as Ballarat, along with the southern half of Death Valley. This places Southern Paiute speakers minimally on the eastern flank of the Argus Range, which abuts the Cosos and is part of the "Coso" petroglyph locality. Third, the

terms "Coso," "Panamint," "Timbisha," and "Western Shoshone" (and even occasionally "Paiute") have been used interchangeably by different authors, with Panamint in some cases referring to Southern Paiute residents of Panamint Valley, despite its current linguistic use as the language for eastern California Shoshone.

All of this evidence points to the fact that ethnohistorical research necessarily must be broadly comparative, and that the ethnographic record relevant in the Coso case involves the Shoshone, Southern Paiute, and Northern Paiute, not that of the "Coso Shoshone" alone, as Hedges implied.

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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-