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Comment on the Complete Works of Stillwater, Nevada, Archaeology

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In 1990, the *Journal of California and Great Basin Anthropology* (Vol. 12, No. 2, pp. 275-279) published a review by Eugene M. Hattori on Stillwater archaeology done by the U.S. Fish and Wildlife Service, Region 1 *Cultural Resource Series* No. 1 (2 vols). Hattori said (1990:275) "The only archaeologist conspicuously missing from this "guest" lineup is Donald R. Tuohy who supervised the initial phase of this [Stillwater] project (Tuohy et al. 1987)." Hattori brought up a most important question. Does any federal agency managing archaeological resources have the right to publish only what it wishes to publish—relegating the first four papers on Stillwater to the "dark literature" on archaeology, and publishing all of the rest of the submitted papers? The following comment will attempt to answer that question. I will start by quoting two recent reviews of Stillwater, Nevada archaeology.

The *Journal* had two separate reviews of the recent work in Stillwater, Nevada. Rhode (1991) reviewed *Prehistoric Human Geography in the Carson Desert, Part I: A Predictive Model of Land-Use in the Stillwater Wildlife Management Area*, by Christopher Raven and Robert G. Elston, as well as Christopher Raven's *Prehistoric Human Geography in the Carson Desert, Part II: Archaeological Field Tests of Model Predictions*. Hemphill (1991) reviewed Sheila T. Brooks, Michele B. Haldeman, and Richard T. Brooks' paper *Osteological Analysis of the Stillwater Skeletal Series*,

Stillwater Marsh, Churchill County, Nevada. All three of the above-mentioned papers were published by the USDI Fish and Wildlife Service, Region I, in their *Cultural Resource Series*, Nos. 2 (1988), 3 (1989), and 4 (1990). The fourth book in that same series is entitled *In the Shadow of Fox Peak, An Ethnography of the Cattail-Eater Northern Paiute People of Stillwater Marsh*, an excellent work by Catherine S. Fowler.

This comment draws attention to several volumes of the "gray literature," apparently so dark gray that no one saw them. I refer to the papers produced on Stillwater archaeology in the late 1980s when a goodly portion of the Stillwater Wildlife Refuge was underwater. Specifically, I refer to four reports (Tuohy 1986a, 1986b, 1986c, 1987) the Nevada State Museum produced for the Fish and Wildlife Service. The only mention of these reports in the above reviews and in the book by Fowler was by Brian Hemphill, and he cited the 1987 (final report) incorrectly. He listed ". . . D. R. Tuohy, A. J. Dansie and M. B. Haldeman" as "editors" of the 1987 report, and he only cited the page numbers of Haldeman's report (pp. 1-78). Donald R. Tuohy was the editor of all the above reports, and the page numbers of the 1987 report to the Fish and Wildlife Service were 1-323. Below, I point out other errors made in the reviews by Rhode and Hemphill and question why Fowler did not cite all the above references in her book. Although her book was an ethnography of the Cattail-Eater Northern Paiute people, she had an appendix (B) which featured "Cattail-Eater Archaeology" with a separate bibliography, and which failed to cite our work.

Could it be that the Fish and Wildlife Service attempted to convert an environmental

disaster into a public triumph through publications suited to the public taste (Raymond and Parks 1989; Tuohy and Raymond 1989; Raymond 1992; Morris and Raymond 1993)? I do not know. I do know that at that time I was suffering from a stroke, and had trouble remembering where I worked in 1985; so much trouble, in fact, that the due date of the final report to the Fish and Wildlife Service was extended for one year.

When the former director of the Churchill County Museum, Sharon Taylor, reported to the Nevada State Museum that there were numerous skeletons weathering out of the Stillwater Fish and Wildlife Service ponds (made greater by the spring runoff in 1984), I did not believe her until we went out together and I saw parts of the human skeletons for myself. Previous to our trip, which was made by Alice Baldrice, Amy Dansie, Sharon Taylor, and myself, I had been shown pictures of how the Fish and Wildlife archaeologist (who shall be nameless in this comment) had completely ignored the Stillwater archaeology. Once properly notified, the Fish and Wildlife Service responded quickly; no doubt they had been through this type of problem before with the discovery of the *Bertrand*, a buried steamboat found near the Missouri River, approximately 25 miles upstream from Omaha on the De Soto National Wildlife Refuge (Petsche 1974).

To be fair to the Fish and Wildlife Service, I must note that they took immediate action to right the wrongs they had committed in the past by appropriating enough money to cover the physical anthropology (Brooks and Brooks 1990), ethnology (Fowler 1992), and archaeology of the Stillwater National Wildlife Refuge (see references). Could it be possible that the Portland District Office spent more money popularizing the results of the Stillwater "digs," to the detriment of publishing the details, maps and drawings of our "salvage" project (Tuohy 1986a, 1986b, 1986c, 1987)?

Let us now examine the reviews of the Stillwater archaeology to find out what the reviewers lacked in the formal presentation of the Stillwater archaeology. I shall proceed from the two reviews by Rhode (1991:127-130), followed by a review by Hemphill (1991:131-134). I have said enough about Fowler's (1992) excellent presentation of the Cattail-Eater Northern Paiute people, and its incomplete archaeological bibliography.

Rhode's smooth assessment of the Fish and Wildlife's *Cultural Resource Series*, Nos. 3 and 4, mentioned the initial work of the Nevada State Museum in his second paragraph, but the "dark gray" references to that work were left out of his bibliography. Rhode did mention two of the early papers (Raven and Elston 1988; Raymond and Parks 1990), and he also mentioned the work of Kelly (1985, 1988, 1990), who dealt with pre-flood conditions in the Carson Sink and the nearby Stillwater Range. Rhode (1991:128) also lauded Raven and Elston (1988) and Raven (1990) for the predictive model of prehistoric land use they developed and used in the Stillwater Wildlife Management Area. So do I. But, as Rhode (1991:128) pointed out "These insights are important both for the present and future explorations, but they are not well-integrated into the present study, in part because they occur at a scale larger than the scope of the present study." To which I comment, Amen!

Rather than pursue the research design and results obtained, I repeat that this paper is not a review of the work done in the Carson Sink, but is a commentary about recent work done on the Stillwater Wildlife Refuge Area. With all of the "arrowhead" collecting done there in the past century (Kelly 1985), the existence of the marsh and marshside middens at Stillwater should reflect changes in the ecosystem as a result of the European arrival. I would like to point out, as Rhode (1991:121) stated, that there is disagreement among archaeologists who worked there as

to the time of arrival of the first humans. I estimated the recovered points to range in age from recent to 5,000 years B.P. (Tuohy 1987: 237); Kelly (1990) argued that residential use of the Carson Desert lowlands prior to 1,500 years ago was negligible; Raymond and Parks (1990: 58) argued that there was no significant change in settlement around 1,500 B.P.; and Raven (1990) argued for a continuous temporal pattern of projectile point deposition "for at least five, and perhaps as many as seven, millennia." If the whole Carson Desert were the topic of discussion, I would make the temporal period extend back to approximately 9,500 years B.C. and say the cultures represented were members of the Fluted Point Tradition (8,000-6,000 B.C.), and the "Western Pluvial Lakes Tradition" (9,000-6,000 B.C.) (Bard et al. 1981:139-142).

And now let us return to the review by Hemphill (1991:131-134). As I pointed out before, he was the only reviewer to cite one of our "dark gray" publications, but he had the editorship and the page numbers of the whole report incorrectly cited. I agree with Hemphill on his assessment of the first six chapters of the Brooks et al. (1988) paper, but I question his call for "three additions." Those "three additions" were: 1) archaeological and ethnographic background sections, 2) the use of multivariate statistical analysis of metric, nonmetric, and dental variation, and 3) the authors' claim of biological homogeneity throughout Nevada could be more effectively tested if samples from outside the Great Basin were considered. I agree with Hemphill on his second and third points, but his first point was adequately covered by our "dark gray" reports, with the addition of the Bard et al. (1981) overview.

This comment, in large measure, is a statement about the present state of knowledge of American archaeology in 1994. The unpublished "gray literature" grows more mas-

sive with each passing day. Most of it does not warrant publication, but some of it is critical to our knowledge. I do not know what influence our "dark gray" publications would have had on Catherine S. Fowler, David Rhode, or Brian Hemphill, but I am certain those preliminary reports would have made some impression on their critical thinking about archaeology in the Great Basin. I recognize some of our ideas in print by various subsequent authors. I also think one solution to this problem would be that the State Historic Preservation archaeologists, who do read these "gray" reports, should make a recommendation as to whether these reports should be published.

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