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The Journal of California Anthropology

Title

Reply to King

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Journal

The Journal of California Anthropology, 4(1)

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Publication Date

1977-07-01

Peer reviewed

Kowta's comments on the reliability of my historical interpretations. The overview made it possible for me to do more, however. I was able to synthesize interpretations of California desert climatic change and discuss the possible demographic effects of such change. I was able to comment on possible reasons for differences between Serrano and Chemehuevi settlement organization. I was able to consider the social meaning of different forms of rock art. I was able to speculate about what caused the expansion of Uto-Aztekan into the Great Basin. Considering Kowta's long and deep experience with southern California prehistory, I would especially have appreciated his comments on these attempts to synthesize, interpret, and formulate hypotheses. I am disappointed that he did not see them as important enough to justify comment.

Kowta in fact seems uncomfortable with the fact that I emphasized the research value of archaeological sites at all, insisting that my paper should have considered matters of "management *vis-à-vis* the non-archaeological public." Since the paper was written in standard English, with a glossary of technical terms, and since it includes recommendations for survey, considerations of National Register eligibility, and so on, I assume that what Kowta misses is some sort of recommendation concerning public use and interpretation of the Monument's prehistory. I object. We do not expect biologists who study the Monument's snakes and cacti to include a public interpretation element in their research reports, nor do we expect geological researchers to include recommendations about how to display rocks. We assume that their research itself serves the public interest, and that it provides useful information for management purposes. Does Kowta think that archaeologists should become public interpreters just because they do research in a park? Is archaeological research itself so illegitimate?

My concern about Kowta's position rises from the fact that his opinion is shared by

many in Park Service management. Despite the fact that National Monuments are created under the Antiquities Act precisely to preserve "objects of historic or scientific interest" (34 Stat. 225; Sec. 2), and the fact that the National Park Service itself was initially created as a preservation agency, an orientation toward recreation and lowest-common-denominator "interpretation" has increasingly come to dominate National Park Service upper management thinking. This has served to justify the sacrifice of archaeological sites that could not be effectively put on display within the parks, the "restoration" of historic structures at the expense of their archaeological integrity, and the employment of Park Service archaeologists and historians whose scholarly abilities are nil. It is not only disappointing but rather frightening to see the same philosophy being adopted by non-Federal archaeologists. The National Parks and Monuments *should* provide our best bank of preserved research resources, and if we are not willing to argue for the priority of research and the preservation of research value over public use and interpretation, who will?

Washington, D.C.



Reply to King

MAKOTO KOWTA

The limited space available for my review precluded a full discussion on all aspects of the solidly executed overview in question. Its author's comment above provides additional details which readers will find useful in arriving at a more complete comprehension of its contents.

The concern alluded to is not so much that King's study should have undertaken the task, but that management problems *vis-à-vis* the

public be addressed. It is my opinion that there is a need for sound public use management plans that give appropriate consideration to both long and short range research needs (now identified for the Monument), are adapted to them, and seek to increase public appreciation of such needs. Moreover, to the degree that the situation described by King may exist, it would appear necessary that qualified archaeologists be involved in the formulation of such plans.

California State University, Chico



Reply to McGuire and Garfinkel

ROBERT L. BETTINGER

McGuire and Garfinkel (1976) have argued that evidence presented in my recent article discussing the origins of pinyon exploitation in Owens Valley, eastern California (Bettinger 1976), fails to demonstrate adequately the beginnings of that procurement system at about A.D. 600, and signals only the initiation of an intensified form of pinyon exploitation that required the processing and storing of pinenuts in the pinyon zone. They contend that prior to A.D. 600 pinenuts might have been processed and stored at lowland winter villages, leaving little direct evidence in the pinyon zone. To support their case, they cite the results of surveys in Reese River, central Nevada (Thomas 1973), where items related to pinyon exploitation, principally rock rings and millingstones, were remarkably rare in the pinyon zone despite heavy reliance on pinenuts as a dietary staple. I will restrict my comments to a few major points.

First, as *explicitly* phrased by McGuire and Garfinkel, the proposition that intensive prehistoric pinyon procurement in Reese River produced little physical evidence rests entirely

on an asserted analogy between prehistoric and ethnographic adaptations in that region. As such, it violates the basic principles of scientific deduction, particularly those relevant to the use of analogy. Their argument can be crudely rendered as follows: (1) Intensive historic pinyon exploitation is documented for Reese River (p. 84). (2) Prehistoric pinyon exploitation in Reese River was as intensive as historic pinyon exploitation (p. 84). (3) There is little physical evidence for prehistoric pinyon exploitation in Reese River (p. 83). Therefore, (4) intensive prehistoric pinyon procurement in Reese River produced little physical evidence (p. 84). By definition, then, McGuire and Garfinkel are unable to demonstrate (2) on independent, archaeological evidence, having stipulated that (3) there is little physical evidence for pinyon procurement in Reese River. Moreover, the situation would be difficult to resolve by new research because the archaeological evidence necessary to demonstrate (2) would always contradict (3). Of course, the real situation is far more complicated than this and there are presumably ways to demonstrate prehistoric Reese River pinyon exploitation on archaeological grounds (for example, by recovering plant macrofossils), but the above analysis should serve to illustrate the inconsistencies inherent in the simplistic approach taken by McGuire and Garfinkel. Added to this, the apparent explanation that rock rings and millingstones are poorly represented in the Reese River pinyon zone because pinenuts were processed and stored at winter villages (p. 83) is remarkably naive because Reese River winter villages are in the pinyon zone, not in lowland communities as is the case in Owens Valley, settlement patterns in the two regions being not at all comparable in this respect. In fact, taken at face value, the limited Reese River material cited specifically by McGuire and Garfinkel is consistent with the interpretation that neither pinyon procurement nor winter villages are typical of the Reese River pinyon zone. Obviously, this runs contrary to