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Reply to McGuire and Garfinkel

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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.

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## ***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 millings, 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 millings 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

the interpretation suggested by Thomas (1973), a view supported by a subsequent Reese River survey in which we found rock rings to be common in the pinyon zone (Thomas and Bettinger 1976). In view of these recent findings, perhaps the most reasonable inference to be drawn from the original Reese River material is that the physical remains of pinyon procurement are so distinctive that there is little chance of missing them, even when they are quite rare. This can justifiably be interpreted to imply that it is unlikely we would have been unable to locate evidence of early pinyon procurement in Owens Valley, whatever its intensity, provided that it occurred with some degree of regularity.

A careful review of the evidence from Owens Valley yields essentially the same conclusion. A crucial aspect of pinyon exploitation in Owens Valley is that travel between lowland winter villages and pinyon groves requires a round trip of about 26 kilometers and a vertical climb of about one kilometer, necessitating the establishment of temporary camps in the pinyon groves during the nut harvest. McGuire and Garfinkel suggest that prior to A.D. 600 these camps might have lasted only a few weeks, leaving no physical evidence. For stays of this duration, camps inhabited by individual nuclear families, the minimum group needed to collect pinyon, would be furnished with food processing equipment and temporary shelters as attested by both archaeological and ethnographic sources (e.g., Dutcher 1893). Thus, pinyon procurement of the restricted magnitude envisioned by McGuire and Garfinkel would certainly leave substantial physical evidence. Moreover, since it is reasonable to believe that hunting gear would be carried on such expeditions, these remains could be dated on the basis of time-sensitive projectile points.

It is conceivable that milling implements and shelters might be omitted for camps lasting less than four or five days, but the products of such a hasty trip would probably not justify the

effort involved. Granting for the sake of argument that such brief excursions did occur, it does not follow that no physical evidence would remain. Indeed, among the basic implements employed in the pinyon procurement system are stone hammers and anvils used to dislodge nuts from both ripe and artificially opened cones (Dutcher 1893). In Owens Valley, this initial processing step was performed in the pinyon groves prior to transporting the nuts to lowland winter villages owing to the weight and bulk of the unthreshed cones and the distance involved. There is no question that these anvils are readily recognized in archaeological contexts; we recorded literally dozens of them during our Owens Valley surveys, classifying them as millingsstones because many exhibited secondary use as grinding implements (Bettinger 1975). It is obvious, thus, that even if pinyon camps lasted only a few days, they would leave solid, datable evidence.

The real point at issue here is what McGuire and Garfinkel see as the nature of pinyon procurement prior to A.D. 600. If, on the one hand, they believe that during this period the inhabitants of Owens Valley were aware that pinyon was edible, that hunters occasionally ate it, and that once in a great while a family might make a trip to collect it, then their view is equivalent to mine. On the other hand, if they believe, as they seem to, that prior to A.D. 600 pinyon procurement was a regular feature of the subsistence-settlement system, then the data clearly fail to support them. This does not mean that their conclusion is false; it merely means that there is no compelling reason to believe it is true.

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