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# Introduction: The Future of Great Basin Anthropology

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**T**HE following four papers were originally presented at the plenary session of the 22nd Great Basin Anthropological Conference, held in Reno, Nevada in October of 1990. Speakers addressed the question of what Great Basin anthropologists should be doing in the next 10 years. I asked them to tackle this difficult task because anthropologists today question many of the fundamentals of our field: the "voices" of ethnography, the inferential methods of archaeology, and the relationships between anthropologists and the world's cultures. The authors make some important suggestions about future problems and approaches in Great Basin anthropology. Not everyone will agree with all of the suggestions, but they need to be considered because, like it or not, anthropology will not proceed in the 21st century as it has in the 20th.

Kay Fowler describes the tasks that lie before us in cultural and linguistic anthropology. The greatest need is to clear the decks of already collected anthropological data. This includes editing and publishing existing field notes, photos, and reports. University presses should take note of the need to publish old manuscripts and notes that, while not theoretically current, nonetheless contain important data: Willard Park's field notes are a case in point (Fowler 1989).

We must learn, however, how to use these ethnographic data carefully since, for the most part, they are neither direct observations nor descriptions of pre-contact life. Ethnographers in the 1930s collected memory culture—lives of informants' parents as recalled by the informants. But long before the parents of Steward's

informants were born, European exploration and colonization, along with the diseases they brought, had already changed the lives of Great Basin peoples (see Beck and Jones, this issue). Carrying out the Hudson Bay Company's "scorched stream" policy in the 1820s, Peter Skene Ogden's trappers certainly reduced the availability of fauna in the northern Great Basin (Cline 1974; Rusco 1976). We can only guess how this affected the people who lived there. The influx of thousands of California-bound immigrants along the Humboldt River in the 1840s and 1850s undoubtedly altered the environment (Clemmer 1989), as did cattle ranches and mining towns in later years (e.g., Thomas 1971). Long before competent ethnography was written, native peoples had adapted to the new "resources" of wagon train livestock, as well as to the ranches and mining towns that provided wage labor, goods, tobacco, and alcohol. Anthropologists are reevaluating ethnographic data worldwide in light of the impact of "world systems" on purportedly isolated peoples (e.g., Wilmsen 1989). With all due respect to Julian Steward and his generation, Great Basinists, too, must reevaluate how they use the ethnographic data base.

Beck and Jones make a similar plea for archaeological data. When I show slides of sites in places such as the Carson Sink in classes, my Kentucky students often gasp in horror—not only because Nevada is such a stark contrast to the humid Ohio River Valley, but because the sites are simply scatters of lithic debris. There are no burial mounds, deep shell middens, or stratified river deposits. The

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greater part of the archaeological record in the Great Basin lies on the surface. Beck and Jones argue that we must come to grips with the reality of analyzing this data set, one that is difficult, if not impossible, to date, where physical associations between objects do not necessarily indicate temporal or functional association. In the past, archaeologists (myself included) collected this record in terms of the category of "site," physical clusters of material assumed to have temporal and behavioral integrity. These are then treated as the archaeological equivalents of ethnographic hunter-gatherer camps. Maybe a few are, but most are undoubtedly complex palimpsests, the result of many decades (if not centuries or millennia) of human and natural activities. We need to analyze them as such.

Beck and Jones suggest that we shift from a focus on sites to a focus on landscapes, and rather than do what might be called "site ethnography," they suggest we reconstruct landscape use histories. Thomas (1988) uses such an approach in Monitor Valley. Using both the concepts of "site" and "nonsite," Thomas examined the distribution of artifact types across environmental zones, as well as the relationships between sites-as-different-sized-samples and assemblage diversity.

Landscape or distributional archaeology is profitably used elsewhere, and leads to new forms of data collection (e.g., Ebert 1992), including piece-plotting all artifacts found within survey units. Such a task might have seemed impossible a decade ago, but with portable computers and laser-assisted mapping devices it is becoming feasible today.

Beck and Jones also note that Great Basin archaeologists have not contributed to the current debate between processual and post-processual archaeology. Marxist, structuralist, or symbolic approaches have yet to be used in the Basin, where an ecological paradigm, in one form or another, still reigns. Nonecological

approaches could be used in Great Basin archaeology; however, Beck and Jones suspect that the Great Basin's contribution lies in a greater commitment to a more refined evolutionary approach. I concur, and add more specifically that the Great Basin can contribute to understanding hunter-gatherer adaptation by documenting variability in prehistoric Great Basin lifeways and analyzing that variability within the paradigm of evolutionary ecology. Here, we already see new approaches to problems that are valuable to the rest of archaeology: the Numic migration (Bettinger and Baumhoff 1982; Bettinger 1991b; Yonker and Bettinger 1992), resource selection and transport (Simms 1987; Jones and Maden 1989; Rhode 1990), and mobility (Kelly 1995). The recognition that Great Basin hunter-gatherers, in contrast to the received ethnographic view, used high altitude locations (Bettinger 1991a; Thomas 1982) has altered views of Basin prehistory. The discovery of human remains at Stillwater Marsh (Brooks et al. 1988; Larson and Kelly MS) and around the Great Salt Lake (Simms et al. 1991) will expand our perception of Great Basin lifeways and provide data for comparative analyses. Great Basin archaeologists may never jump on the post-processualist band wagon, but we can nonetheless help shape a post-processual archaeology.

On other fronts, several Basin archaeologists are in the vanguard of implementing ethnographic studies in the analysis of site structure (e.g., Metcalfe and Heath 1990; Simms and Heath 1990). The cultural resource management (CRM) project undertaken by the Intermountain Research at Tosawihui Quarry promises to provide archaeology with new approaches to cope with the volumes of material that such sites generate. Large regional studies initiated in Monitor Valley, Owens Valley, and Steens Mountain are now coming to fruition; they will no doubt play major roles in structuralist archaeological research elsewhere in North

America (e.g., Thomas 1983a, 1983b, 1988; Bettinger 1989; see Beck and Jones, this issue).

Elston and Hatoff add some comments on the particular issues of cultural resource management and the role of the public in archaeological research. Elston takes a hard but honest look at the relationship between academic and CRM archaeologists. Academic archaeologists often denigrate CRM, but there is nothing inherent to the CRM process that should make it poor archaeology. Since most archaeological research is CRM, it is everyone's major source of comparative data. And, since it operates with temporal restrictions but generally adequate funding, CRM often creates more accurate and efficient field methods, benefitting everyone. Rather than bemoaning the quality of CRM, therefore, Elston urges academic archaeologists to actively improve it by participating in the report review process. He further points out that while faculty train students for jobs in academia, the majority of their students will probably be employed in CRM (and this trend will continue well into the future). He urges academic archaeologists to recognize this fact of life, and suggests skills students should learn for today's job market.

Whether it is CRM or academic research, however, archaeology is ultimately supported by the public. Hatoff argues that archaeologists need to remember this and incorporate greater public participation in projects where feasible. He gives the Grimes Point/Hidden Cave area as an example of public participation in archaeological research. Incorporating the public into research, through volunteer or tour programs, may at first seem like an onerous burden to a project director. But a well-informed public is much more likely to support publicly funded archaeological ventures and archaeologically related government legislation. The benefits of incorporating the public are not always felt immediately, but, especially when directed at children, will eventually become known. As

someone who worked on the excavation of Hidden Cave and later conducted research in the Carson Sink, I can attest to the good will that the Hidden Cave project generated among the local population, and the leads it opened to me.

This brings us to the most difficult issue facing anthropology as the 20th century draws to a close, an issue that Kay Fowler touches upon. Ethnography and archaeology are strong symbols to many Native Americans. Unfortunately, they are often symbols of the ultimate theft—the appropriation of another's history and culture. Archaeology has faced this issue primarily in terms of the reburial of human remains, but across the country the division between anthropologists and Native Americans goes beyond burials to questions of legitimacy and authority. In the eyes of some, but by no means all, Native Americans, Anglo anthropologists do not have the right (or knowledge) to speak for Native American history and culture.

Many anthropologists might argue that members of a culture do not necessarily have a monopoly on understanding that culture and its history. But this argument, while correct (I know one Native American who knows far more about European colonial history than I do), only serves to widen the gap between anthropologists and Native Americans. Native Americans become concerned when they see their culture (or what they take to be misrepresentations of their culture) used to pursue what they see as political objectives in Anglo society. (At the recent Southwest Symposium in Tucson, this concern was expressed over the recent book *The Zuni Man-Woman* [Roscoe 1991], which one Native American saw as a way to justify gay lifestyles in Anglo society.) And given the amount of archaeological and ethnographic material that languishes on the shelves of museums and archives (as Kay Fowler notes), there is some justification in the claim that anthropologists collect for the sake of

collecting and for career development, rather than for increasing and disseminating knowledge.

We must invite Native Americans to participate in the learning process. However, we cannot expect simply to bring Native Americans "into the fold"—although there is nothing wrong with opening up possibilities, especially financial ones, for more Native Americans to enter anthropology. Instead, we must recognize that Native Americans will bring completely new perspectives to the discipline. Considering the downright hostility and derision that we give to competing theoretical perspectives (witness the debates between processual and post-processual archaeology), some anthropologists may give the same lack of respect to Native American ideas. When Native American concerns are given serious treatment, when tribal representatives have genuine control over a situation, when anthropological interests are explained in layman's terms, and when the benefit for Native Americans is made clear, we find that Native Americans can be more than accommodating. This was the situation in the Carson Sink, where Fish and Wildlife Service Archaeologist Anan Raymond consulted with the local Fallon Paiute-Shoshone Tribe as soon as possible after the discovery of burials in the Stillwater Marsh, and then at every step of excavation and analysis afterwards. The result was that the Tribe was satisfied and an enormous amount of anthropological data was gathered.

Truly incorporating Native American perspectives into our research, rather than appropriating or, worse, ignoring them, will not be simple, for it does not mean simply writing an additional chapter that describes the Native point of view nor does it mean that the Native American viewpoint is the only correct one. It does mean compromise. It does mean intense scrutiny of our motives. And it does mean some soul searching. For example, at the

Southwest Symposium a Native American panel noted that it was difficult for Indians to accept that anthropologists take Indian religion seriously in interpreting prehistory when those anthropologists themselves are not religious.

Great Basin archaeologists do not yet have as much interaction with Native Americans as those working in other regions—the Southwest, for example. It will be a challenge to develop these connections because we cannot count on institutional mechanisms or bridges to do the job for us. Perhaps the greatest challenge of the 21st century lies not in evaluating the ethnographic database, dating surface sites, resolving the processual/post-processual debate, or building bridges between CRM, academic archaeology, and the public, but in reconciling ourselves, individually, and our science, collectively, with the people whose past and present lives are of such great interest to us.

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