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Doing Great Basin Archaeology Recently: Coping with Variability

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Great Basin archaeologists spent the 1970s and most of the 1980s tearing down the Desert Culture hypothesis without presenting compelling means for dealing with the empirical variability that made it untenable. Recent research seeks to understand this variability by examining the effect of key variables in extreme environmental contexts, especially in wetlands and at high altitudes, and by developing and refining models of optimality that anticipate variability as the local expression of general evolutionary ecological principles. Research on intraregional and ethnic variability has lagged behind—the former because it is said to be costly, the latter because it is problematical in theory.

KEY WORDS: Great Basin archaeology; regional variability; ethnicity; wetlands research; alpine research; optimal foraging theory; Numic; Fremont.

INTRODUCTION

In comparison to their counterparts elsewhere in North America, Great Basin archaeologists worry less about cultures and culture history and spend most of their time attempting to understand how aboriginal populations made use of natural resources in an environment that was sometimes harsh and often unpredictable, and how this shaped their activities and social behavior. The Great Basin is held to be ideal for this sort of thing, being a simple place of simple peoples (e.g., Murphy, 1970, pp. 152–154). In truth, however, the Great Basin is anything but simple; and the closer one looks, the less simple it gets. There is bewildering vari-

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ation on every scale in every dimension. Most Great Basin archaeologists are convinced that the current methodology permits one to simplify and make sense of this variability in a way that illuminates general processes. For these individuals variability is something to be used—to be worked with and worked around. This is not an easy task. As the prevalent emphasis on cultural ecology indicates, it requires hard choices about the variability that is important and the variability that is not. These decisions must, in turn, be operationalized through carefully crafted research designs that pose problems in terms general enough to be of theoretical relevance and yet specific enough to engage patterns detectable in the archaeological record. The design of recent Great Basin research demonstrates increasing sophistication in dealing with variability this way, through informed choices of research context and the development and application of unifying theory. The future challenge lies in dealing with all the variability that has been ignored in the process.

RESEARCH CONTEXT

One response to the problem of variability is to narrow the scope of empirical inquiry in such a way that many key variables are held constant. This sharpens the play of the remaining variables within the limits imposed by the constants that define the experimental setting. Many Great Basin archaeologists interested in human ecology have taken this approach and implemented it through detailed investigations in special contexts they regard as particularly sensitive to the forces that shaped prehistoric adaptation. Much of this effort has centered on two distinctive natural settings: low-elevation wetlands and high-elevation alpine steppes. These are contrasting environmental extremes where one has good reason to expect extreme (and contrasting) adaptive responses. In both cases the research maintains a broad perspective by asking how aboriginal activities in these places articulated with larger regional patterns.

Wetlands Research

Wetlands/marsh research is being actively pursued in Oregon (Aikens and Greenspan, 1988; Cannon *et al.*, 1990; Greenspan, 1990; Oetting, 1990a, b; Wilde, 1989), Utah (Hunter, 1991; Janetski, 1990a, b; Simms, 1990; Simms and Heath, 1990), and Nevada (Kelly, 1985, 1988a; Raven, 1990; Raven and Elston, 1988; 1989; Rusco and Davis, 1987) [for a history of Great Basin wetlands research see Fowler and Fowler (1990)]. In Nevada and Utah, investigators have frequently benefited from fluctuating water levels that have fortuitously exposed sites previously unavailable for study (e.g., Baker and Janetski, 1992; Raymond and Parks, 1990; Simms *et al.*, 1991). Wetlands research more generally has also benefited from the timely publication of ethnographic accounts of lake/river/marsh-adapted groups that were neglected in the more familiar summary treatments of Steward (e.g., 1938) and others (Barter, 1990; Evans, 1990; Fowler, 1989, 1990a, b; Janetski, 1991; Raymond and Sobel, 1990; Rhode, 1988; Sutton, 1988; see also Aikens and Greenspan, 1990).

The avalanche of data thus acquired both fuels and feeds on a continuing controversy regarding the role of marshes in Great Basin adaptive systems. In one view, marshes are resource-rich oases that supported enclaves of sedentary or semisedentary wetland specialists distinct in many respects from the mobile resource generalists often pictured as more typical of the Great Basin (Heizer and Napton, 1970; Jennings and Norbeck, 1955; Madsen, 1982; Madsen and Berry, 1974). Thomas (1985, pp. 18–20, 1990a) terms this the limnosedentary hypothesis. The alternative limnomobile hypothesis portrays marsh resources as less attractive to humans than those available in more mesic environments. Individuals subscribing to this view see marshes as important primarily during relatively short periods, when a few high-quality marsh resources are momentarily superabundant (e.g., Madsen, 1989b; Madsen and Kirkman, 1988) or when resources are scarce elsewhere (Kelly, 1988a, pp. 12–13, 1988b; Thomas, 1985).

Ever sensitive to the importance of variability, both parties now concede the bankruptcy of the dichotomy (Kelly, 1990; Madsen, 1988; Madsen and Janetski, 1990; Simms, 1988; Thomas, 1990a) but, for the most part, remain steadfast in the more general tenets of their original positions. This simply shifts the debate to a more nebulous contrast between "limnogood" and "limnobad" (Madsen, 1990; Madsen and Janetski, 1990; Thomas, 1990a). The limnogood camp (e.g., Madsen, 1988; Raymond and Parks, 1990; Simms, 1988) currently relies heavily on the contingency models of optimal foraging, in which resources are ranked and used relative to momentary rates of energetic yield for time spent procuring and processing them (i.e., excluding search time), and patches (of resources) are ranked and used according to rate of energetic return for time spent finding, procuring, and processing resources [i.e., including search time (cf. Bettinger, 1991b, pp. 84–90)]. These individuals cite experimental data suggesting that certain marsh resources (e.g., *Scirpus*) are high-ranking (Jones and Madsen, 1991; Simms, 1987). Since these high-ranking taxa are often relatively abundant in marshes, it follows that human groups optimizing momentary rates of energetic return should use those taxa and the marsh "patch" itself intensively whenever available. This is said to be consistent with archaeological evidence placing the earliest occupation of the Great Basin in marsh settings, i.e., at Danger and Hogup Caves (Fig. 1) (Madsen, 1982, p. 213; Madsen and Berry, 1974; Simms, 1988, p. 423; cf. Aikens, 1970; Jennings, 1957; see also Willig *et al.*, 1988). The interpretation is plausible enough yet difficult to reconcile with coprolites recovered from the early sites in question. These are dominated by species argued on the basis of aforementioned experiments to be "low-ranking" (e.g., *Allenrolfea*; but see below); the "high-ranking" species said to account for marsh occupation (e.g., *Scirpus*) are scarcely present (Fry, 1976). More convincing is Raven's (1990; Raven and Elston, 1989) regional survey, which demonstrates a general

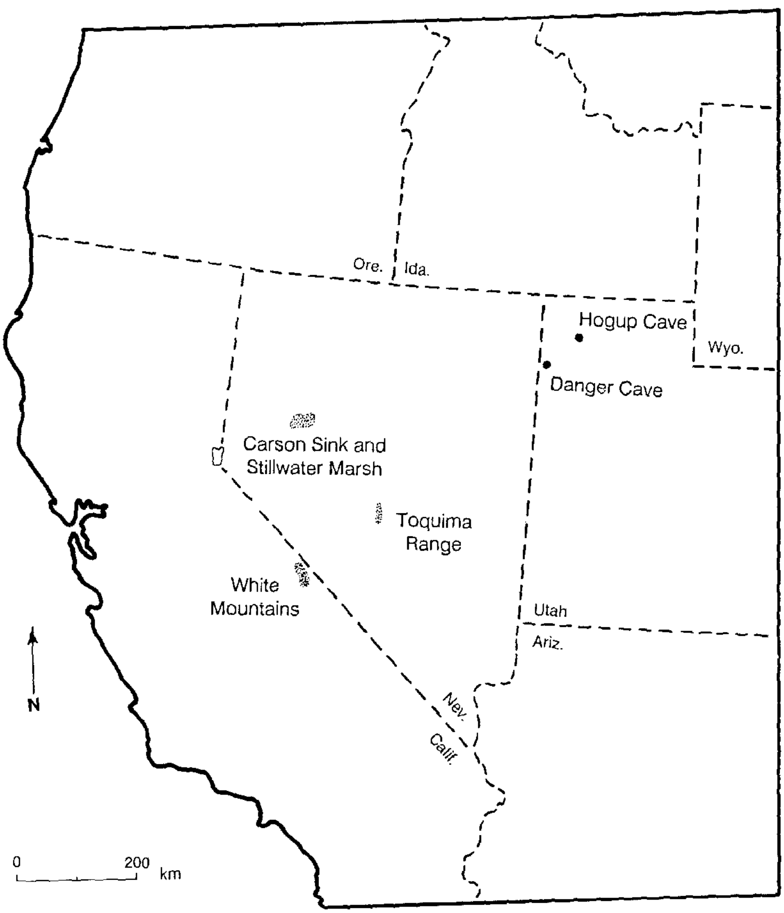

Fig. 1. Location of major sites and site complexes specifically mentioned in the text.

correspondence between the distribution of "high-ranked" resources and the intensity of residential site use in the Stillwater Marsh.

The limnobad camp also employs optimal foraging theory (e.g., Kelly, 1988, pp. 12–18), noting that most marsh resources produce low returns relative to the time spent gathering and processing them. Greater weight, however, is placed on archaeological evidence generated by regional survey and excavation in the Carson Sink, Nevada, which is held to document sparing residential use of marshes (Kelly, 1985, 1988a, b, 1990; Thomas, 1985). This is interpreted as occurring relatively late in time in response to deteriorating climate or encroachment by Numic-speaking peoples (see below). This argument, too, does not square with important evidence. Most notably, it is contradicted by a spectacular series of residential sites recently exposed in the Stillwater Marsh, Nevada, argued to represent sedentary or semisedentary villages (Raymond and Parks, 1990). These clearly document greater residential marsh use than some in the limnobad camp had predicted but their meaning relative to the more basic issues involved in the wetlands debate remains uncertain. If they are permanent villages, they are very odd indeed. The lithic assemblages (Elston, 1988; Kelly, 1990, p. 271). are depauperate enough to suggest that the village inhabitants failed to develop regular means for obtaining raw material for stone tools, something one normally expects in sedentary settings—especially where marsh resources are processed in quantity. Too, the Stillwater sites are temporally "out of phase" with the regional record of occupation, particularly in late prehistoric time, when they seem to have been abandoned (Kelly, 1988a, p. 15; Raven, 1990, Fig. 71; Raven *et al.*, 1988, p. 436). This may simply reflect a shift of marsh location (Elston *et al.*, 1988, p. 372), but it is perhaps not a coincidence that marsh localities in Utah also exhibit basic changes of use in the late prehistoric period (Baker and Janetski, 1992; Janetski, 1990a, b; Simms, 1990).

Alpine Research

Fundamental discord is conspicuously lacking in alpine research, partly because there are simply fewer investigators working in fewer localities. It is also because (compared to wetlands) alpine environments are simpler, more isolated, less variable over time, and depositionally less complex, making the patterns easier to interpret. As a result, the archaeological records themselves are clearer, more finely documented (e.g., Bettinger and Oglesby, 1985; Grayson, 1986, 1991; Grayson and Livingston, 1989), and, as it happens, strikingly similar. Most of the data derive from surveys and excavations conducted between 1981 and 1989 in two of the highest mountain ranges in the Great Basin, the White Mountains of eastern California [4343 m (Bettinger, 1990, 1991a)] and the Toquima Range of central Nevada [3640 m (Thomas, 1982)]. These studies disclosed the presence of more than a dozen intensively occupied residential bases, termed villages, at or above the modern treeline and ranging in elevation from 3200 to 3850 m. The villages display unexpectedly large and complex archaeological assemblages that attest to warm-season, high-altitude occupation lasting at least 1 month, and quite possibly more than 2, by nuclear families or multifamily social units such as bands. This pattern is undocumented ethnographically and its presence had not previously been suspected in the Great Basin [for a history of Great Basin alpine research, see Thomas (1982)].

In addition to villages, the alpine zones of the White Mountains and Toquima Range also display numerous rudimentary rock walls, blinds, and sparse lithic scatters dominated by highly fragmented projectile points and bifacial knives taken to represent the short-term presence of hunters or hunting parties pursuing mountain sheep, the only common seasonally present ungulate. These materials appear to predate the villages, which, in both the Toquima Range and the White Mountains, appear relatively late in time, between A.D. 200 and A.D. 600. The temporal succession of the two patterns clearly documents a major change in the aboriginal use of the alpine zone from short-term, logistical mountain sheep procurement to intensive residential hunting and gathering. The investigators link the shift to population increase that occasioned more intensive use of more marginal resources (plants) in more marginal environments (alpine communities) (Bettinger, 1991a; Grayson, 1991; Thomas, 1982).

There is less unanimity regarding the explanation for the demographic changes said to account for the near-contemporaneous appearance of alpine villages in these two widely separated ranges. One possibility is that they are essentially independent and reflect the cumulative effect of long-term population growth in each area (Grayson, 1991; Madsen, 1992). The coincidence in timing may be due to large-scale changes in climate affecting both areas (Bettinger, 1991a, p. 672). Alternatively, it can be argued that the two events are historically connected to spread of Numic-speaking peoples from southeastern California northward and eastward into the Great Basin, which is believed by many to have occurred at roughly the time that alpine villages appeared in each range (Bettinger, 1991a, pp. 673–675; Bettinger and Baumhoff, 1982; Thomas, 1982). That the theoretical argument held to account for the Numic spread can be interpreted in different ways has led to some disagreement regarding the degree to which its predictions match patterning observed in the rather large faunal assemblage obtained from the White Mountain sites (Grayson, 1991; Madsen, 1992).

UNIFYING THEORY

A second strategy for coping with variability is through unifying general principles. This differs from the first strategy in the sense that it actively seeks (rather than limits) contextual variability. This is essential to illustrate the explanatory power of the general principle in widely varying circumstances. That, of course, was the appeal of Jennings Desert Culture/Archaic concept (Jennings, 1957, 1964, 1968), which united locally varied exploitative patterns under an explanatory umbrella in which flexible and situationally appropriate subsistence behavior resulted in adaptive efficiency. In the past decade, Great Basin archaeologists have advocated an approach that reworks essentially the same idea with principles drawn from evolutionary ecological theory, most notably the diet breadth and patch choice models of optimal foraging theory (Madsen and Janetski, 1990; Madsen and Jones, 1988; O'Connell *et al.*, 1982; Raven and Elston, 1989; Simms, 1987; Tucker *et al.*, 1992). As noted above, these models figure prominently in debates surrounding wetlands and alpine research. The same principles, or transformations thereof (cf. Bettinger, 1987, pp. 136–137, 1991b, p. 108), underlie much of the more specialized research being conducted with lithic assemblages (e.g., Elston, 1990) and archaeofaunas (e.g., Grayson, 1988, 1989; Dansie, 1987; Lyman, 1988; Sharp, 1989). Variability itself is a key issue here in at least two regards, one related to inferences that are made about the general nature of forager behavior and the other to the use of experimentally determined resource return rates to infer prehistoric subsistence practices.

With respect to the first, much is made of the fact that in the diet breadth and patch choice models, optimal behavior is dependent on momentary circumstances, which constantly vary (e.g., Madsen, 1988, pp. 415, 418). From this it tends to be generalized that foragers lack commitment to particular tactics and simply do what is optimal at a given moment (Kelly, 1990, pp. 262–271; Madsen, 1988, p. 418; Simms, 1988, pp. 422–423). The assumption leading to this expectation, however, is central only to optimal foraging models of contingency form, in which choices are determined by expected momentary rates of return. These models are fine if one is willing to assume that the forager is not subject to additional constraints, in the presence of which contingency models become problematic.

To see this, imagine a human forager who must, on a given day, obtain enough calories from a fixed area to avoid starving. Here the forager is required to optimize the rate at which calories are obtained subject to specific temporal, spatial, and caloric constraints. It is easy to imagine that a "contingency" forager, whose only thought is to optimize the rate at which calories are obtained moment by moment, might completely cover the allotted area in less than the allotted time without acquiring the needed quantity of calories—and be forced to make a second pass to make up the difference. Such a strategy will be less efficient than one in which the forager harvests a range of resources broad enough to satisfy caloric requirements in the course of a single pass, even though this requires the harvest of "suboptimal" resources and results in suboptimal momentary rates of energetic return.

There is nothing wrong with contingency models; they clearly have their place. It is clearly a mistake, however, to generalize about hunter-gatherer behavior on the basis of the rather specialized assumptions that underlie contingency models. The impiications are potentially far-reaching because we know that Great Basin hunter-gatherers were constrained in a number of important dimensions—caloric requirements, foraging space, foraging time, and ability to transport resources, to name a few. In each case, these can be expected to lead to optimal behavior at odds with the predictions of contingency models. In general, one expects that such constraints become increasingly salient as hunter-gatherer systems become less forager-like and more collector-like (Binford, 1980; Thomas, 1988, pp. 579–580). That follows because forager-like systems tend to produce immediate returns on foraging investment and, in that sense, approximate fairly closely the assumptions of contingency models. Collector-like systems, on the other hand, incorporate many long-term, delayed-return strategies, implying a host of constraints that tend to make the expectations of contingency models unrealistic. Noncontingency models (e.g., linear programming) seem more appropriate here.

Several investigators have already begun to address these sorts of problems through predictive work on transport and travel constraints (Jones and Madsen, 1989; Metcalfe and Barlow, 1992; Rhode, 1990a). This research bears in a direct way on the wetlands debate, because wetlands restrict mobility in a number of ways (Madsen, 1988, p. 417), being more difficult to traverse than drylands (e.g., Raven, 1990, p. 11). This may partly explain the unusual frequency of arthropathy (diseased joints) in the Stillwater skeletal population (Brooks *et al.*, 1988, 1990). It is quite possible, in any case, that soft footing adversely affects foraging returns for individual marsh resources and the wetlands patch itself. Prehistoric foraging returns may have been lower than has been suggested by return-rate experiments (see above), since the latter have been conducted only for short intervals (Simms, 1987) on hard ground (Jones and Madsen, 1991), so that mobility was not a factor. This leads to a more general observation regarding the use of experimentally derived return rates to predict and interpret prehistoric subsistence behavior (see also Bettinger, 1991b, pp. 103–104).

The experiments that have been conducted with return rates (Fowler and Walter, 1985; Jones and Madsen, 1991; Simms, 1987) do provide important and critical insights regarding the relative costs of different resources and are obviously helpful to archaeological interpretation (e.g., Raven and Elston, 1989). The difficulty arises when investigators generalize about the ranking of a particular resource from a particular experimental design and assume that it applies wherever the presence of that resource can be inferred or documented archaeologically. For example, as noted earlier, experiments with pickleweed (*Allenrolfea*) produced very low returns, from which it is inferred that the species is low-ranking and that its prehistoric use (e.g., in Danger and Hogup Caves) indicates substantial diet breadth (i.e., because low-ranked species are being used). The problem here is that aboriginal groups often harvested the same resource in a variety of ways in a variety of circumstances, at markedly different rates of return. As noted just above, a high-ranked resource that produces high returns for short intervals on dry ground may become low-ranked (in terms of return rate, at least) when obtained over longer intervals on wet ground. Special circumstances can just as easily "turn these tables" and transform a low-ranked resource into a high-ranked one, particularly in wetlands.

Madsen and Jones (Madsen, 1989b; Madsen and Kirkman, 1988; Jones and Madsen, 1991) have shown how in wetland areas water occasionally winnows grasshoppers and crickets into windrows, where they can be harvested in quantity at return rates that are orders of magnitude larger than those obtained when they are hand-picked individually. Windrows more obviously facilitated the native procurement of the tiny brine fly [*Ephydra hians* (Sutton, 1988, pp. 44–49)]. Many marsh plant seeds seem equally subject to this phenomenon. That, at least, is the experience of wetlands conservationists, who have long turned windrows to their advantage when collecting seed for use in marsh propagation (Martin and Uhler, 1939, p. 105). It is thus thinkable that the pickleweed seed, which dominates the Danger and Hogup Cave middens and coprolites, derives from just this sort of high-return windrow procurement, as recounted in a Western Shoshoni tale (Sutton, 1989; cf. Miller, 1972, pp. 44–46). That, however, would dramatically change how we interpret diet breadth at those sites, which currently furnish the basis for inferences suggesting a broad-spectrum pattern commonly incorporating low-ranked resources throughout the Archaic in the eastern Great Basin (Simms, 1983; cf. Madsen, 1992). If high-return windrow procurement is represented, on the other hand, the pattern immediately becomes one of relatively narrow diet breadth dominated by high-ranking resources.

The larger lesson here is that often it is the mode and circumstance of procurement (in this case, windrows vs. hand-picking), rather than the resource (grasshoppers, crickets, or pickleweed), that primarily determines return rates. In that sense, wetlands and individual wetlands resources can be either high-ranking or low-ranking, depending on these circumstances. This suggests that in our experiments, theorizing, and fieldwork, we need to pay as much attention to how resources are being procured (and processed) as to what is being procured.

VARIABILITY IGNORED

The easiest way to cope with variability is to ignore it. No one openly advocates this, of course, but as we observed at the outset, everyone does it, and assumes the privilege of doing so, because otherwise nothing would get done. The privilege is abused, however, when used as an excuse to ignore variability that is easily controlled and conceded to be important as a matter of accepted theory. Such abuse is evident in the current disregard for field programs that engage functional variability at the regional level. Abuse also occurs when variability is ignored simply because it is difficult to reconcile with current theory, especially when this is accompanied by reluctance to develop new theory and method to manage that variability. Great Basin archaeologists seem too ready to respond in this way to variability potentially explicable in terms of ethnicity.

Regional Variability

Steward (1938) was perhaps the first to point out in a systematic way how many aboriginal Great Basin peoples had compensated for the marginal productivity of their environment through seasonal movement. Jennings, in various versions of the "Desert Culture/Archaic" hypothesis (Jennings, 1957, 1964, 1968; Jennings and Norbeck, 1955), convincingly argued that this pattern extended back to the earliest occupation of the Great Basin. Acceptance of that idea, and its logical implication that such patterns could not be studied by excavating "type sites," no matter how rich (Thomas, 1973), caused a generation of Great Basin archaeologists to conduct, during the late 1960s and 1970s, probabilistic surface surveys aimed at documenting regional subsistence-settlement patterns (Aikens *et al.*, 1982; Bettinger, 1977; O'Connell, 1975; Thomas, 1973; Weide, 1974).

Inexplicably, although regional subsistence-settlement patterns remain a major focus of interest at the conceptual level (e.g., Bettinger, 1989, pp. 342–343; Fowler, 1982; Madsen, 1989a; Madsen and Jones, 1988), the probabilistic regional survey—the archaeological means most appropriate to the study of such phenomena—is today not generally employed [important exceptions are the studies by Delacorte (1990), Kelly (1985), Leech (1988), Rhode (1987, 1990b), and Thomas (1988); see also Cannon *et al.* (1990, p. 179)]. As a result, Great Basin archaeology increasingly lacks an empirical basis for implementing its regional theory. The strategy continues to be used subregionally, especially in cultural resource management and academic programs that target communities or landforms (e.g., dry lakebeds, marshes, alpine steppes) too large to be studied *in toto* (e.g., Bettinger, 1991a; Jones and Beck, 1990; Raven, 1990). It is important to do this kind of work, just as it is important to excavate single sites. The problem emerges when we obtain a local record and have no reliable basis for deciding how it fits in a larger regional system. That is all the more lamentable because the technical quality of excavation, as measured by the amount of data we actually acquire and can control, has increased manifoldly (e.g., Basgall and McGuire, 1988; Hughes, 1989; Elston and Budy, 1990; Madsen and Kirkman, 1988; Madsen and Rhode, 1990; Metcalfe and Heath, 1990; Raven and Elston, 1988; Simms, 1989; Simms and Heath, 1990; Thomas, 1988; Zeier and Elston, 1992), as has our understanding that these data can be understood only in regional terms (e.g., Beck and Jones, 1990; Bettinger, 1989; Jones and Madsen, 1989; Metcalfe and Barlow, 1992; Rhode, 1990b; Wilke and McDonald, 1989).

Beck and Jones (1992) argue that archaeologists currently avoid regional surveys because they generate too much data, i.e., too much variability. That, however, depends on the kind of variability one is trying to measure and the level of precision at which one needs and wishes to measure it. Data, whether generated by excavation or regional survey, are no more or less complex or variable than our theories make them. If, as Beck and Jones argue, regional data are deemed too complex, that says a great deal about our control of regional problems. In the final analysis, the value of the regional survey lies as much in the kind of thinking as in the kind of data it generates. It is no coincidence that the most productive research program in the history of Great Basin archaeology, initially christened the Reese River Ecological Project (Thomas, 1969), began with problem-oriented regional survey and continues to operate largely within a conceptual framework established by that survey (e.g., Thomas, 1988). Here regional variability does not complicate analysis; it is the subject of analysis.

Ethnic Variability

Prehistorians frequently simplify and explain variability in the archaeological record in terms of large-scale patterns said to result from the behavior of culture-historically distinct social or ethnic units. The practice, however, has never been popular in the Great Basin (in modern times at least) because it requires inferences about the behavior of social entities (e.g., ethnic groups), the presence of which is neither assumed nor explained in the generally accepted framework of human ecology that guides most research and interpretation (O'Connell *et al.*, 1982, pp. 228–231). Despite this, it is generally believed that units approximating ethnic groups must have been present, and in two cases the empirical data periodically are deemed compelling enough to hazard speculation.

Scholars generally acknowledge the presence, centered mainly in Utah, of a distinct cultural entity termed the Fremont, characterized by horticulture and distinctive architecture, ceramics, basketry, footgear, and art (Madsen, 1989a). Fremont evidently developed *in situ* from an Archaic base around A.D. 500, incorporating many elements borrowed from surrounding regions, mostly the Southwest and Plains. The disappearance of Fremont around A.D. 1450 is widely laid to a combination of deteriorating climate, which made horticulture nonviable, and the advance of a second suspected ethnic unit composed of Numic-speaking peoples directly ancestral to ethnographic Great Basin populations. The Numic advance, or spread, is believed to have begun in eastern California, where, according to one hypothesis (Bettinger, 1989, pp. 343–347; Bettinger and Baumhoff, 1982, 1983; Young and Bettinger, 1992; see also Aikens and Witherspoon, 1986; Sutton, 1986), ancestral Numic groups developed an intensive adaptive strategy emphasizing the use of marginal resources, e.g., small seeds. This is said to have permitted them to expand into the Great Basin at the expense of groups embracing less intensive, more specialized adaptations. As noted above, alpine and wetlands research furnishes substantial evidence for late prehistoric adaptive change in many parts of the Great Basin that can be read as supporting the model (Bettinger, 1991a; Janetski, 1990a; Kelly, 1988a; Simms, 1990; Thomas, 1982). The same data, however, can also be explained in terms of demographic and climatic forces acting more locally—and therein lies the problem: How does one distinguish local adaptive change from change that is the result of ethnic replacement? By the same token, but in more general terms, what distinguishes ethnic units from adaptive ones?

The Fremont, the Numic, and other such entities are typically regarded as problematic because they are difficult to identify archaeologically, but that is really not the problem. They are difficult to identify because we have no theory that tells us what it is we are looking at and, thus, what it is we should be looking for. This is amply evident in the morass of definitional debate surrounding the Fremont (cf. Adovasio, 1986; Madsen, 1989a; Simms, 1990), the identifying traits of which are mostly stylistic and imply patterned behavior that transcends simple ecology. Simply put, the Fremont is a cultural unit in search of a theory.

Defining Numic ethnicity largely in terms of adaptation (cf. Bettinger and Baumhoff, 1982, 1983) circumvents that problem but the issue immediately reemerges in adaptive disguise in relation to the hypothesized Numic spread (Madsen, 1992; Simms, 1983). The issue here is how Numic groups managed to remain adaptively (and thus ethnically) distinct from groups such as the Fremont with whom they came in contact as they advanced. The contingency theorists argue that it ought to be relatively easy for hunter-gatherers to shift from one "strategy" to another through tactical "fine-tuning" as momentary circumstances change (e.g., Simms, 1986). Accordingly, any initial adaptive advantage enjoyed by advancing Numic groups would have been countered by appropriate adaptive responses on the part of groups threatened along the front (Madsen, 1992; Simms, 1983). In the Numic spread hypothesis, on the other hand, it is argued that Prenumic and Numic adaptive strategies can be regarded as distinct local adaptive "peaks" in an adaptively complex landscape characterized by several such peaks separated from each other by suboptimal "valleys." If adaptive behavior and change are informed by momentary rates of return, as in contingency models, movement from the Prenumic to the Numic peak would have been discouraged by the decrease in adaptive efficiency entailed in the transition.

The usual counter to the above is that Prenumic groups, able to observe and copy the Numic strategy, would have simply jumped from the Prenumic peak to the Numic peak, avoiding the valley between. The argument is quite sensible but represents a fundamental departure from the basic assumptions of evolutionary ecology because it implies that adaptive behavior is, at least in part, socially acquired, and not merely determined by individuals whom selection has programmed to act in accord with objective measures of utility (e.g., foraging return rates). Recognizing the importance of social transmission requires that we take into account the full range of such processes, including enculturation, which often lead to symbolic and group-level behaviors at odds with evolutionary ecological expectations (Bettinger, 1991b, pp. 181–211; Boyd and Richerson, 1985). This opens the door for a much more complicated scenario in which what we call "adaptive strategies" are parts of complexly coordinated patterns of behavior that extend far beyond subsistence to encompass a broad range of social, political, economic, and ritual activity—in short, the whole of cultural behavior.

Coordination of these various spheres into a more or less workable system is the likely means by which populations reach local peaks in given adaptive landscapes. Under such conditions, rational choice is highly constrained by the nature of the coordination, which is in turn mediated by a variety of symbolic behaviors connected with group (ethnic) identity, e.g., language. Such behaviors are inherently resistant to change, at least in part because they are symbolic and not subject to objective evaluation in terms of intrinsic utility. Viewing adaptive strategy as part of a larger package of symbolically coordinated cultural behavior makes it easier to understand the difficulties Prenumic groups would have faced in attempting to respond to the challenge posed by the Numic advance. It is quite thinkable that many specific Numic innovations were adopted by Prenumic groups who were incapable of using them to full advantage given the nature of their existing system. The recent history of attempts to modernize "primitive peoples" is littered with such examples and the pattern seems equally applicable to cases of cultural competition.

IMPLICATIONS

Whether the Fremont is or is not an ethnic group or whether the Numic spread hypothesis is right or wrong is really not at issue here. What is at issue is whether Great Basin archaeology is finally ready to move beyond Desert Culture thinking. In point of fact, the simpler models of human ecology, in which time and calories are all that matters, have been pushed close to their limits, although, as we have seen, there is a good deal of very interesting mop-up left to be done. We are all aware that science necessarily proceeds by small steps and that this sometimes means that we must be content to work with simple, unrealistic models. There comes a point, however, when we either resign ourselves to the fact that our "cutting-edge" research should consist of fine-tuning the simple, unrealistic models or face up to the difficulties of developing fundamentally different, and potentially better, models. I suspect that down deep most Great Basin archaeologists believe that there is more to human behavior than time and calories, and that somehow such things as style and ethnicity do matter. The question is whether we want to go on ignoring behavioral variability of this sort or do something about it. Rumblings on several fronts (Beck and Jones, 1992; Simms, 1990; Thomas, 1990b) suggest that Great Basin archaeologists are preparing for the latter course of action.

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