Some Thoughts on Evolution, Ecology, and Archaeology in the Great Basin

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Some 30 years ago, we opined that culture history and descriptive ecology, the two analytic frameworks then being used to explore Great Basin prehistory, could not in the end explain it (O’Connell et al. 1982). We agreed that both were important: culture history as the approach best suited to basic description of the archaeological record; descriptive ecology as the simplest means of documenting the relationship between certain aspects of that record and the nonhuman environment in which it was situated. Missing from the enterprise in our view was a reliable way of addressing the underlying “why” questions: why the record took shape as it did, why it articulated with its environment as it did, and why the past human behavior inferred from those lines of evidence varied as it did.

Like many archaeologists of that time, we thought that a materialist approach, one that paid little attention to speculation about the motives, ideas, or agency of past human actors, was the best way of tackling these questions. We were inspired by the work of Julian Steward but recognized the limits of cultural ecology.

We argued that the explanatory framework of behavioral ecology provides the tools to move beyond culture-historical description and mechanistic responses of culture to an externalized natural environment. The emerging discipline of behavioral ecology offered a way of transcending these limits, one that we thought would allow us not only to test Steward’s model and other ethnographically grounded formulations like it but also to develop similar arguments about human behavior in environmental settings significantly different from those of the nineteenth-century Great Basin.

We wanted to know why culture was at times plastic and at other times resistant and why cultures could be behaviorally homogeneous or fonts of behavioral diversity. We were, and remain, well aware of the forces in anthropology and broadly across our culture that forbid any quest to know human behavior in a scientific sense and especially in a deterministic sense. We decided to treat the matter as an empirical issue.

Looking back, we see that the essay reflects a certain missionary zeal that probably accounts for the mixed reaction it received among some of our colleagues, notably some of our distinguished elders. We are less inclined toward proselytizing now, but our passion for the approach remains intact.

Here we offer “further thoughts” on the topic in honor of Don D. Fowler, a true historian of anthropology. Our retrospective places evolutionary ecology and its subset behavioral ecology in intellectual context, and we share a glimpse of how we arrived at the position we took in 1982. We selectively review progress made over the last three decades but note that more thorough reviews are available elsewhere (e.g., Bird and O’Connell 2006; Zeannah and Simms 1999). The next iteration of behavioral ecology in Great Basin archaeology is already upon us, as archaeologists investigate the late Holocene sea change of intensification and cultural complexity. The debate over “prestige hunting” is one element of this investigation, and while we find it a productive line of research, we also see some familiar pitfalls. We employ the prestige debate to offer some reminders about key concepts in behavioral ecology that may help to resolve some perceived differences of interpretation and move the research forward.

In Pursuit of a Great Basin Past

Great Basin archaeology, as we learned it, was dominated by the culture-historical approach to data collection and analysis. Broadly speaking, the exercise involved excavating sites containing rich arrays of material remains; describing certain of those remains in terms of formal artifact types; tracing the distribution of those types through time and space; identifying readily bounded, co-occurring sets of types as archaeological “cultures”; and accounting for changes in their composition and distribution by reference to past movements of people, ideas, or both. Typical Great Basin examples include Marwitt 1970 on the Fremont, Jennings 1964 on the Desert West, and Heizer 1956 on the Lower Humboldt Valley. The approach was unabashedly inductive; its results, narrowly historical.

Lewis Binford (1962) and other proponents of the New (later, processual) Archaeology argued that the discipline could
do much better than this. They reckoned that the material re-
cord represented a far more complete body of information on
past human behavior than the culture historians had imagined;
that it could be tapped most effectively through systematic, com-
prehensive approaches to data collection and analysis; and that
properly tackled it offered the best available basis for developing
and testing explanations for long-term changes in the human
condition. Walter Taylor (1948) presented a similar argument 14
years earlier, but the spirit of the times was against him. Binford,
writing in the 1960s, found a more receptive audience, at least
among the younger set. Some of his students and others farther
afield pursued these leads in a series of influential case studies.
Based on fieldwork in the central Great Basin, David Hurst
Thomas's (1973) archaeological evaluation of Steward's model of
seasonal transhumance was among the very best of these efforts.

As phrased initially (and as the subsequent shift in title to
"processual" implies), the New Archaeology's goals were more
than merely methodological. Binford himself was a protégé of
the archmaterialist Leslie White; many of his archaeological
followers found similar inspiration in the works of Steward and
Marvin Harris. Countering the established Boasian tradition
in American anthropology, all denied the proposition that an
accounting of culture history was in any sense an explanation of
that history but, instead, saw history as a phenomenon that itself
demanded explanation.

Despite the rhetoric, their attempts to achieve this goal
founded on the prior problem of reconstructing those aspects
of past behavior reflected in archaeological remains. Absent a
relevant ethnographic model of the sort that made Thomas's ex-
ercise so successful, that basic difficulty soon became the primary
focus of attention. "Middle-range" research on site-formation
processes and their behavioral implications preoccupied the field
(Binford 1977; Schiffer 1987). Concern with the ultimate goal of
explaining variation in past human behavior faded accordingly.
As Barbara Price trenchantly observed,

What is surprising... is that the "new archaeology" begins
its downward deductions at so relatively middle a level, pre-
cluding significant generalization and producing a corpus of
work remarkable (at least in retrospect and given its initially
revolutionary program) for its intellectual conservatism
[1982:714].

By the late 1970s, we saw this as a significant problem and were
ready to embrace a theoretical framework that would help re-
gain purchase on broader questions about cultural variability.
We were of course not alone in this.

Growing Points:

Behavioral Ecology and Human Behavior

Darwin's theory of natural selection sought to explain the emer-
gence of morphological variation without reference to a Creator.
Beginning in the 1950s, ethologists Niko Tinbergen, Konrad
Lorenz, and others extended this "selectionist" thinking into the
realm of behavior. The disciplines now called evolutionary and
(its subset) behavioral ecology were among the results (Krebs
and Davies 1997). Tinbergen (1963) in particular drew a crucial
distinction between "proximate" and "ultimate" causation in
explanation. Proximate explanations are concerned with how
behaviors work, and the concept is recognizable to anthropologists
as functionalism. Ultimate explanations focus on why behaviors
arise in the first place—questions that echo anthropology's inter-
est in explaining the development of cultural form and variability
in general (that is to say, nomothetic) terms.

One of our sources of inspiration, evolutionary biologist Eric
Charnov, often illustrated the difference between proximate and
ultimate causation with the question, "Why do birds fly south
for the winter?" As he said, proximate explanations speak to
physiological changes in birds during the fall season, the mecha-
nisms of bird navigation, and so forth. They show how migration
works. None of these explains why some birds evolved to migrate
seasonally while others did not. "Who was the first bird to fly
south?" Charnov would ask. Ultimate explanations focus on the
circumstances that select for migration from among the array of
behaviors present in an existing population.

Anthropologists can apply the proximate/ultimate distinc-
tion to many of their own questions. For instance, archaeologists
generally describe the origins of agriculture in largely proximate
terms: as a historical process and as a system that functions to
achieve certain purposes. Ultimate explanations aim at why
agriculture was developed, adopted, or in some cases rejected
when and where it was. Perhaps surprisingly, attempts at such
explanations remain few and far between (Kennett and Winter-
halder 2006).

Our excitement about evolutionary and behavioral ecol-
ogy was heightened by a 1979 visit to Utah by biologist George
Williams, author of the landmark book Adaptation and Natu-
ral Selection: A Critique of Some Current Evolutionary Thought
(1966). Williams critiqued the progressive view of adaptation
and was one of a cadre of evolutionary biologists, including John
Maynard Smith, John Krebs, Robert MacArthur, Robert Trivers,
William Hamilton, and others, who pioneered the application of
selectionist thinking to the study of animal behavior. They
argued that selection operates on the level of the individual or-
ganism, not on whole species. They saw adaptation as a process
that favors the persistence and spread of certain behaviors at the
expense of others, not as the product of wholesale transforma-
tions toward "better" species. They recognized that virtually all
organisms (especially members of the same species) deal with
conflicts of interest, regardless of whether they are sentient or
not. Most important for anthropology, they saw adaptation in
Darwinian terms, rather than in the tautological, progress-laden
dictums of Herbert Spencer that are still so pervasive among so-
cial scientists.

Nicholas Blurton Jones, a former student of Tinbergen's,
was among those who carried this discussion into the human
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arena. In an edited volume with the telling title *Growing Points in Ethnology*, Blurton Jones (1976) drew attention to anthropology's purported interest in nomothetic explanations for variation in human behavior and to the fact that it was repeatedly thwarted in pursuit of that interest by fears that are deeply rooted in Western thought, particularly the fear of explanations that eschew reference to human intention in favor of attention to "mindless" material circumstance.

The same fear existed in the mind of Darwin, who was alarmed by his own early notebooks and sketches (1838–1844) where he subscribed to philosophical materialism, which he saw as a more heretical notion than evolution. Darwin even saw his materialism informing "the citadel itself—the human mind." Though Darwin felt that natural selection was a key to explanation, for human affairs he could only allow that "light will be thrown on the origin of man and his history" (Gould 1977:21). It is not the theory of evolution that bedevils Darwin in our culture but, rather, his materialism.

The tenacity of this cultural bias was evident in the visceral reaction to Leslie White half a century ago because he was a "materialist," who did not acknowledge that "it is people who make history" (Peace 2004:185–186). This cultural preconception continues; witness the widespread appeal of such phenomena as postmodern anthropology, postprocessual archaeology, agency theory, historical process, and the recent eclectic spasm dubbed "processual-plus" archaeology.

Blurton Jones wondered if an integration of behavioral ecology and ecological anthropology might break this impasse. He noted that work done under the banner of ecological anthropology had shown that adaptation is a concept relevant to the study of human affairs, and he speculated about the ultimate explanations that might lie behind that finding. His provocative observation caught our attention: "Surely we are not here dealing with the directing effects of natural selection? Or are we?" (Blurton Jones 1976:439).

**Behavioral Ecology in Great Basin Archaeology**

More than half a century ago, W.D. Billings (1953) drew attention to a characteristic common to many Great Basin scholars: their attraction to (some might say infatuation with) the region itself—the dry white playas and great salt lakes, the vast sagebrush oceans and rugged montane islands. For anthropologists, that appeal has additional, discipline-specific dimensions. Steward's cultural ecology was shaped both by his youthful experience of the land and its indigenous inhabitants (Kerns 2009, 2010) and by a deep interest in fundamental questions about the determinants of human behavior. For archaeologists, the unusually well-preserved records derived from dry cave deposits spoke not only to the Native experience of local environments and the changes they sustained over time but also to developing ideas about the basic nature of hunter-gatherer lifestyles and the proposition that they represented what many thought of as the fundamental human condition. For us, the intersection of austere landscape, rich archaeological record, detailed ethnohistory, and broad disciplinary relevance combined with the appeal of a newly emerging theoretical perspective to create an irresistible opportunity. Thirty years ago, the obvious point of entry was through foraging theory. Charnov and Orians (1973) had shown that simple mathematical models allowed one to explain key aspects of animal subsistence behavior in organisms as distantly related as fish and birds. By the time we drafted our 1982 essay, provocative ethnographic applications of these models had already been reported. Bruce Winterhalder (1977, 1981) had explored the utility of formal models of prey and patch choice among the sub-Arctic Cree. O'Connell and Kristen Hawkes (1981) had used the prey model as the framework for a hypothesis about the recent elimination of formerly important seed resources from the diets of central Australian Alyawara. Eric Alden Smith (1980, 1981) had employed the notion of foraging efficiency as a basis for predictions about the size of foraging groups among the Inuit.

Archaeological applications had appeared even earlier. Edwin Wilmsen (1973) appealed to Henry Horn's (1968) model of blackbird territoriality in speculating about group size and settlement location among Paleoindian populations on the Great Plains. John Beaton (1973) proposed that widely recognized but poorly understood changes in the composition of coastal California shell middens could be explained by reference to long-term, predation-driven declines in the availability of economically high-ranked prey and the consequently increased appeal of lower-ranked taxa. Frank Bayham (1977, 1979) employed a diet breadth model to understand changes in hunting patterns at the famed Ventana Cave, Arizona, and showed that the same model helped to account for similarities in Archaic patterns of faunal procurement in the Southeast, in the Midwest, and at Hogup Cave in the Great Basin.

In a widely read review essay in the Michael Schiffer-edited *Advances in Archaeological Method and Theory*, Great Basin archaeologist Robert Bettinger (1986) summarized these and other early applications of models from behavioral ecology to problems in anthropology in general and archaeology in particular. In a subsequent piece written with California archaeologist Martin Baumhoff (Bettinger and Baumhoff 1982), Bettinger used the concept of alternative subsistence strategies and competing "adaptive peaks" to propose a novel explanation for the displacement of resident Basin populations by migrating Numic speakers. In short, our polemic was drafted during a period of substantive exploration of behavioral ecology as a framework for the systematic study of human behavior.

Early archaeological applications of foraging models commonly founded on the paucity of quantitative data regarding the costs of acquiring various food resources and processing them for consumption. Once the potential of the approach had been illustrated by the case studies noted above, it was clear that ethnographic and experimental work designed to provide the
necessary data was essential. We gathered some of those data and shared the results at conferences and in publications in the early 1980s (e.g., Jones 1981; Jones and Simms 1980; Simms 1982). These data served as the basis for tests of Steward's model of ethno- graphic diet breadth (Simms 1984, 1987; Zeanah and Simms 1999). The surprising result: even when differences in men's vs. women's foraging goals were considered in the analysis, the diet breadth model indicated that small seeds should not have been part of the ethnographic diet. Only when one considered the storability of seeds, to be consumed in winter when other options were unavailable, did they enter the optimal diet.

Far from marking a "failure of optimal foraging theory," as some critics suggested, the exercise illustrated the capacity of the approach to move inquiry in productive directions. Indeed, further studies adding complexity to the modeling process followed in short order. For example, transport costs were calculated (Jones and Madsen 1989), and the differential transportability of resources was seen to hold implications for mobile vs. logistic settlement strategies (Rhode 1990).

Settlement patterns became a target of investigation, and this required the development of prey choice models that take resource abundance into account in order to model the proportional contribution of resources to the diet. The signal examples were studies in the Carson Desert of western Nevada by Christopher Raven and Robert Elston (1989), Robert Kelly (2001), and David Zeanah (2004). These investigators plotted the regional distribution of resource patches over the last two millennia, quantified return rates available from potential prey in each, assessed patch rank by reference to men's vs. women's foraging goals, and predicted the resulting distribution of archaeological sites and the character of associated assemblages.

As anticipated, subsequent analyses showed that late prehistoric residential site location on the desert floor was determined by women's foraging choices while special-purpose sites used mainly by men were more widely distributed across the surrounding terrain. Analysis of human skeletal remains exposed by flooding of the Carson Desert in the 1980s revealed differences in lifetime workloads consistent with the inferred settlement patterns. CT scans of humeri and femora indicated that women were significantly less mobile than men but engaged in substantial upper body work. Men's remains showed the effect of repeated long-distance movement over steep terrain (Larson and Kelly 1995).

Elston and Zeanah (2002) carried this line of research further by using the prey choice model to predict the sexual division of labor and its patch choice implications at various Great Basin locations in Paleopubian times, a period well beyond the reach of standard ethnographic analogies. The impact of prey choice on processing and transport strategies and its implications for archaeological assemblage composition were further explored in two studies by Duncan Metcalfe and Renee Barlow. Theoretical modeling indicated that high-ranked prey, defined as such by the fact that they have relatively low handling costs, should be processed and their low-utility parts discarded at the point of acquisition, mainly because that practice allows foragers to maximize the overall utility of loads carried back to residential bases (Metcalfe and Barlow 1992).

Treating lower-ranked items in similar fashion also improves load utility, but the gain is more than offset by the consequent loss in time available for foraging. Low-ranked items should more often be returned to base for processing. The implication: debris produced from handling high-ranked prey is likely to be underrepresented at residential sites, with debris from low-ranked prey overrepresented, a finding crucial for analysts interested in the reconstruction of past diets.

A second study illustrated the role of handling costs as determinants of residential site location, the key message being that foragers should position base camps in or near patches containing economically important but relatively hard-to-handle prey (Barlow and Metcalfe 1996). This finding provides additional support for the findings of the Carson Desert studies noted above.

Building on a lead initially offered by Hawkes and O'Connell (1992), Jason Bright, Andrew Ugan, and Lori Hunsaker (2002) extended the concept of handling costs to include investment in related technology. The basic argument: increasing diet breadth by definition entails greater investment in processing, which in turn favors increased investment in processing technology. The greater the diversity of low-ranked prey added to the diet, the greater the payoffs for developing tools that, despite their higher cost to manufacture, increase processing efficiency for specific resources. Thus, as diet breadth increases, technological diversity and the associated costs of making and maintaining the technology should increase as well.

Ugan et al. (2003) and Bettinger et al. (2006) subsequently extended the argument in a discussion of factors that determine the geographic spread of more expensive technologies at the expense of less costly ones: the high-cost bow and arrow vs. the low-cost atlatl and dart being the provocative case in point. Counterintuitively, the argument contends that broad-based foraging economies and technologies will generally displace those focused on the exploitation of a narrower range of high-ranked prey, despite the fact that they are in a fundamental sense more expensive.

**Intensification: New Frontiers?**

Anthropologists will recognize the replacement of less costly technologies with more expensive ones as "intensification." Greater investments in grinding stones, specialized basketry, pronghorn traps, snares, seed beaters, rabbit nets, fishing technology, and the bow and arrow all exemplify the process. The path toward intensification began after 4,000 years ago in the western Great Basin (Bettinger 1999) but is most apparent between 1500 and 650 B.C., when there were larger populations of
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Renee Barlow (2002, 2006) has quantified the costs of Fremont farming using ethnographic, historical, and experimental data and shows that prehistoric maize farming was competitive with many wild plant foods. She documents significant variation in the costs and benefits of different types of farming and in the process reveals a key feature of intensification: increasing productivity per unit area correlates with declining rates of return. The decision to adopt maize farming was driven by subcontinental demographic realities (Simms 2008) but was subject to the effects of these realities on local ecological considerations, primarily the trade-offs between farming and exploiting wild resources. Barlow's model predicts various degrees of commitment to farming and to residential stability, with alternative adaptive strategies occurring in varying mixes over time. The fit with the archaeological record of the Fremont is dramatic, and the extension of this line of investigation into the realm of inequality, group size, leadership, resource depression, competition, architecture, and other trappings of cultural complexity is the next logical step. The tools of behavioral ecology would bring hypothesis testing and an analysis of evolutionary processes that would only build on and synthesize post hoc historical descriptions.

In the western Great Basin changes in the organization of societies are evident in the shift from public to private goods (Eerkens 2004) and the evolution of social inequality (Eerkens 2009). Climatic patterns favored increases in the abundance of large game (Broughton et al. 2008), and the amelioration of food resources in general at this time holds implications for human demography. In the late Holocene, changes in hunting behavior to some degree reflect declining efficiency and a role for prestige in the adaptive mix (McGuire and Hildebrandt 2005).

These forays into the evolution of late Holocene behavior provoke healthy debate, some polemics, and some contention; all are familiar to those of us who explored the application of behavioral ecology to Great Basin archaeology in the late 1970s and early 1980s. We urge this research on, but our vantage also stirs some reminders about basic principles of behavioral ecology and rouses "some thoughts" about Great Basin archaeology. We employ the prestige hunting debate as a vehicle for those thoughts.

**Transcending the Prestige Hunting Debate**

The lively debate over costly signaling in the form of "prestige hunting" is an extension of pioneering research on late Holocene intensification and cultural complexity in California (e.g., Broughton 1994; Erlandson 1991; Hildebrandt and Jones 1992; Hildebrandt and McGuire 2002, 2003; Kennett 2005; Raab 1992, 1996; Wohlgemuth 1996). Kelly McGuire and William Hildebrandt (2005) propose that Late Archaic intensification in the western Great Basin is reflected in rising human populations that increase hunting pressure on large game. They argue that instead of depressing the number of large game and their representation in the archaeological record as predicted by a diet
breadth model, there is a proportional increase in large game in archaeological assemblages after 4000 BP. They propose that large game hunting continued despite rising costs because in a context of increasing cultural complexity, prestige became more valuable. A corollary of their argument is that diets remained broad at this time, and they present evidence that expensive resources, such as seeds, indeed remain in the Late Archaic diet. Their zooarchaeological evidence comes from two sites in northern and central Nevada (Pie Creek and Gatecliff shelters), but McGuire and Hildebrandt assemble other evidence for intensification from the western Great Basin: increases in the costs of technology, the bow and arrow after about 2000 BP, the costly movement of toolstone over long distances, the construction and maintenance of costly hunting facilities such as game drives, and the Late Archaic elaboration of rock art.

Jack Broughton, Frank Bayham, and David Byers (Broughton and Bayham 2003; Byers and Broughton 2004) and more recently Broughton et al. (2008; Broughton et al. 2011) employ data from the western United States to show that the increases in large game after 4000 BP coincide with climatic amelioration favorable to large-game populations. Human populations in the western Great Basin at that time were in fact depressed in the wake of mid-Holocene desiccation (Louverback et al. 2011) but subsequently increased, likely in response to the increasing productivity of the environment after 4000 BP (and demographic expansions in the Southwest and California). These researchers argue that human predation eventually depressed large game abundance in archaeological assemblages after 3000–2500 BP and that prestige hunting either is unlikely to have had an effect or is unnecessary to invoke as a cause of the changes in the archaeological record.

Hildebrandt and McGuire (2002:231) initially cast the argument for prestige hunting as a refutation of optimal foraging, implying that if prestige becomes a relevant currency in hunter decision making, then efficiency becomes irrelevant. Broughton and Bayham acknowledged that hunting is “motivated by multiple fitness-enhancing goals,” but they dismissed prestige as a possible “rationale” (2003:785). The discussion became polarized into two camps, each advocating for an essentialist type—optimal foragers or prestige hunters—and these positions colored subsequent investigation. The situation presents an opportunity to refer to underlying issues regarding costly signaling theory but, more importantly, to offer some reminders of basic tenets of behavioral ecology relevant to this issue.

McGuire and Hildebrandt appeal to costly signaling theory as a framework for understanding prestige hunting. Costly signaling (see Bliege Bird and Smith 2005) is a contemporary term for a rich intellectual tradition regarding the mysteries of reciprocity in the sociological investigations of Thorstein Veblen (1931 [1899]) and Marcel Mauss (1914), the economic anthropological inquiries of Marshall Sahlins (1972), and the practice theory of Pierre Bourdieu (1977). Human reciprocity is a form of signaling and raises a conundrum because at times seemingly wasteful efforts at reciprocity and altruism may bring prestige and power to individuals or benefits to groups. Biologists too have a long history of evaluating animal communication systems that bring costs and benefits to both signalers and receivers (Krebs and Davies 1997; Trivers 1971). The study of signaling in biology weighs the selection pressures for signaling against the underlying tendency of selection to favor lower risk and lower energetic costs.

Evolutionary studies of signaling in human cases reveal a wide range of reciprocity, from extensive food sharing based on reciprocal altruism to sharing that seems to be unconditionally generous but which in fact yields benefits to providers. The concept of prestige hunting as applied to the Great Basin is of the latter variety and is a form of symbolic capital wherein successful hunters benefit from their efforts and their generosity in ways that extend beyond caloric return (e.g., Bliege Bird and Smith 2005; Hawkes 1993).

McGuire and Hildebrandt have good reason to investigate prestige hunting. The application of foraging models in ethnographic cases shows that in general, energy maximization drives human hunting, but it also shows that men’s hunting does not inevitably maximize energy and minimize time (e.g., Bliege Bird et al. 2001; Hawkes et al. 1991; Hill et al. 1987; Wiessner 2002). Of course, the relevant question is what circumstances will favor one or another behavioral alternative in an adaptive mix, not whether a particular culture or time period is all one way or another.

Brian Coddington and Terry Jones (2007) highlight the key problem: payoffs associated with signaling are likely to be highly situational in both absolute and relative terms. The selective pressures for signaling must be consistently present to reinforce the behavior, or the alternative behavior of energy maximization and nonsignaling will overcome the inefficient “costly” behavior of prestige seekers. Costly signaling will thus represent some fraction of the possible behaviors that make up an evolutionarily stable strategy (ESS), a situation where the behavioral mix resists invasion by other behaviors.

Since the pressures for costly signaling are not constantly present, other behaviors such as energy maximization will always be part of the invading strategy and, hence, the adaptive mix. The only way prestige hunting could constitute an ESS would be if it provided a payoff so great that energy maximization was rendered inconsequential. Ethnographic studies have thus far not found such a case.

This is not a death knell for prestige hunting in the late Holocene Great Basin but, rather, an indication that the issue needs to be reframed. The question is not whether representatives of an entire culture or an entire archaeological period are “prestige hunters” but, rather, a matter of which circumstances select for an increased frequency of prestige hunting relative to energetically efficient foraging.
On the other side, Broughton et al. (2008) present substantial
evidence that climatically and ecologically induced fluctua-
tions in the abundance of large game play a significant role in
shaping archaeological faunal assemblages during the middle and
late Holocene. We agree but suspect that the abundance of large
game is not what is driving human population growth and the
trend toward intensification. These trends reflect increased in-
vestment of human labor, increased productivity, and declining
efficiency. It is unlikely that large game were ever the primary
component of any diet in the region.

Broughton et al. (2011) employ evidence from Hogup Cave,
Utah, and the Little Boulder Basin in northeastern Nevada to
propose that at least in those places, late Holocene faunal assem-
blages take their form from climatically driven increases in large-
game populations. They also argue that diet narrowed in these
places, implying that people increasingly turned to inexpensive
large game instead of costlier resources.

The analysis of Broughton et al. (2011) is sophisticated but
narrowly archaeological. Their conclusion that the late Holocene
eastern Great Basin was a place of contracting diets as people
feasted off inexpensively procured large game is strangely out
of context. Late Holocene North America in general was a time
of increasing human populations, accompanied by increases in
residential stability, investment in new technologies and infra-
structure, and increases in cultural complexity. This was true of
western Great Basin foragers and eastern Basin Fremont farmers
as well. As Barlow clearly showed, heavy investment in the culti-
vation of plant domesticates is a high-cost/relatively low-return
strategy—the epitome of a broad diet. Large Fremont farming
villages and a dense human population were located along the
Wasatch Front, a few days walk from Hogup Cave.

It is more likely that the increases of large game in archaeo-
logical assemblages such as Hogup Cave reflect increasing human
populations and the intensification of labor. Ethnographic evi-
dence shows that while big game hunting may produce high
returns upon encounter and successful pursuit, the high costs of
travel and search, the problem of failed search and pursuit se-
quences (Bird et al. 2009), and the sheer difficulty of large game
hunting ensure that it will remain a highly variable and often a
relatively small fraction of the overall diet.

Both parties in the prestige hunting debate bring evidence
to the problem that begs for synthesis: a model that accommo-
dates the evidence for variability in the frequencies of alternative
adaptive strategies and trends over time, rather than normative
characterizations of coarse-grained chronological categories
such as “Late Archaic” or “late Holocene.” Both parties in the
debate advocate for one or another alternative adaptive strategy,
rather than conceptualizing and pursuing an investigation of the
circumstances that would select for trends in the mix of behav-
iors over time.

The tendency of archaeologists to cast the past in norma-
tive terms is remarkably persistent. Optimal foraging theory
and costly signaling theory are not essentialist categories that
characterize even the behavior of a single individual, let alone an
entire group, culture, or period. This was a flaw of the traveler-
processor model 30 years ago (Bettinger and Baumhoff 1982),
and it is a problem here.

Efficient hunting and prestige hunting constitute an evolu-
tionarily stable strategy. The concept is germane to framing the
trade-offs between prestige hunting and efficient hunting. Ef-
ficiency will always experience selection because foragers must
eat, and one’s own effort is the best available means of getting
enough. On the other hand, male hunters, depending on their
life history stage, experience the need for allies and supporters
and compete for mates. The larger and more entwined the social
setting, the greater the potential payoffs associated with signaling
to aid the creation and maintenance of these relationships. Ethn-
ographic accounts convey a sense of this; for instance, Codd-
ing et al. report:

When offspring provisioning benefits are high and opportu-
nity costs low, men should shift their focus to low-variance
resources when and where they become available. For
example, turtle hunting among Meriam is associated with
moderate variance and is most frequently undertaken by
unmarried men; turtle collecting during the nesting season
is more reliable and is undertaken more by married men....
Similarly, Hadza men switch from targeting large unreliable
prey to smaller reliable prey when their wives are pregnant..., and
Maru men switch from hunting kangaroo to hunting
monitor lizards when older.... Larger social networks pro-
vide a forager with a larger pool of potential recipients and a
larger audience, leading to higher payoffs for returning with
large quantities of food to distribute widely, such as gain-
ing higher status, deference in decision-making and perhaps
greater social support.... these in turn may provide indirect
benefits to existing offspring, including increased offspring
survivorship (2011:6).

Despite these trade-offs, prestige hunting will always be a
fraction of the behavioral set because energy maximization will
always be the invading strategy. By framing the question in terms
of an ESS, archaeology may be able to muster the evidence for the
circumstances where a higher frequency of prestige hunting may
be expected. Single sites, however, will never prove or disprove
such questions.

Thus far the prestige hunting debate has used ethnographic
evidence only in a post hoc manner. A better approach might be
to marry theory with arguments about process based on ethn-
ographic cases to model/predict variation in costly behaviors rel-
ative to efficient behaviors. Ethnographic analysis exemplified by
the quote above can help archaeologists take an important step
in this direction. The general understanding of the late Hol-
ocene Great Basin, intensification, and the attendant behaviors is
unlikely to be resolved if approached only in a narrowly empirical and strictly archaeological manner.

Looking Forward

Prestige hunting is part of the larger investigation of intensification and cultural complexity in the western Great Basin, and to their credit, McGuire and Hildebrandt frame their argument in these terms. Larger geographic scales are also appropriate for this issue, and Simms (2008) provides a general synthesis that considers the effects of surrounding regions on the ancient history of the Great Basin—especially the Southwest and California.

When larger scales of analysis are employed, the findings from individual sites can be placed in context, restraining the unrealistic expectation that site-by-site comparisons of assemblages will reveal prestige hunting vs. efficient hunting or broad diets vs. narrow diets. Site-specific circumstances and the resulting variability will cumulatively reveal patterning as the sample grows, and it is in these larger patterns and trends that matters of regional demographics, technological investment, shifts in storage strategy, residential and logistic mobility, inequality, leadership, and prestige will be identified.

The late Holocene western Great Basin and the eastern Great Basin/northern Colorado Plateau are contexts wherein we should expect behaviors associated with intensification, including but not limited to prestige hunting. Radiocarbon frequencies from multiple studies show that there were more people in the eastern and western Great Basin between 1500 and 650 BP than at any other time in antiquity (Lindsay 2005; Louderback et al. 2011; Massimino and Mercafe 1999). These trends fly in the face of an argument that foragers lived the good life by avoiding high-cost resources because large game was so plentiful.

Research aimed at identifying trends may be productive. For example, Jacob Fisher (2010) employs faunal analysis and strontium isotopes from large-mammal assemblages at the Fremont village of Five Finger Ridge in central Utah to test for change over time in the travel and transport costs of large-game hunting. Fisher finds change in faunal exploitation during the life of the village that is inconsistent with a least-cost prey choice model. The preliminary nature of his conclusions is surely due in part to the paucity of baseline data on strontium isotope levels in large game, but his results certainly suggest the potential utility of this line of research.

Deanna Grinstead (2010) attempts to model how far a hunter can travel before the efficiency of large game hunting declines to the point where it is costly. She proposes that hunting remains efficient at distances of 150–200 km. Her model only measures the energetic expenditure of a human walking and carrying a load, rather than the crucial currency of time that is essential to the power of foraging models. Further, she models humans as single-prey loaders, rather than employing patch choice and the marginal value theorem, which would predict how far a hunter would go before stopping at a profitable patch.

We suspect that the distances hunters can travel before the costs of large game hunting become costly are much shorter. McGuire et al. (2007) model travel costs from the Owens Valley to high-altitude hunting camps in the White Mountains. They find that return rates for such ventures are well below 1,000 kcal/hour, return rates that make such hunting expensive. Whitaker and Carpenter (2012) appropriately return to the variables of time and energy, as well as patch choice and opportunity costs. They find that the distance at which large game hunting becomes costly is highly variable, but under many circumstances hunting trips become costly beyond only a few tens of kilometers.

We propose that the circumstances of prestige hunting are subject to modeling among contrasting Great Basin situations ranging from village base camps in wetlands, to fall pinyon camps, to quarrying and lithic reduction locales, to high-altitude hunting expeditions, to large Fremont villages and dispersed Fremont hamlets. Archaeological contexts such as these may be compared and framed with those known ethnographically to generate expectations of the alternative adaptive strategies that could compose an ESS under various circumstances such as those above. For instance, Hadza hunters of all ages rarely miss an opportunity to show off (Hawkes et al. 1991), and their efforts are widely noted and comparatively evaluated (Blurton Jones et al. 1997). Martu hunters are equally opportunistic (Bird et al. 2009), as are Meriam men in Torres Strait (Briggs Bird et al. 2001; Smith et al. 2003). Nunamiat hunters appear to be provisioners (Binford 1978; see Coddin et al. 2011 for a comparative review). Young adult Ache hunters pursue prestige because they have the best chance of success due to their physical abilities and the benefit of prestige may translate into mating opportunities. Middle-aged Ache men with families to feed favor a risk-minimization strategy that aims at consistency in provisioning. Older men whose physical capabilities have become diminished may tend to focus on resources with lower pursuit costs and perhaps greater processing/handling costs (Hill and Hurtado 1996).

We will, of course, never see all of these individual behaviors in the archaeological record, but behavioral ecology never proposed to model behavior on a moment-to-moment basis, even in ethnographic situations. The goal is to provide explanations of the forces that produce a behavioral mix and changes in frequencies of alternative behaviors over time. After all, it is this sort of change that we recognize retrospectively as "history." We do not presume to speak to all aspects of this issue here, and we do see the investigation of prestige hunting only as an example of the broader exploration of intensification and cultural complexity. We applaud the efforts to tackle the many faces of intensification using evolutionary modeling that are in play among archaeologists working in the western Great Basin. We suggest that Fremont archaeology has much to gain from further work along these lines. Indeed, as McGuire and Hildebrandt (2005) note, the processes on the opposite sides of the Basin are similar. A synthesis enriches the descriptive histories but does not
challenge them, because history is always part of evolution and adaptation—the outcome of organisms dealing with the problems of life.

The goal of academic debate is understanding. The Great Basin past is better understood now than it was 30 years ago, and the principles of behavioral ecology have helped inform that effort. We find much to be enthusiastic about and think that Basin archaeologists are among the leaders in practicing scientific archaeology. Many of the criticisms we made in 1982 are no longer valid, and to us, the evolution that has taken place in our field is gratifying and healthy.

References Cited

Barlow, K. Renee

Barlow, K. Renee, and Duncan Metcalfe

Bayham, Frank E.

Beaton, John M.

Bettinger, Robert L.

Bettinger, Robert L., and Martin A. Baumhoff

Bettinger, Robert L., Bruce Winterhalder, and Richard McElreath

Billings, William D.

Binford, Lewis R.

Bird, Douglas W., Rebecca Bliege Bird, and Brian F. Cuddling

Bird, Douglas W., and James F. O’Connell

Bliede Bird, Rebecca, and Eric Alden Smith

Bliede Bird, Rebecca, Eric Alden Smith, and Douglas W. Bird

Blurton Jones, Nicholas

Blurton Jones, Nicholas G., Kristen Hawkes, and James F. O’Connell

Bourdieu, Pierre

Bright, Jason, Andrew Ugan, and Lori Hunsaker

Broughton, Jack

Broughton, Jack M., and Frank E. Bayham

Broughton, Jack M., David A. Byers, Reid Bryson, William Eckerle, and David B. Madsen
2008 Did Climatic Seasonality Control Late Quaternary Arido­dactyl Densities in Western North America? *Quaternary Science Reviews* 37:1936–1937.

Broughton, Jack M., Michael D. Cannon, Frank E. Bayham, and David A. Byers

Byers, David, and Jack M. Broughton

Charnov, Eric L., and Gordon Orians


Some Thoughts on Evolution, Ecology, and Archaeology in the Great Basin

Consultants, Montrose, Colorado; and SWCA Environmental Consultants, Salt Lake City.

Louderback, Lisbeth A., Donald K. Grayson, and Marcos Llobera

Marwitt, John C.
1970 Median Village and Frequent Cultural Regional Variation. Anthropological Papers No. 95, University of Utah Press, Salt Lake City.

Massimino, Jacqueline, and Duncan Mcalpine

Mauro, Marcel

McGuire, Kelly R., and William R. Hildebrandt

McGuire, Kelly R., William R. Hildebrandt, and Kimberly L. Carpenter

McAlpine, Duncan, and K. Renee Barlow

O'Connell, John F., and Kristen Hawkes

O'Connell, James F., Kevin T. Jones, and Steven R. Simms

Peavey, William J.

Price, Barbara

Raab, L. Mark


Raven, Christopher R., and Robert G. Elston

Rhode, David

Sahlins, Marshall D.

Schiffer, Michael B.

Simms, Steven R.


Smith, Eric A.


Smith, Eric A., Rebecca Bliege Bird, and Douglas W. Bird

Taylor, Walter W.

Thomas, David H.

Tinbergen, Niko

Trivers, Robert L.

Ugan, Andrew, Jason Bright, and Alan Rogers

Veblen, Thorstein

Whitaker, Adrian R., and Kimberly L. Carpenter

Wiesner, Polly

Williams, George C.
Wilmsen, Edwin N.


Winterhalder, Bruce


Wohlgenuth, Eric


Zeanah, David W.


Zeanah, David W., and Steven R. Simms

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