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Modeling the Gastric: Great Basin Subsistence Studies since 1982 and the Evolution of General Theory

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It is important to an understanding of the entire Shoshonean culture that it was stamped with a remarkable practicality. So far as its basic orientation is definable, it was "gastric."
Julian Steward (1939:4)6

Steward's famous statement signals our proposal that research strategies for investigating prehistoric subsistence serve as a barrenometer of theoretical thinking among Great Basin archaeologists. Steward's statement reflects the materialist axiom that interactions of environment and cultural infrastructure mediate cultural structure and superstructure. This is a persistent historical thread throughout the history of anthropology (e.g., Harris 1968:165; Stocking 1988:122-102) and in studies of foraging societies (see Bettinger 1973:8, 45-50, or R. Kelly 1992:50 for histories). Given these materialist underpinnings, coupled with the fact that subsistence, settlement, and environment are the most available data in Great Basin archaeology, it is not surprising that the greatest attention to theory revolves around subsistence issues, regardless of how much biogeography we pay cultural complexities. We do not deny this complexity, but suggest that research strategies based in theory help us approach complexity.

We evaluate developments within the materialist foundations of Great Basin research since 1982, as seen in three research tasks: mid-range investigations, optimal foraging models, and the travel-processor model. Originally a different theoretical perspective advocated each tack, illustrating the contrasting goals and assumptions driving Great Basin research. Cultural materialism adopted mid-range theory; evolutionary ecology promoted optimal foraging models, and the travel-processor model drew, at least implicitly, from cultural inheritance.

However, Great Basin archaeologists use these three tasks in a manner the interweaves among the three theories, and defies strict theoretical boundaries. It is another contention that this is evidence that the materialist tradition thrives in Great Basin subsistence studies, despite debates among Great Basin archaeologists over the last fifteen years. Instead of initiating a paradigm change, theoretical disputes have shifted the attention of Great Basin prehistorians away from simplistic culture-historical and cultural ecological reconstructions. Investigations of archaeological formations processes better inform our understanding of the past, whereas our explanatory models replace adaptation and history with the more theoretically powerful concepts of evolution and selection.

We choose 1982 as a point of departure because the volume Man and Environment in the Great Basin (D. Macnair and O'Connel 1982) marks a previous attempt to review the status of Great Basin archaeology. Perhaps more important, the early 1980s were a watershed in how Great Basin archaeologists perceived and used theory. This is important considering the long-standing role the Great Basin has played in the development of anthropological theory.

1982: UPHOLD AND CHANGE

Materialist approaches to investigating subsistence in the Great Basin trace their intellectual descent from two disparate theoretical perspectives. On the one hand, Great Basin archaeology developed with the milieu culture history, with its steadfast emphasis on description of spatial and temporal variability among archaeological assemblages. On the other hand, Basin prehistorians benefited from the indigenous development of cultural ecology with its attempt to understand causal factors within cultural systems. The history of Great Basin archaeology can be understood as an attempt to reconcile these two different perspectives, a struggle that continues to this day (cf. Bettinger 1991:1; Kelly 1997 also Rhode, Chapter 3). However, archaeologists are aware of flaws in both approaches as they are complex and diversity in the archaeological record.

Culture historians consistently perceived subsistence variability as cultural variability and produced...
In the initial volume, *Epistemology, a long-term study of Monte Vale, O. Thomas (1981a, b) aimed at a very different set of issues: the distance between theoretical discourse and field-based empiricism. This exchange resonated with practical archaeologists as Thomas focused on how to reconstruct behavior from the archaeological record. Thomas accomplished this by building a "working model" (1983:640) for subsistence, technology, mobility, and group composition based on ethnographic analogy for the protohistoric period. This model also stemmed from processual archaeology, but the research emphasis was different from that of O'Connell and colleagues (1982). Although many paid lip service to explanation and nonheuristic goals, in practice midrange theory was the goal. Archaeology "began as a downward deduction at so resolutely middle a level. . . ." (B. J. Since 1981:714), or observed attention to more general issues by conflating "theories of general sets" with midrange issues exploring the "linkage between theories of limited sets and empirical patterns." (Bettine 1981:81). Larger issues were ignored or seen as contingent on first "decoding" the archaeological record (Linford 1982:19-21). No one could deny the value of improving our ability to decode the archaeological record, and solving midrange problems became central to archaeological field work in the face of federally mandated, theoretically informed research design. Midrange research also posed a threat to culture historians because it was consistent with the refrain that "thick" work had to come before theory. For these reasons, midrange theory has had the broadest impact on Great Basin archaeology during the 1980s and 1990s.

Like O'Connell and others (1982), Bettiner and Haubold (1982) stressed the need for general theory to overcome the limitations of culture history and cultural ecology. Instead of illustrating their interest in explanation using a family of models like O'Connell and others (1982), they used the Numic spread as a case study to convey their message about theory. Specifically, they proposed a natural selection as a mechanism to tackle a problem that more agreed was behaviorally facile when left as a cultural historical description: migration of Numic speakers from a southern California homeland into their present range around one thousand years ago. The travel—processor model was much more than midrange theory because it incorporated a number of resource cores, population density, fertility, selection for unbalanced sex ratios, and the long-term consequences resulting from short-term decisions-making sense directly from evolutionary theory.

Unfortunately, linkage to the Numic spread debate caused the loss of much of the general theoretical foundation of the travel—processor model, despite Bettiner's (1991:100-105) attempt to rescue it from this particularist fate. Just as many identified evolutionary ecology only as optimal foraging theory, the relevance of the
traveler-processor model was reduced to the Numic spread. Too, the traveler-processor model maintains a culture-historical flavor by explaining subsistence-settlement variability in terms of cultural variability. However, it fits within an evolutionary theory of behavior, cultural inheritance theory, which focuses on mechanisms of cultural transmission and a presumption of group selection (Bettiger 1991a; Boyd and Richerson 1985).

Connections through Uphill and Change

Contrary to what arises as the central feature of theoretical discussion, we focus on attention on shared middle-range themes in order to clearly specify fundamental differences between theoretical approaches. Optimal foraging is a good example. Popularly referred to as optimal foraging theory (OFT), "foraging theory" alone does not stand as a theory, but as an array of formal models based on evolutionary theory" (Simms 1987:19). Bettiger (1991:1205–1210) effectively shows that optimal foraging theory is middle-range theory and that confusion results from an unfortunate disconnection from general theory. Theoretical contexts with very different aims, and fundamentally different assumptions employ OFT as a mid-range research strategy. The traveler-processor model draws from principles of optimal foraging, but this alone does not identify the underlying theoretical assumptions of the model (Bettiger and Baumhoff 1982) or cultural inheritance theory (Bettiger 1991b; Boyd and Richerson 1985). Other examples of the connections among research tactics of different theoretical perspectives abound. R. Kelly’s (1990, 1991b) work in Stillwater, and the analyses of White Mountain faunal collections by Grayson (1990b) and Broughton and Grayson (1991) employ optimal foraging to address mid-range issues. In the former case, theoretical assumptions from evolutionary ecology clearly underlie the use of foraging theory, however Kelly’s (1985) broader applications of OFT lean heavily on cultural materialism. In the latter case, Broughton and Grayson (1993) explicitly deny any attempt at moving beyond the mid-range use of optimal foraging in their particular application. They surely hold a position about general theory, but it is not evident simply through the use of optimal foraging models.

We direct attention to these matters less as a clarification of what optimal foraging is than as a device to suggest there are more important issues at hand. Discourse emphasizing mid-range issues obscures real contrasts between general theories in the Great Basin. We agree with Bettiger who argues that differences among researchers turn "on more deeply hidden general theoretical suppositions" (Bettiger 1991:110). He continues, "If there has been recently common movement to act and middle-range research is independent of generalizing theory, that is only because this permits eclectic movement between different theoretical positions without declaring commitment to some coherent theoretical framework" (Bettiger 1991:111).

The various theoretical positions of the early 1980s drew a fundamental distinction between perception of subsistence in behavioral vs. cultural terms. Mid-range theory (D. Thomas 1983a) and evolutionary ecology (O’Connell et al. 1982) treated subsistence as behavior. This perspective, and the shiftings between optimal foraging theory and mid-range theory, revealed a pressing need to ask new questions of the archaeological record and develop new ways of reading that record such as rapprochement studies, harvesting and processing experiments, computation of transport costs, and ethnoarchaeological studies. Knowledge from these kinds of studies pertains to a broad array of research issues in the Great Basin. In contrast, the traveler-processor model (Bettiger and Baumhoff 1982) perceived subsistence as cultural variability, emphasizing ethnic maintenance and tradition-theoretical questions of cultural inheritance. These are important issues that also draw attention to the problems of the mid-range (e.g., how to cognate ethnoecology). As for general theory, the differences in how these theoretical perspectives approach behavior vs. culture, change vs. tradition, and evolution vs. adaptation are crucial to how they approach the past. When Bettiger (1991:1211) refers to "commitment to some coherent theoretical framework," he is talking less about acceptance of a dogma, than the realization that theory is present even if it is unacknowledged, and that consistent application is the only way we can approach common problems from different theoretical perspectives.

A CLOSER LOOK AT THE RESEARCH TACTS

To perceive the underlying differences in theory among these research tactics, these relationships to theoretical perspectives, their connections among each other and to prior theory in the region, as well as their success in producing new research questions, it is necessary to take a closer look.

Mid-Range Theory and Cultural Materialism in Monitor Valley

The epistemology for Monitor Valley (D. Thomas 1983a) defines an approach for investigating foraging strategies advocating development and application of "mid-range" models. Thomas notes that general theory often fails to explain variability among particular cases. Examplifying this particularism vs. generalist paradigm, Thomas uses the forager-collector model (Binford 1980) to illustrate variability among Great Basin hunters and gatherers. In its most general form, the model represents a mobility continuum that varies according to global differences in effective temperature. Thomas points
our that Great Basin bands sharing the same culture, language, and technology, and located within a 100 km radius of one another, ranged from full-time foragers to full-time collectors. Although the general model characterizes Great Basin adaptive diversity, it fails to explain why such local variability occurs without corresponding global-scale climatic variability. Thomas cautioned that there is no reason to assume that any particular subsistence-settlement strategy should characterize any specific region, and that adaptive strategies can change through time. He concludes “we currently lack the theoretical models to explain this variability” (D. Thomas 1983:219).

Thomas suggests that explanatory models must arise from archaeological investigations of particular cases designed to explore variability in the archaeological record. For example, the research designs for Monitor Valley (D. Thomas 1983a) and Hidden Cave (D. Thomas 1985) emphasize identification of alternative foraging and collecting strategies in the archaeological record. To be successful, such research approaches must use mid-range models to link expectations with observations of the archaeological record (D. Thomas 1983).

The analysis of mountain sheep bone from Horizon 2 of Carcass Shelter (D. Thomas and Meyer 1981) is a good example of applying mid-range models to assign meaning to archaeological data. Using Binford’s (1978) Modified General Utility Index (MGU), Thomas and Meyer show that bone representation in the deposit is inversely proportional to economic utility. Such a pattern is consistent with Binford’s (1978a) reserve utility profile, characteristic of Nunamiuse butchering sites where hunters discard low utility parts on the spot and transport high utility parts elsewhere for storage and consumption. The similarity of the archaeological curve with the mid-range model provides evidence that Horizon 2 represents a logistic field camp (D. Thomas 1983:219).

Another mid-range analysis of faunal remains from Horizon 2 subsequently challenged this interpretation by showing an inverse correlation between bone density and MGU. (Grayson 1983:217), suggesting that the assemblage is more passively explained as resulting from differential bone preservation than from human exploitation. Although the two mid-range models have different implications for interpreting Horizon 2, Thomas sees further development of such models as crucial to learning how to interpret the archaeological record and thus 2 priorities of archaeological research (D. Thomas 1990).

Great Basin subsistence studies have undoubtedly benefited from mid-range research inspired by the Monitor Valley epistemology. For example, Grayson’s (1983:391) admonition that naturally accumulated and culturally introduced small mammal remains were indistinguishable at Galleta Shelter proved taphonomic studies directed toward identifying distinguishing criteria (Hockett 1951; Schmitt and Justel 1944; Schmitz and Lapham 1951). As a result, the ability of archaeologists to read small mammal assemblages has improved, illustrating just how powerful mid-range research can be.

The lack of a role for higher-order theory in directing mid-range and archaeological inquiry, however, troubles critics of the Monitor Valley epistemology (Bettina 1979:77–81; Binford 1979:3–4, 33–41). D. Thomas perceives the world from a cultural materialist production (1990:279), with implied nonfunctional assumptions of adaptation, homogeneity, and group selection. Bettina 1979:37; B. Kelly 1992:3–7, is necessarily, he allows little leeway for that paradigm to direct archaeological inquiry. Beyond using the forager-collector model as the context for a direct historical approach to the ethnographic record, Thomas explores that investigations of particular cases, under the rubric of mid-range theory, will animate development of higher-order theory (D. Thomas 1983:211).

However, such an approach cannot escape general theoretical assumptions about how the world works requiring that mid-range models derive from general theory. This is best illustrated by the observation that Binford’s (1978a) mid-range model of foraging strategies is underpinning an optimal foraging model based on theoretical assumptions that economic relationships govern foraging behavior. (Bettina 1979:207–11: Grayson 1983:213:63–66). Similarly, D. Thomas’s (1985:234–37) use of the economic defensibility model as a tool for understanding the extent of territoriality expressed by different Great Basin groups according to the density and predictability of resources, also employs principles of optimality to generate expectations about behavior at the mid-range, but not in higher-order theory. The latter example is particularly revealing because of the similarity between Thomas’s proposed effects of resource structure on territoriality and the apparent correlation between resource structure and adaptive strategy in general. Sedentary, collecting systems (i.e., Green Valley Pueblo) occurred in areas with contiguated, dependable, and abundant resources, but residential mobile strategies (i.e., Kawish Mountain Shoshone) were the norm whenever resources were dispersed, unreliable, and rare. Intermediate environments with resource structure changed seasonally supported a mixed foraging and collecting strategy (i.e., Reuse River Valley Shoshone). Given that resource density and predictability explain territoriality among the same groups, it seems logical that these factors may also explain mobility strategies. So, the Monitor Valley epistemology logically seems from the cultural ecology tradition of Steward (1952:520–21) who noted that local variability among the distribution, productivity, and reliability of critical resources determined population density, and the size, permanence, and dispersion of villages. Thomas is incorrect, then, to claim that we lack theoretical models capable of explaining adaptive variability.
among Great Basin foragers; we have had such models for sixty years. However, available models are capable of post hoc explanations and are unable to predict substantive settlement variability. For example, Thomas cannot explain why some Great Basin groups residential "map onto" pine (i.e., Kawaiy Mountain and Snake River Valley Shoshone) while others reside elsewhere and logistically collect pine (i.e., Owens Valley and Carson Sink Paiute) in a manner that allows him to predict the mode of pine procurement in Modoc Valley (D. Thomas: 1983:215–165, 1983b:151–53). It is difficult to see how research focused at the red-range will ever develop such predictive powers; these must come from general theory.

To illustrate the limitations of divorcing higher-order theory from the structure of archaeological inquiry, consider ways in which the forager-collector model fails to characterize variability among Great Basin adaptive strategies. For example, Thomas sees spatial incongruity of critical resources as encouraging forager-like adaptations, whereas the lack of spatial incongruity promotes collector-like strategies (D. Thomas: 1983:214–15). If true, this relationship contradicts that anticipated by the forager-collector model, which holds that collecting systems are organizational responses to temporally and spatially incongruent resources (Binford 1981:7–81).

Too, Billerter (1993:725) notes that Great Basin foragers and collectors had differences of technological organization (i.e., curated vs. expedient) anticipated by the model, and suggests that this may reflect limitations on logistic mobility imposed by population density and transport costs. Such inconsistencies between the theoretical model and particular cases offer an opportunity to use general theory to flesh out the causes of local variability and generate expectations about the circumstances that promote foraging and logistic organizations in the Great Basin. These are questions that clarify research questions for archaeological inquiry and highlight the kinds of mid-range research required developing and archaeological application.

Optimal Foraging Theory and Evolutionary Ecology

O’Connell and others (1982) propose evolutionary ecology as a suitable theoretical framework for building predictive explanations of subsistence variability in the Great Basin. Evolutionary ecology assumes that natural selection favors behaviors that maximize reproductive fitness. As circumstances relevant to human decision-making change, the frequencies of alternative behaviors change, producing new configurations of behavior and culture. Such an approach differs from other anthropological paradigms by providing a mechanism for change that enables formulation of predictions for possible test. The approach also differs by assuming that selective forces on behavior are strongest at the individual rather than group level. By emphasizing the inclusive fitness of individuals, evolutionary ecology seeks explanations at a more parsimonious level, focusing attention on how conflicts of interest between individuals have group level consequences. The priority placed upon individual selection does not deny the possibility of group selection, but simply recognizes that group selection can override individual selection only under limited circumstances (R. Kelly 1993:435; Krebs and Davies 1988:8–9; Maynard-Smith 1976: et. Richmond and Boyd 1982:84–91). Rather than assert what is universally true, evolutionary ecologists argue for individual selection as a logical starting point in the research process.

Evolutionary ecology relies on optimality as a tool for testing hypotheses about the causes of behavior. Optimality models rose to prominence because particular. Currens and constraints as selective forces of behavior, allowing prediction of behavior based on the costs and benefits of behavioral alternatives. This is not a functionalist assumption that all behavior must be seen as optimal. Instead the approach assumes that evolution will select behaviors leading toward optimality, if the model has correctly identified currencies and constraints that represent selective agents on that behavior (Simms 1987:79–81). Thus, it is an inherently evolutionary approach (cf. Bettinger and Richerson 1996:223–226).

O’Connell and colleagues propose optimal foraging models as a theoretically coherent basis for explaining known spatial and temporal subsistence variability in the Great Basin and for predicting "prehistoric subsistence patterns where suitable ethnographic models do not apply" (O’Connell et al. 1982:234–235). To illustrate their explanatory utility, they use the diet breadth model (Schoener 1971) to pose an explanation for subsistence change during the Postocene–Holocene Transition. The diet breadth model tracks resources according to the ratio of gross calories gained to handling time. It is a standard model that predicts if a forager should take or pass a resource on encounter according to whether the returns obtained from harvesting that resource are greater than those gained from continuing to search for higher ranked resources. Two important implications of the model are that the decision to take a resource depend on the abundance of higher ranked resources, and that a decline of higher ranked resource abundance can expand diet breadth to include previously ignored resources. Based on these principles, O’Connell and colleagues suggest that seeds may have entered diets of Great Basin foragers after 8500 B.P. in response to declining encounters with large game (O’Connell et al. 1982:234).

This hypothesis relies on a reasonable inference that seeds ranked lower than large game in the diets of Great Basin foragers, justified by various ethnographic cases around the world (e.g., Winterhalder and Smith 1981). To evaluate these assumptions, Simms (1987) experimentally harvested and processed a set of plant resources, comparing their caloric return rates to those
estimated (from historical, ethnographic, and consera-
vation literature) for animals. Simmons showed that game, as
expected, were among the highest-ranked resources
available to Great Basin foragers, with each correlating
generally with body size. Seeds, although variable in
return rate, were the lowest ranked resource class. Given
the principles set forth in the diet breadth model, it was
reasonable to assume that both pre-Apache and Apache
foragers took large game whenever available but that
seeds entered either diet as the abundance of higher-
ranked resources declined.

However, influences became more complex when Simms
estimated resource encounter rates to calculate the op-
timal diet breadth of ethnographic hunters and gather-
ers. The model predicted that under many circumstances
of animal abundance expected in the ethnographic
Great Basin, gatherers should include the lowest ranked
seeds from their diets, even when men’s and women’s
diet breadths were modeled separately and large game
were excluded from the suite of resources available to
women. If the diet breadth model fails to predict the
harvest of seeds by ethnographic gatherers even when it
allows for larger game in the resource set, then it also
fails to explain the inception of seed use at the end of the
Peopledocene as resulting exclusively from a decline of
encounters with large game. Simms (1987:82-83) sug-
gests that corollaries might account for seed use: gather-
ers “banked” low-ranked but storable seeds in anticipa-
tion of over winter shortages of higher-ranked resources.

There is an important, but often overlooked, point to
make about this analysis. Note that Simms never as-
sumed the foraging of Great Basin foragers to be opti-
mal (as measured by the diet breadth model); nor did
he test the diet breadth model against the behavior of
Great Basin foragers. Instead, he was using the diet
breadth model to test a hypothesis that the intensifica-
tion of seed use at the end of the Peopledocene signaled
a diet breadth expansion in response to declining large
game densities. Failure of the model to support the hy-
pothetical model to suggest other constraints, in this case
the costs and benefits of storage, as the selective force
explaining the subsistence transition. In addition, this
shifted the contingency of the analysis from a “daily” to
a “yearly” time frame (Simms 1987:83). This exem-
plified the point that optimal foraging theory best serves
archaeology as a set of corollary principles that speak to
a body of empirical data and 1ollow logically from gen-
eral theory in evolutionary ecology (Simms 1988:420).
Thus in a broader sense than mid-range models, opti-
mal foraging models qualify as middle-range theory with
explicit links to guiding theoretical principles (Betting-

Critics warn of several pitfalls in using optimal for-
aging models as an archaeological tool. One criticism
concerns the use of foraging experiments as replications
of prehistoric resource return rates (Bettinger 1991:102-
Such critics are skeptical that modern scholars
approach the expertise of prehistoric foragers, whose
skills are irreproducible. Too, it is difficult for modern
experts to judge the circumstances under which pre-
historic foragers chose to take resources. This would be
critical when “model of procurement” affects the return
rate obtainable from a resource. For example, resources
obtained from woodlands along the margins of wetlands
are likely to provide greater returns than herbage of the
same resource using a seed beater (Bettinger 1993:33).

Similarly, cultural hunting tactics, particularly of
small to medium sized animals, are likely to provide
greater returns than individual stalking of the same spe-
cies (Simms 1987:77-78). Conversely, “green plant pro-
curement” tactics, designed to maximize gross harvest
of immature seeds, probably produce lower return rates
than “brown plant procurement” of the mature seeds of
the same species (Bettinger 1994:47-48; Bettinger and
Bauchdell 1982:83-85). Although experimentation
can illustrate the range of return obtainable by various
harvest techniques, it is much more difficult to make infer-
ences based on the presence of a particular resource in
archaeological context. For example, the presence of
pimeweed chaff in cave deposits and coprolites of the
Great Basin evidence that diets were very broad
(Simms 1985:842-852), was pimeweed collected only
under exceptional circumstances (i.e., woodlands) when
they provided returns high enough to merit a much nar-
rower diet (Bettinger 1994:11), or is the chaff from pro-
cessing what may only have been small amounts of
pimeweed inside the cave?

Another criticism concerns the demands optimal for-
aging models make of archaeological data (C. Beck and
1991). To investigate prehistoric subsistence, optimal
foraging models must generate predictions that are test-
able in the archaeological record. These critics argue
that optimal foraging models only have predictive ap-
plicability for individual expansions, and argue that the
archaeological record reflects aggregate behaviors al-
terred by site formation processes, making foraging mod-
els too difficult to apply.

Another important criticism concerns the applicabil-
ity of optimal foraging models to the kinds of subsis-
tence behavior archaeologists wish to understand. For
example, Bettinger (1993:49-50) notes that the most
commonly used optimal foraging models (i.e., diet breadth
and patch choice) are contingency models that calculate
optimal behavior according to momentary circumstances.
Contingency-based predictions can be misleading if other
foraging constraints select for foraging efficiency over
the longer term. For example, a forager whose selective
constraint is to avoid starvation, but who optimizes
behavior according to momentary contingencies may
collect the necessary calories less efficiently than a for-
ager who takes resources that seem suboptimal concern-
ing momentary returns. According to Bettinger, this
problem may be particularly relevant to foragers who store little or collect little, among whom noncorrigibility foraging models may be more appropriate."

Too, reliance on optimality as a research tool focuses attention on the economic consequences rather than evolutionary causes of the behavior. As a result, an optimality approach runs the risk of assuming all be-

havior to be adaptive, and formulating explanations that are functionalist rather than evolutionary (Bettinger 1992:121-23; D. Thomas 1983:24). We return to these significant criticisms later.

The Traveler-Processor Model and Cultural Inheritance

Unlike Great Basin cultural materialists and evolutionary ecologists who trace their heritage from the eco-

logical tradition of Julian Steward, Bettinger and Baumbauch (1982, 1983) draw inspiration from cultural-
historical approaches to Great Basin archaeology. They critique cultural ecology for failing to explain cultural evolution, because it emphasizes transformations within adaptive systems rather than selection of adaptive vari-

ability. In their view, migration and diffusion are more compatible with evolutionary theory than the ecologi-

cal idea of adaptation, because they imply selective re-

placement rather than transformation. Bettinger and Baumbauch advocate an evolutionary approach for in-

vestigating prehistoric adaptive variability that empha-

sizes group rather than individual selection.

Bettinger and Baumbauch use logistic distributions as a springboard for their challenge to the adaptations-

ization program. The complex mosaic of language groups among North American foragers, previously as-

cribed by glottochronology, suggests that migrations were

common in prehistory. Migrations made sense in an evolutionary context because they imply the selection of one group over another. However, prehistoric migrat-

ions challenge the implicit assumption of cultural ecol-

ogy that foragers with similar technologies will adapt to the same environment in similar ways. They use the spread of Numic speakers through the Great Basin as an example of the paradox. If the demands of making a living in the Great Basin imposed a particular kind of subsistence adaptation on Great Basin foragers, how could one group ever supplant another?

The traveler-processor model deduces a theoretical conundrum of forager subsistence adaptation, using opti-

mality foraging models to elicit expectations about how

pry and patch choice decisions affect settlement and

mobility among foragers. Foragers with high average

foraging returns and narrow diet breadth possess a "travel-

er" mobility strategy. Travelers restrict their diets to

rare resources with high caloric returns but low encoun-

ter rates. Therefore, travelers endure high search costs while enjoying low handling costs, encouraging a mo-

bile settlement strategy in which foragers search for high-

ranked resources and frequently move among the most productive resource patches.

At the opposite end of the spectrum, processors have

lower average foraging returns and broader diet breadth. They exploit many resources with high encounter rates but high handling costs. As a result, processors require

less search time but incur higher handling and processing costs that taxes, and thus occupy fewer resource

patches for longer periods. The model specifically assumes that either strategy is possible under identical environ-

mental conditions but traveler strategies are more effi-

cient when population sizes are small, whereas processor strategies are better when population densities are

comparatively high. Moreover, processor strategies pre-

vail if the two compete in the same resource distribution.

Making an inference from some archaeological evi-

dence of the western Great Basin, Bettinger and Baumbauch propose that ethnic Numic were processors, whereas pre-Numic foragers were travelers. Numic pro-

cessors experienced higher population growth rates be-

cause of a presumed bias in sex ratio toward women, resulting from the value of women's labor in processing expensive resources like seed. Therefore, as population growth among processors brought the two strategies (ethnic groups) in competition with one another, Numic-speaking processors displaced coexisting travelers.

The ability of the model to satisfactorily explain the Numic spread hinges on its supposition that Numic and pre-Numic cultures specialized in one or the other strat-

ey. Otherwise, evolution would select individual trav-

er who adopt the processor strategy when competing with processors, and select individual processors who revert to the traveler strategy when competition abated. In this scenario, no group-level selection could occur because individuals can defect to the opposing strategy according to their own best interest.

To build the constraints into the model, Bettinger and Baumbauch think of travelers and processor strategies as separate adaptive peaks, where a shift from one strat-

ey to another incurs a cost of suboptimal foraging effi-

ciency. This cost would come from other aspects of the adaptive systems (i.e., ideology and social organization), which must change to efficiently adjust to the subsis-

tence adaptation. Thus, although individual travelers may adapt by taking lower ranked resources, they would lack the appropriate organization to be competitive with processors. Sociocultural constraints on subsistence ver-

tainty could emerge from the effects of female infanti-

cede on sex ratios, to the ability to appropriately sched-

ule "green phase" seed harvesting to a bunching oriented mobility strategy (Bettinger 1992:53-54). Both travelers and processors resist change in subsistence strategies because of the cost to adjust the entire cultural system.


1996:150-160) has since linked the traveler-processor model to the theoretical framework of cultural inher-

ance theory (Boyd and Richerson 1985), but has never
explicitly recast it as a cultural inheritance model. Cultural inheritance holds that cultural behavior is subject to the process of natural selection. However, models of evolutionary biology cannot predict some consequences of cultural evolutionary processes, since the mechanisms of transmission and mutation (i.e., learning) differ from genetic evolution. If we read Bergner correctly, his explanation of the Numic spread relates to the process of indirect bias, in which an unenculturated individual prefers to copy the behavior of individuals who possess a particular trait. In the traveler/processor model, unenculturated processors prefer to copy the behavior of successful processors, whereas unenculturated travelers use success as a traveler as an indicator of which behaviors to learn. However, it may not be clear to the student which behaviors led to the indicator trait. Thus, the learner may copy a whole repertoire of behaviors that have nothing to do with the foraging success of the mentor. As a result, seemingly maladaptive behaviors may be selected within a cultural group.

Forces of indirect bias are particularly strong in cases of migration between adjacent groups bearing different indicator and preference traits (reflecting the different strategies of each group). In these situations, the state of indirectly biased behavior ensures that unenculturated individuals of one group will not mistakenly copy role models of the competing group. In the Numic/spread scenario, the preference of unenculturated travelers and processors for mentors who exhibited indirectly biased cultural behavior (e.g., spoke a particular language, had certain traits of female sex steroids, had a particular world view, manifested artifacts of a particular style) prevented individuals from copying the subsistence behavior of the opposing group, even if copying that behavior had been in the best interest of that individual. This scenario sets up circumstances where group selection can override individual selection (i.e., variability between groups is greater than variability within groups and isolation between the groups). According to Bergner (1994:13), this model justifies perception of ethnic groups as acting as "pseudo-species" rather than associations of self-interested individuals.

Curiously, few skeptics of the traveler-processor model have criticized its theoretical emphasis on group selection. Instead, critics have concerned whether the explanations of the migration fits the archaeological record (Eaton 1994:5; Simms 1985), whether potto-chronological analyses are reliable for inferring ethnic migrations (Grayson 1994b), and whether the traveler-processor model represents an appropriate use of optimality foraging theory. We confine our consideration to the latter issue because of its pertinence to Great Basin subsistence issues.

Simms (1983) points out that the model suggests greater differences in the subsistence strategies of Numic and pre-Numic foragers than the archaeological record can support. For example, plant macrofossil assemblages from caves and coworkers suggest that intensive use dates at least from the middle Holocene. Experimentally estimated return rates for some seeds suggest that stews were already exceptionally broad, fully qualifying pre-Numic foragers as processors rather than travelers. This criticism, while employing return rate data, also seems heavily on the recognition that such processidlike traits as thecoiled basket and the milling stone are "the hallmark of the Desert culture" (Jennings 1957:29).

Bettger (1991, 1994) counters this experimental return rate data with the experiment in which Numic and pre-Numic foragers procured these particular resources. By this argument, pre-Numic foragers may have taken low-ranked seeds only when circumstances allowed a substantially higher return rate than the experiments indicate (i.e., windows). In contrast, Numic foragers may have taken them, even when the return obtainable was exceptionally low (i.e., green phase procurement). Bettger's point that mode of procurement presents a significant challenge to using experimental return rate data to interpret archaeological assemblages is valid. Nevertheless, the archaeological fact that pre-Numic foragers took seeds and then the Numic spread explanation as the displacement of processors by better processors. The more similar the subsistence strategies of Numic and pre-Numic foragers are, the more difficult it is to conceive of them as separate adaptive peaks. This credibility results because optimal foraging models assume that individuals adapt their foraging behavior according to beliefs that subsistence efficiency accretes to reproductive fitness. Thus, diet breadth and patch choice should vary dramatically among individuals according to changing circumstances. Simms's (1987:43-45) simulation of diet breadth suggests that the reintroduction of Great Basin foragers upped from big-game hunters to broad-spectrum gatherers in response to short-term variability in resource abundance. Small changes in the abundance of high-ranked resources in Great Basin habitats will produce large swings in diet breadth (Simms 1987:45-46, fig. 5). Yet, to see selection of alternative subsistence-settlement strategies as accounting for ethnic spreads, the traveler-processor model demands that subsistence strategies be fixed at the level of the ethnic or cultural group, that behavioral variability within groups is low, and that variation between groups can be maintained. Otherwise, demographic feasibility between travelers and processors would override group selection.

Using the traveler-processor model in another context may help clarify the explanatory dilemma posed by diverging optimal foraging models from this theoretical underpinning. Simms (1983:8) points out that archaeological perceptions of pre-Archaic subsistence conform to a traveler strategy, whereas those of Archaic subsistence conform to a processor strategy. If this is
correct, the transition from pre-Archaic to Archaic can be modeled as a shift from traveler to processor strategies. But here, linguistic evidence has not already framed the issue as cultural historical description where the spread of one ethnic group replaces another with little interaction between the two. Should we perceive this subsistence change as the evolution of individual travelers to processors or the selective replacement of a group of travelers by a group of processors? By itself, optimal foraging theory offers no criteria for distinguishing the two alternatives.

Explanatory parsimony belongs to the former alternative over the latter. Even if natural selection favored some processor groups over traveler groups, by definition, at least one group of travelers must have evolved into processors. This transition resulted from selection of individual variability within the population and is the most straightforward explanation of the subsistence change. Any attempt to build group selection into the explanation incurs the onus of explaining why one group of travelers could evolve into processors, while other groups (exhibiting the same initial subsistence variability) were incapable of the transition. Further, the leap to group selection denies an exploration of connections among groups and demographically fluidity resulting from residential cycling among populations. Sometimes firm boundaries between groups exist and other times they do not. By assuming that culture mandates group selection, we ensure we will never understand why. These are the explanatory hurdles cultural inheritance must face if it is to become a general theory for investigating prehistoric subsistence variability.

PROGRESS IN INVESTIGATING GREAT BASIN SUBSISTENCE ISSUES SINCE 1981

Having reviewed the foundations of each research tack and theoretical perspective, we now consider research conducted in four subsistence domains in Great Basin archaeology. There are wetlands adaptations, alpine research, pithic procurement strategies, and forager-farmer interactions. We propose that the contributions of each tack interweave so completely that it is almost impossible to tease them apart (in some cases, single individuals have worked within the framework of more than one tack). We also describe the explanatory successes and failures with the hope of encouraging further discourse among the three tacks.

Wetlands Adaptations

The archaeological record of wetlands has always drawn the attention of Great Basin prehistorians (cf. Fowler and Fowler 1949), but efforts only began to shift from describing culture history to investigating wetland adaptations in the latter half of this century. Much of this research was done by scholars seeking to counter the Desert Culture model by demonstrating that wetlands sponsored a different culture because they were rich in resources compared to upland zones. Heizer (1956:77) summarized this perspective by describing Great Basin wetland adaptations as "limnosedentary," drawing archaeological evidence for the intensive use and consumption of palustrine resources from various case sites of the western Great Basin (Heizer and Napton 1970). Subsequent treatises drew support from more sophisticated models of wetland ecology, but continued to emphasize the abundance of subsistence resources available in wetlands (Jenness 1986; D. Madsen 1979, 1982).

Aided with the forager-collector model, D. Thomsen (1987) brought a more sophisticated understanding of forager settlement and mobility to bear on wetlands research that had been available to Heizer and his students (cf. Heizer and Napton 1970:43-44). His interpretation of Hidden Cave as a logistical site challenged the limnosedentary model by highlighting the contradiction inherent in the need to cache specialized gear for procuring wetlands resources by sedentary foragers residing in wetlands. Based on a consideration of ethnobiographic data from the Carson Desert, Thomas offered a "limnomobile" model of forager adaptation to wetlands. Marshes were the focus of a settlement system that maintained high residential and logistic mobility to procure resources elsewhere. Despite the emphasis on mobility rather than sedentism, Thomas's model shared the assumption that resource abundance accounted for the wetland adaptation.

X. Kelly (1985) offered a new perspective stressing resource quality rather than abundance by introducing optimal foraging models as a strategy for investigating wetland adaptations. He used Simms's (1987) post-consumer return rates to infer the relative productivity of various wetland and upland resources. Kelly observed that upland game animals were far more energetically profitable (in post-consumer return rates) than many wetland adaptations and predicted that intensive use of marsh resources and sedentary occupation of wetlands were unlikely except during periods of climatic stress on upland alternatives. Probabilistic survey of the Carson Desert seemed to support this hypothesis and the limnomobile model in general.

In hindsight, it is easy to criticize Kelly's application of optimality on several fronts. First, consideration of post-consumer return rates invokes the diet breadth model, which assumes that resources occur randomly in the environment. Yet by definition, marsh and upland resources occur in patches and are more properly considered in a patch choice model. Too, although resource abundance is not the whole story in understanding the economic potential of wetlands, it is not irrelevant. Kelly lacked the means to estimate resource encounter rates (a consequence of abundance) and was unable to calculate overall foraging return rates, the
condition that determines diet breadth and patch returns. While the use of a diet breadth model was a logical beginning, until other constraints pertinent to foraging tactics such as seasonality, sexual division of labor, central place foraging, storage, and transport costs were added to the model, the conclusions may have been premature. Given these limitations, it should not have been too surprising when catastrophic floods of the 1980s proved Kelly's conclusions erroneous (R. Kelly 1998) by exposing large complex archaeological sites bearing pit structures, storage facilities, human burials, diverse artifact assemblages, and subsistence residues of marsh exploitation.

Yet Kelly's work challenged earlier approaches and together with the rescue archaeology of large numbers of sites and burials exposed by flooding, provoked a maelstrom of research. Experimental harvests were designed to estimate the post-encounter return rates of wetland resources; that Kelly had not considered (Landstrom 1993; D. Madsen and Kirkman 1988; Raymond and Sobel 1990). Mid-range models of animal taphonomy were applied to distinguish cultural from noncultural faunal remains in wetland assemblages (V. Butler 1996; Schmitt 1988), and site structure models helped under the duration of occupation at palustrine residential sites (Simms and Heath 1990). These analyses were bolstered by the famous flood exposure of hundreds of human burials in wetlands of the Great Salt Lake, Carson Desert, and Malheur basins. These provided a new source of bioarchaeological data for investigating Great Basin subsistence and settlement (e.g., Hemphill and Larsen 1995; Larsen and Kelly 1995).

This research clarified why Kelly's theoretically based conclusions failed to account for the data at hand and led to the development of new models designed to surmount the shortcomings. Confrontation with the dilemma of considering resource abundance as well as cost of procurement was incentive for application of an innovative technique for estimating the composition and distribution of vegetation communities by soil type in Stillwater Marsh (Raven and Elton 1986). This technique was also applied to the entire Carson Desert (Zeana et al. 1995). These allowed formal application of the patch choice model in which a broad array of wetland, desert, and montane habitats were modeled as men's and women's foraging patches (Zeana 1996).

The model suggested that wetlands were consistently the highest ranked foraging patches for women (with upland alternative competitive only during productive union years or short-term fluctuations of marsh productivity resulting from drought or flood) and among the highest for men. However, the model indicated that men would find competitive hunting opportunities in the upland patches, particularly during mesic climatic conditions. Seizing on the conflict of foraging interests between men and women, Zeana (1996) applied a central place foraging model to predict whether residential base camps should occur in woman's or men's patches. The model indicated that foragers of the Carson Desert would receive the highest combined foraging returns rates by living near women's best patches, whereas men logically procured their best hunting opportunities. Given that women's best patches were usually in wetlands, the model predicted that wetlands would be the locus of residential occupation. These predictions were almost exactly duplicated by an independent model, based on transport costs, which differed only in seeing wetlands as more productive patches for men and uplands least productive for women (R. Kelly 1995, 1998, 1999).

The result is a theoretically based understanding of wetland resource use in the Carson Desert that draws empirical support from a broad array of archaeological data including site distributions (R. Kelly 1998; Raven and Zeana 1998), assemblage composition (R. Kelly 1998; Zeana 1996), and bioarchaeological data (Larsen and Kelly 1995).

Several years ago, D. Madsen and Janetski (1990b) took wetlands research in the Great Basin to task for falling victim to "euthen" debates. By this, they meant that limnocentric and limnolobics models of wetland adaptation tend to conceptualize continuums or forager behavior as opposite extremes rather than modeling variability within that continuum. D. Thomas (1990b) rightly criticized Madsen and Janetski's interpretation of history, partially because Basin archaeologists have used such dichotomies as heuristic devices for thinking about variability. Bettinger (1993a-f) suggests that the case of wetlands research has simply shifted "to a more subtle contrast between limnogood and limnobad," in which advocates of both camps call upon optimal foraging models for support.

But Thomas and Bettinger miss the point of Madsen and Janetski's call for modeling behavior from a "hierarchically ranked body of theory which allows us to predict human behavior in a situational context" (emphasis in original, D. Madsen and Janetski 1990b:2). We see optimal foraging models as providing a tool for investigating wetlands adaptations, to which scholars with various theoretical perspectives have contributed. Kelly notes that this has created a theoretical framework that anticipates how adaptations will differ in other Great Basin wetlands, by modeling how the subsistence-settlement behavior of foragers might vary in different resource landscapes (R. Kelly 1997:17-18). The conclusions of Kelly's first effort were undeniably wrong, but it is equally undeniable that our understandings of forager adaptations to wetlands in the Great Basin have benefited from the line of inquiry he initiated.

**Alpine Research**

Archaeological research has focused on high-altitude adaptations only since the discovery of alpine "villages" in the Toquima range (D. Thomas 1978b) and the White
Mountains (Bettsinger 1949 b). These sites contain rock-
ing structures, rough berm, rock-lined braids and cor-
age sites, diverse surface assemblages with abundant
ground stone and cemeteries, and diverse faunal and flo-
tal assemblages. The high investment in facilities ap-
parent on these sites suggests relatively prolonged peri-
ods of occupation. Thus, they appear to represent sea-
sonal, long duration, residential base camps of bands
that exploited a variety of plants and animals.
Village components, over a "previllage" mani-
festation of high-altitude resource use. Prevalence assem-
bles contain abundant faunal remains and hunting tools,
but little evidence for long-term occupation, food
storage, or plant-food processing. In contrast to later
villages with structures, the earlier components usually
are open mounds with fire hearths, lithic scatter, or
isolated artifacts associated with rock hunting blanks
and carins. Projectile points and bifaces dominate previllage assemblages; ground stone tools are present,
but significantly less common than in village assemblages
(Bettsinger 1948). Mountain sheep bone is ubiquitous
in previllage faunal assemblages, although small mam-
mal remains and plant macrofossils also occur (Grayson
1991 b; Sachar 1993). These characteristics suggest that
alpine land-use strategies of previllage foragers were
more mobile, more reliant on large game hunting and
less dependent on plant procurement than those of later
villagers (Bettsinger 1952:667-668). Prevalence assem-
bles may reflect a logistic strategy operating out of
lowerland base bases (Bettsinger 1952:249-481; Canady
1979; Delacorte 1990:142-147; D. Thomas 1983:289), a
highly mobile residential strategy of trek-
king through montane resource patches (Haggag and
Alpine villages in the White Mountains feature
Rogueate and Desert series projectile points, whereas
prevalence assemblages contain Elko and Little Lake se-
ries points. This suggests that the previllage to village
transition occurred sometime after 1300 B.C. (Bettsinger
1948). The alpine village of Atla Yagoma is associ-
ated with Desert series points (D. Thomas 1982b), hint-
ing that the transition may have happened somewhat
sooner there. The temporal range of projectile points in
both mountain ranges hints that the previllage-village
transition was roughly coincident with the timing and
direction of the Nemic spread inferred by glotto-
thronology (cf. Lamb 1958). A shift from a mobile, hun-
ing-oriented pattern to a residentially stable pattern in-
volving intensive plant procurement is strongly con-
sistent with the predictions of the travel-processor
core as marking the spread of Nemica speakers across
the Great Basin (Bettsinger 1918). For these reasons,
accessibility to the travel-processor model has en-
phased investigation of high altitudes under the suspi-
cion that villages are a hallmark trait of the spread of
processors through the Great Basin. Test implications
seen clear; the previllage-village transition should have
occurred on many Great Basin mountain ranges, with
the timing of the transition occurring progressively later
northward and eastward from southeasterns California,
and evidence of broadening diet breadth and patch
choice should accompany the transition wherever it oc-
curs (Bettsinger 1940:675).
Despite the straightforward appearance of the prob-
lem, most tests have produced ambiguous results. D. Thomas (1994:60) notes that the median radionuclide
date from Alta Yagoma is 440 B.C., whereas that from
villages in the White Mountains is only 345 B.C., con-
temporary to the temporal pattern inferred from projec-
tive point dating and contrary to the eastward expan-
sion expected for the Nemica spread (cf. Bettsinger
1918:675). The dates suggest that the development of
alpine villages was an independent process in both
ranges.
Using alpine faunal assemblages from the White
Mountains, Grayson (1991 b) predicted that village (i.e.,
Nemic) faunal assemblages should contain greater taxo-
nomic richness and diversity, and lower proportions of
large game than previllage (i.e., pre-Nemic) fauna. His
analysis did show that previllage faunal assemblages
contained significantly more large mammal remains, but
failed to find significant differences in either assemblage
richness or diversity. Grayson concluded that the faun-
local record of the village-previllage transition provided
only limited support for the travel-processor model.
In an analysis patterned after Grayson's, Sachar
(1991) analyzed flotation samples from Midway, an al-
pine village in the White Mountains. Like Grayson,
Sachar expected that previllage assemblages would be
taxonomically rich and diverse than village assem-
bles, but found no significant differences in either rich-
ness or diversity between the two assemblages. Simi-
larly, Sachar expected significant differences between the
abundance of different plant species, with greater pro-
portions of lower-ranked resources comprising village
samples than earlier samples. Sachar did find that
previllage samples had significantly greater amounts of
goosefoot whereas village samples had greater amounts
of pine nuts (although Sachar assumes that goose
foot is lower ranked than pine and concludes that the
pattern contradicts expectations of the travel-processor
model). Unexpectedly, Sachar notes that more plants
represented in high-altitude flotation samples are low-
land, non alpine species, and that the relative abundance
of alpine and lowland species does not differ between
village and previllage assemblages. This suggests that
alpine plant procurement may have been relatively un-
important for both village and previllage strategies.
Grayson (1991 b: 354) proposed an alternative expla-
nation for the occupation of alpine villages that finds sup-
port in Sachar's (1992) analysis. He suggests that alpine
villages represent an intensification of previllage alpine
exploitation strategies due to population growth, instead
of a rapid replacement of a travel strategy by a pro-
osso strategy featuring expanded diet breadth. According to Gravano, alpine villagers intensified their use of marrows and tender pine nuts during years of poor prior. Sarvis, a strategy expected on mountain ranges adap-
to densely populated lowlands like Owens Valley and Bishop River Valley.

D. Madzen (1995) considerable the validity of Gravano's test of the traveler-process model, arguing that divers-
ity, richness, and composition of faunal assemblages are inappropriate measures of diet breadth. Bouthnoff and Gravano (1999) concluded that the intent of the analysis was to test the traveler-process model as form-
ulated by Bettinger and Bahnshoff (1982, 1985) and not to test the dietary breadth model. Bettinger (1994:41) conceded with Madzen that faunal assemblages richness and diversity do not measure diet breadth, but reasoned that significant differences in faunal and floral assemblage composition are quantitative differences of dietary breadth between travelers and processed.

This exchange dashed doubts about a particular point that was never clearly articulated. The traveler-process model is both a model about the culture history of the Nunami spread and an optimal foraging model about subsistence change. Gravano's and Scaife's analysis suc-
cceeded in demonstrating that the traveler-process model has ambiguous implications for alpine faunal and floral assemblages. Their analysis, however, is not a test of the traveler-process model, but a test of a hypoth-
thesis about alpine subsistence change, using principles from optimal foraging, Gravano (1992:41:44) asserts that whether or not the Nunami spread occurred is irrelevant to testing "whether or not the adaptive changes (Bettinger and Bahnshoff) inferred ... actually hap-
penned" (Gravano 1992:41:44). Clearly, Gravano is concerned with the traveler-process model as O.F.T., not an evolutionary model of the selection of competing ethnic groups.

The theoretical problem that Madzen, Gravano, and Bettinger were struggling to articulate concerns the assump-
tions of cultural inheritance theory underlying the traveler-process model, and the utility of optimal for-
aging models for testing hypotheses about subsistence change. It is possible to test whether the inception of alpine villages is a broadening of prey and patch choice or a transition from travelers to processors (the lack of evidence from the White Mountains and Owens Valley seems compelling that this can be tested (Bisaghi and Giambautzzi 1993, Bisaghi and McGill 1988; Bettinger 1989, 1991b, Bettinger et al. 1982, Delcorre 1992)). The hypothesis that cannot be tested against alpine floral and faunal assemblages is whether the in-
ception of alpine villages represents selection of an im-
migrating group of processors over an indigenous group of travelers. This is an issue that optimal foraging mod-
els cannot address because cultural inheritance theory freezes subsistence strategies at a level of the group, whereas optimal foraging models assume that behavior is individually malleable. Thus, hypotheses drawn from optimal foraging models concerning subsistence change cannot distinguish whether individual processes evolved from travelers or a group of processors instead of a group of travelers.

Gravano's explanation of a transformation from a traveler to a processor strategy has found some empirical and theoretical support. Canaday (1997) sampled five Great Basin mountain ranges, finding evidence of villages in the Tonopah Range of central Nevada. Perhaps more surprising than the absence of villages, Canaday found prehistoric manifestations comparable to those of the Taquima Range and the White Moun-
tains only in the Taquima Range. Elsewhere, alpine sites of any sort are rare and almost exclusively mining re-
lated. He suggested that the co-occurrence of village and prehistoric patterns is, if not all, the Great Basin mountain ranges is evidence that they represent in situ adaptations rather than selective replacements.

Zeeman (1995) developed a transport cost model designed to predict optimal central place locations for prehistoric foragers in the White Mountains and Owens Valley. Montane regions offer the best places to live when foraging season rates were high and diet breadth narrow, allowing foragers to map onto the best foraging patches. Optimal central places were in lowlands when diets were broad enough to require herds of low-
land seeds simultaneously with hunting alpine game. In this scenario, task-specific hunting parties foraged expeditiously alpine zones from lowland base camps.

Alpine villages represent a third, more intensive strat-
ey of long duration residential occupation of alpine zones that becomes economical only under circum-
stances of broad diet breadth and population packing in the lowlands. High population densities fill lowland camp spots leaving only upland locations available for occupation by the growing population. This model is consistent with Canaday's findings because it predicts more than one prehistoric alpine landscape strategy and sees the prehistoric-village transition as an investi-
ation of high-alitude land use in response to broadening diet and population growth.

Regardless of the role population pressure may have played in shaping decisions, it fails as a prime mover. If alpine villages resulted from population pressure, then vil-
lages should consistently occur on mountains next to densely populated valleys, as in the White Mountains and Tonopah Range. Yet this does not explain the pre-
ence of villages in the Taquima Range, which separates Big Smoky and Monoite valleys. Nice did Canaday (1997) find evidence of prehistoric or village pattern in the Ruby Mountains, Mauled by the heavily populated Lamoille, Huntington, and Ruby valleys. Informant- R. Kelly's (1953) transport model at lowland vs up-
land resource use in the Carson Desert. Canaday (1997) suspected that the presence of extensive wetlands in a valley bottom may prevent alpine village or prehistoric
patterns from ever being competitive strategies. If so, this would explain why neither pattern is present in the Ruby Mountain (adjacent Ruby Marsh, but fails to explain why both patterns occur in the White Mountains, which are adjacent to rich wetlands in Owens Valley (for example Fish Slough). The population pressure hypothesis is not as useful to distinguish population growth from population migration as the travel-processor model. If high-altitude resources were of value to foragers adapting to local population growth, then they should have had the same utility for foragers coping with immigrating con-
testors. This is a classic case where a post hoc explana-
tion is substituted for a theoretically coherent hypoth-
is, inhibiting our ability to predict whether alpine vil-
lages occur in other mountain ranges, or explain why the distribution of known villages fails to correlate with lowland population density.

Candey's (1992) work shows that alpine archaeo-
logica|’s records in the Great Basin are too variable to be explained as cultural historical traits fixed at the level of ethnic groups. However, opting for population pres-
Sure as a prime mover is equally invalid. This does not mean that the transition cannot be modeled as a shift from traveler to processor strategies, or that popula-
tion growth and immigration were uninvolved in the transition. It does mean that understanding the alpine previllage village transition will require a theoretical perspective that can model how alpine resource use may vary from one to another range to another.

Pilon Procurement Strategies

Julian Steward emphasized the importance of pilon rates as a storeable food (1938:227-28) and a determinant of winter village location (1931:241) among ethnobotanic Great Basin foragers. For this reason, tests of the Desert Culture model emphasized assessing the antiquity of pilon procurement in the Great Basin (Bettinger 1976; D. Thomas 1935; D. Thomas and Bettngger 1976). Most studies analyzed site distributions in pilon woodlands under the assumption that statistically significant associa-
tions of assemblages with pilon-jumper zones reflect the antiquity of pilon procurement. This assumption was challenged because documenting site proximity with pilon woodlands without direct subsistence evi-
dence fails to prove use of pilons as a food, and contra-
dicts the infrequency of pilon macrofossils in lowland caves and coprolites (e.g., Madsen 1981; see also D. Tho-
mas 1984c; Bettinger 1987). However, a more concerted effort to recover pilon macrofossils has since bolstered inferences drawn from site distributions (Bettinger 1989; Rhodc and Thomas 1983).

The dilemma that these studies revealed was that pilon-jumper settlement patterns are geographically and temporally variable across the Great Basin. Sites in Reive River Valley cluster near the pilon-jumper zone from 6000 B.P., suggesting that Steward's cultural ecological assessment of the role of pilon in Great Basin adapta-
tions is valid back to the Middle Holocene (D. Thomas and Bettinger 1976). However, pilon woodlands in Owens Valley remain sparsely occupied (as much as 1400 B.P.), suggesting that foragers bypassed pilon until that time, and challenging the Desert Culture model (Bettinger 1977). The northward expansion of pilons through the Ho-
locene complicates the task of assessing spatial and tem-
poral variability among pilon procurement strategies. Pilon only achieved its modern distribution during the late Holocene (D. Madison 1986), requiring knowledge of when pilon arrived in any particular region before making inferences about the antiquity of pilon procure-
ment. Partially for this reason, paleoenvironmental re-
search has emphasized dating the appearance of pilon in pollen cores and packrat middens. Results have been surprising: pilon pine may have arrived in the Bonneville Basin as early as 7900 B.P. (D. Madison and Rhode 1990) but probably did not appear in the western Lahontan Basin until 1550 B.P. (D. Kelly 1985b; Wigand 1990).

Unfortunately, temporal variability in the distribution of pilons does not explain variability among archaeologi-
cal pilon-jumper settlement patterns. For example, pilon arrived in central Nevada by 6600 B.P. (Thompson and Harton 1983), about the time that occupation of pilon zones began in Reive River Valley. However, pilon is present in the White Mountains by 4900 B.P. (Jennings and Elliot-Fisk 1993), millennia before the association of sites with pilon woodlands is Owens Valley. Simmons (1984b) applied general principles of the diet breadth model to predict how ethnobotanic foragers modi-
fied their subsistence-settlement strategies in response to the arrival of pilon. Harvesting experiments (Simmons 1987; see also Barlow and Metcalfe 1996) reveal that pilon nuts yield higher calorie return sites than many seeds in archaeological cave deposits and coprolites pre-
dating the arrival of pilon. Thus, according to the ex-
pansion of the diet breadth model that foragers take high-ranked resources whenever they come across them, Simmons expected that Great Basin foragers incorporated pilon nuts into their diets as soon as they were locally available. Extrapolating this dietary prediction to an expectation about settlement patterns, Simmons predicted that occupation of pilon-jumper woodlands would be begin about the time of the local arrival of pilon.

This prediction fits the archaeological records of Reive River Valley and Grouse Creek, but not that of Owens Valley. Simons suggested that delayed occupation of pilon woodlands in Owens Valley may have been a consequence of excessive costs required to transport nuts to residential bases on the valley floor. Thus, Simons used the failure of the diet breadth model to account for the Owens Valley case as a basis for posing transport costs as another important selective constraint governing the economics of pilon use among Great Basin foragers and
causing the variation observed among archaeological site distributions in various pine woodlands.

Realizing the need for data, researchers began exploring the effects of transport costs on the return rates of various Great Basin resources, including piñon. K. Jones and Maden (1989) calculated the maximum transport distance (MTD) that a burdened carrier could transport a standardized volume of a resource before incurring a net caloric loss. They found that MTD ranged from 8.9 to 10 km for various resources and held that this had implications for forager site catchment and mobility strategies. Resources with high MTD (those providing high returns after handling and transport costs) were most likely to have been obtained logistically while resources with low MTD are more likely candidates for foraging procurement. For instance, piñon nuts (MTD = 8.9 km) are likely to be in the catchment of a site located 50 km from a piñon grove, but pinyon pine (MTD = 0.0) is unlikely to be, unless the site occupies a pinyon pine patch.

Rhode (1996) notes a discrepancy between the high MTD values of some Great Basin resources and the lack of evidence for ethnohistoric foragers actually transporting resources for distances exceeding 100 km. Rhode suggested that trade-offs between the returns obtainable from foraging locally and those gained by logistical procuring distant resources constrained the distance over which ethnohistoric foragers could economically transport resources. For example, foragers camped on a subsistence campsite should only have been willing to procure and transport piñon from remote groves if the return rates for doing so exceeded that of procuring locally available piñon. Of doing reduced the maximum expected transport distance of piñon to 15 km for foragers residing on a patch of subsistence camp.

Rhode suggested that resource transportability and diet breadth greatly affect central place location. He calculated that a group using both subsistence camp and piñon enjoys higher returns rates by camping on the subsisunce camp and transporting piñon (64 kg calories per hour) than by camping in piñon and transporting subsisunce camp (550 calories per hour). Therefore, optimal central place locations occur on resources with higher transport costs in situations where foragers simultaneously use two discrete resource patches with different transport costs from the same central place.

D. Metcalfe and Barlow (1992) and Barlow and Metcalfe (1996) modeled the extent of field processing for resources required to optimally obtain and transport the resource, depending on the round-trip distance. The model assumed that central place foragers maximize the utility of the package returned home compared with returns expended in field processing and transport. Resources consist of high utility parts (i.e., edible tissue, usable material) and low utility parts (i.e., bone, shell, inedible plant tissue) that can be discarded either at home or at the procurement location. The goal of field processing a resource is to increase the utility of a transported load by culling low utility parts. However, too much field processing reduces the number of trips that foragers can make to and from the resource patch. Therefore, foragers must trade off the number of trips and the utility of each load to optimize the return rate of resource transported to a central place.

The round-trip distance between central place and patch determines the extent of field processing worthwhile, with more processing expected as the distance increases. Obviously, the trade-offs for field processing and transport vary for different resources.

Barlow and Metcalfe (1996) use their model to predict field processing decisions for pinyon seeds and piñon nuts. They find that the rate at which overall return rates declined as field processing increased (assuming that resource processing at home is free because it incurs no opportunity costs) differed drastically between the two resources. The return rate for pinyon diminished from a high of about 1,500 calories per hour with no field processing to 500 calories per hour for minimal field processing. In contrast, piñon return rates declined more gradually, from about 5,000 calories per hour for no field processing to about 4,000 calories per hour for short transport distances at which initial field processing becomes worthwhile.

These findings hold implications for where central places are optimally located. Transport costs other subsistence strategies such as Danger and Hopgans caves (Harper and Alder 1970; Jennings 1971; D. Maden 1982a). These deposits held immense quantities of piñon seeds, chaff, and various plant parts, whereas piñon macrofossils are comparatively rare and are usually from bulbs. Barlow and Metcalfe (1996) note that the ubiquity of piñon in the caves is consistent with the extensive processing required to obtain relatively small quantities of seed at a camp positioned near piñon. In contrast, optimal transport of piñon from groves 20 to 40 km distant would require discarding of needles, cones, and scales in the grove, transporting only the seeds encased in hulls, or perhaps only the most mature, which do not preserve. They further note that a decrease in the number of cultural levels bearing piñon hulls at Danger Cave occurs about 6000 B.C., corresponding with the appearance of upland sites in the region. The shift in the representation of piñon at Danger Cave may reflect a shift in the location of central places and indicate the varying functions of Danger Cave in regional settlement systems before and after 6000 B.C.

This has implications for interpreting variability in piñon-woldwood settlement patterns elsewhere in the
Great Basin. For example, in both Reese and Owens river valleys the absence of piton procurement is inferred from the appearance of camps in piton woodlands. The presence of milling equipment, storage features, and residential structures on these sites suggests processing, storage, and consumption of piton at central place base camps, not piton procurement. Therefore, the presence or absence of piton camps in various piton woodlands may reflect a decision of whether to reside in piton zones, not simply a choice of whether to procure piton (4.41 Barlow and Metcalfe 1969; 55). Support for this inference is found in the presence of piton bulls in lowland sites in Owens Valley (Bassall and McGee 1988) and alpine (Scharf 1984) sites in the White Mountains that predate the appearance of camps in piton zones by many centuries. Thus far, no one has used transport models to test whether transport costs account for the archaeological record of piton procurement in any region, but such a test is forthcoming. R. Kelly (1985:236) predicts that the transport costs required for using piton at the Carson Desert, compared to the returns obtainable from wetland resources, would have prevented ex-b US. residential occupation at piton woodlands or logistic procurement of piton from being economical for foragers with the option of residing in Stillwater Marsh. This prediction differs slightly from that of Zesch and others (1985) who predict (without modeling transport costs) that residentia land and logistic piton procurement strategies would occasionally be economic, particularly during periods of stints on wetland patches. Kelly's survey and excavation data from the Stillwater Mountains will provide a format for testing these predictions, but given the paucity of milling stones in Stillwater Mountain piton woodlands (R. Kelly 1985b:12, 31), the scarcity of piton bulls at sites in the Carson Desert lowlands (Reinhart 1965:68), and the lack of evidence for piton consumption in sagebrush and nitrogen isotopes from human bone collagen (Schowengerdt 1993), it seems likely that Kelly's prediction will be supported.

The development of transport models in Great Basin subsistence studies illustrates the important role that general, explanatory theory plays in archaeological research. These models derive from evolutionary theory and bear clear implications for understanding variability in piton procurement strategies. However, they also have important implications for use distributions and assemblage composition that clarify earlier disputes about the relative validity of survey data vs. excavated data as evidence of prehistoric subsistence. Therefore, they qualify as middle range theory at its best by explicitly subsuming mid-range issues under higher order theory.

**Farmers and Foragers**

While attempting to perceive the Fremont of the eastern Great Basin as Southwestern farmers rather than Great Basin foragers, the variability of Moways expressed among the Fremont makes this characterization archaeologically recognize that the Fremont at least supplemented farming with foraging (Mawson 1970), and there has long been a sense that full-time foragers were contemporary with the farming period (e.g., Rudy 1945). Despite this recognition, what Fremont variability means in terms of actual behavior and the scales of time over which this variability was expressed has gone unanswered, in lieu of descriptions of unchanging variable and local constraints. By definition these entities exhibit little, if any, variability—what we recognize here as a premature assumption of group selection.

Taxonomic purity can also be maintained by definitional lines, classifying only farmers as Fremont (e.g., Tuleh and Rechen 1965:15-14, 1975). This position relegates contemporaneous foragers of the region to the status of Archaic, remnants, further altering archaeological foundations associated with alternative and history-as-progress, rather than looking toward the explanatory potentials of evolution and selection. Research on the Fremont however, shows signs of becoming better integrated with the theoretical advances we have described for forager archaeology in other parts of the Great Basin.

By the early 1980s, the documentation of Fremont variability indicated that reduced mobility required neither sedentism nor agriculture, and could be based instead on concentrated wetland resources (D. Madzen and Lindsay 1977; D. Madzen 1981). The notion that the Fremont could be less mobile without relying fully on farming fits nicely with the earlier descriptions of the Great Salt Lake Fremont as settled people who farmed less than other Fremont (Barnett 1975). The ethnohistoric record of the Utah Lake Ute, who occupied substantial villages much of the year without agriculture, supported the possibility that this may have been the case in other areas as well (Jantoski 1986b, 1990, 1991).

Mid-range research, especially focused analysis (Lapo and Schmitt 1977; Schmitt and Lupe 1975; N. Sharp 1985), and botanical analysis (D. Metcalfe and Heath 1990) provided archaeological evidence for the role of foraging in Fremont subsistence. On the other hand, stable carbon isotope analysis from skeletons found at large Fremont residential sites across Utah show that corn was as important to many Fremont as it was to many Americas, challenging the notion that the Fremont were only casual farmers (Collette 1993; 1997).

These studies moved understanding of the Fremont beyond the description of a farming people that also foraged. In turn, Basin archaeologists have tentatively included the Fremont as a variation on the linear-sedentary and limnemobile discussion in the western Great Basin (D. Madzen and Janetski 1990), foreshadowing a significant reap toward developing theoretical models about mobility that consider agriculture as simply another strategy among a pantheon. A recent study exemplifies our point.
Barlow (1997) provides data on the costs and benefits of farming relative to foraging. He finds that casual gardening produces returns similar to many arts and tubers exploited thousands of years before farming arrived in the region. More intensive farming practices produce returns similar to those of low-ranked seeds that were also taken thousands of years before farming. Thus, farming is not clearly better or worse than foraging when considered as a "package" decision. Barlow (1997) finds that processing and cultivation costs are significant constraints when harvest yields (quantities per unit area) are small, artichokes were over much of the Southwest. This means that the initial decision to farm revolves more around post-harvest processing efficiency and cultivation costs than the absolute abundance of harvests. Thus, Fremont foragers should combine casual farming with foraging as soon as the option became available, but should abandon farming only when the availability of higher ranked foraged foods declined.

Barlow's (1997) findings can be used to generate predictions that Fremont farming may have intensified in several different selective contexts. For instance, farming colonists should intensify their agricultural practices if competition with indigenous farmer-foragers constrained their foraging opportunities. On the other hand, if colonists did not encounter such competition, then they should adopt the mix of casual gardening and foraging expected for indigenous populations in the early Fremont. Certainly, Basketmaker period colonists would arrive with cultural baggage, but this alone is insufficient to explain the form that their culture took under the circumstances produced by their immigration. Indeed, subsistence and settlement data alone would not be able to distinguish colonists from indigenous populations, a point worth considering given the tendency of Basists to label cultures on the basis of subsistence.

Barlow's study (1997) is a significant step toward bringing the kind of subsistence modeling successful in other parts of the Great Basin to the Fremont case. Further, by identifying some of the constraints and pressures controlling Fremont agriculture, our understanding builds upon historical accounts of the arrival of farming from the Southwest via diffusion or migration. We now have a basis for understanding why these things happened and a basis to explain variability among Fremont farmers and foragers.

Fremont studies increasingly emphasize description and explanation of behavior across a range of alternatives (D. Madzen 1982, 1989; D. Madzen and Simms 1998; Simms 1986), and have led to greater attention to weak ecological patterns. The concept of adaptive diversity adds a temporal component consistent with advances in life history theory. Adaptive diversity is not just a term for the existence of different lifeways. It refers to behavioral versatility over the life spans of individuals that may include periodic shifts between sedentary and nomadic subsistence strategies. The concept thus highlights a "behavioral dynamic" within cultures and has been used as an improvement to the familiar metaphors of migration, abandonment, and depopulation (Rusbrooth and Upham 1992:52-57; Upham 1994). Archaeology along the eastern shore of the Great Salt Lake indicates the presence of contemporaneous, interacting systems of farmers and foragers evident in architecture, site type, assemblage composition, settlement patterns, projectile points, and ceramics (Simms 1998).

Stable carbon isotope analysis of skeletons from the Great Salt Lake sites indicates subsistence variability ranging, from full-time foragers to full-time farmers (Coltrain 1997; Coltrain and Stafford 1998). Stable isotope analyses the cumulative diet, yet the pattern holds across a sample of dozens of skeletons spanning the centuries before a.d. 900 and 1200 (Simms 1999). On the other hand, DNA analysis on the same skeletal remains reveals homogeneity in population genetics across this variability (O'Rourke et al. 1998). This suggests gene flow among people with widely differing subsistence patterns.

The findings in the Great Salt Lake case are inconsistent with the assumptions of group selection underlying the traveler processor model and cultural inheritance theory in that ethnic and linguistic affiliations do not necessarily limit adaptive strategies that may cross-cut these boundaries. It is well established that under conditions of less genetic isolation and greater gene flow, group selection is "a slow homogenizing process comparably with the 'cog-cut and thrust of individual selection'" (Dawkins 1976 cited in Krebs and Davies 1978; also see E. A. Smith and Waterhailer 1994:129-133). The high degree of adaptive diversity, combined with the genetic homogeneity across this diversity, makes this case an unlikely one to presume group selection in the absence of exploration of individual selection.

Comparison of the DNA from Great Salt Lake individual with Anasazi remains from southeastern Utah show genetic differences (O'Rourke et al. 1998, Paet et al. 1996). We point this out not to argue for the identification of the Great Salt Lake and Anasazi as relevant units of selection, but to acknowledge the possibility for group selection in cases where genetic isolation is evident. Whether the expectation of contrast between southern Fremont and Anasazi is sufficient to expect group selection remains to be seen, but discussions as to whether the boundary was permeable or not suggest that research is poised to move in this direction (Gear 1996; D. Madzen 1982, 1989; D. Madsen and Simms 1998, Simms 1999). We argue that the influence of group and individual selection on prehistoric subsistence change is an empirical issue that requires an evolutionary framework to approach, and that the a priori assumption of group selection by anthropologists hinders our search for the relative roles of cultural boundaries and traditions vs. the pressures for change within and between cultures.
Another significant change arising from farming forager research has been the consideration of subcontinental spatial scales to help understand the prehistory of regions. Painting a broad-brush picture, Upham (1993) suggests the prehistory of the Desert West as a "sea of nomads" (1994:17), "sprinkled with neolithic communities" (Childe 1936:87-98, cited in Upham 1994:17). Upham attempts a "purposive conflation of culture areas" (1994:17) and argues that variability across this large region is better seen as an "adaptive mosaic" (1994:17). This mosaic is unfathomable when cast as the interaction of cultural units, firmly bounded by tradition, ethnicity, and language. Rather, it is a behavioral dynamic between settled and nomadic subsistence-settlement strategies.

In the Great Salt Lake case, changes in the degree of adaptive diversity are consistent with demographic phenomena on a geographic scale larger than that of the Fremont. During the peak of Fremont farming in the eleventh century, adaptive diversity increased as agriculture spread and foragers cycled in and out of locally unstable farming systems. As farming systems scaled back between A.D. 1350 and 1500, adaptive diversity further increased (D. Madzen and Sims 1998; Sims 1994, 1999). The peak of Fremont farming and the period of dense would potentially have impacts upon the foragers of the Great Basin, a possibility that is obscured by the perception that while the "Fremont" stage came to the eastern Great Basin, the western and central Great Basin remained lodged in the Archaic stage. Attention to broader spatial scales suggests it may be worth considering the effects of massive demographic change in the Southwest and California during the last 2,000 years as shaping the context in which Great Basin foragers made their decisions. If so, the late Holocene in the Great Basin would be much more than a "Late Archaic," implying only continuities with previous periods and positions from other regions.

A significant implication of a multiregional scale is that forager-forager systems existed across areas larger than individual linguistic and ethnic groups. To be sure, these things exist, and in some areas with twin data sets and refined chronologies it may be possible to identify ethnic boundaries. For instance, Gebb (1996:68-113) searches for ethnic distinctions in the Anasazi-Fremont case while considering the processes that operate across regions. Tolbert and Riches (1996:197-198) report a case in northeastern Utah where indigenous foragers may have been the backdrop to small colonists of farmers—V. Geiden Chidioe's "sprinkling of feodalist communities."

A behavioral perspective in the context of broad spatial scales has simulated Fremont research in additional ways. Ceramics hold promise for examining how, when, and interaction spheres (e.g., e.g., P. Dean 1992; Gebb and Lynes 1996; Sims et al. 1997; Spurr 1995). By diverging ceramic study from a normative Fremont culture, it becomes possible to explore ceramic use, not just in terms of diffusion and contact among "peoples," but in terms of the circumstances of ceramic adoption and persistence. This perspective holds obvious value for understanding ceramic use by foragers in the Great Basin, regardless of whether this occurs across ethnic and linguistic boundaries.

The study of human-landscape relationships also reveals the impact of theory-based research and a behavioral perspective. Janetski (1997) employs optimal foraging models and advances in end-range study of faunal assemblages to argue for declining foraging efficiencies near the peak of the Fremont period. This trend is associated with the areas of greatest agricultural intensification, such as the large plains in the Millard County area in the inter-centruy Utah. In light of Baco's (1997) model of farming adoption, Janetski's (1997) findings help us to understand why we see rural Fremont area in some areas while intensification proceeds in others. Considering evidence for adaptive diversity, it is apparent that this variation is plausibly due to the life histories of individuals, and is not absolutely fixed by ethnic and linguistic boundaries. This does not deny the reality of "cultural factors," but means that they are subject to selective forces, not as unchanging slavers of tradition.

The presence of conflicting adaptive strategies stimulates exploration of how the spread of farming shaped forager decision-making. Gebb (1996:68-113) explores the norms of Fremont and Anasazi farmers to foragers determined the contours and opportunities available to foragers. Across the Desert West, the dynamic between these patterns and the nomadic was driven by the two demogaphic results of the spread of farming and sediment: continental population growth and encroachment, farming sites, locales, and subregions served as population sinks, or even magnets, that attracted surrounding foragers (Upham 1994:153-159).

D. Madzen and Sims (1998) attempt to organize these findings as "contexts of selection." They identify four contexts that reflect different circumstances shaping decisions: behavioral options, matrix modification, symmetry, and switching strategies. Rather than labels for mutually exclusive categories of people, the contexts of selection point to circumstances that varied across time and space. For instance, the arrival of farming to the Fremont region provides new behavioral options depending on whether indigenous foragers were adopting farming or migrating colonists moved into the region. They would also differ depending on whether the farming option was presented to nomadic foragers, or to logistic collection. Similarly, matrix modification points to the very different role that population density played during the early Fremont, versus the peak of Fremont farming in the A.D. 1200s, and the terminus of the pattern a century or two later. Rather than a prime mover, population density becomes a factor whose role
in the system changes markedly depending on the context of selection.

It is easy to presume that the relevance of these perspectives for Great Basin archaeology is limited to the last 2,000 years when farmers met foragers. However, if farming is seen as just one more force shaping mobility, the variation in mobility present in the prefarming or non-farming parts of the Great Basin suggests that adaptive diversity may be a process relevant to the mid- and early Holocene periods as well as the late Holocene.

Greater attention to behavior, and appeal to concepts such as adaptive diversity leads to the conclusion that farming may be profitably explored as a variant of Great Basin adaptations that can contribute to the development of general theory. The Fremont case also warns against the presumption that foragers are isolated, autonomous systems impervious to what is happening elsewhere on the continent. Examples ripe for investigation include the passive adoption of ceramics by Great Basin foragers and the patchy founding of alpine villages we discussed previously.

Integrated with theoretical advances resulting from the study of Great Basin foragers, the Fremont case holds promise for helping to tease apart culture change, demographic fluidity and behavioral plasticity, from the forces of cultural tradition and boundary maintenance that go to the heart of the difference between evolutionary ecology and cultural inhabitation theory.

DISCUSSION AND CONCLUSION

The years 1982 and 1983 saw Great Basin prehistorians turn away from the culture historical and culture ecological traditions that had characterized Great Basin subsistence studies until that time. In their place, they advocated three different research eacts, each drawing from a different theoretical perspective. Considering competition among the three tracks, 4000 (cf. Betteniger 1993) foresees the demise of the materialist orientation in Great Basin archaeology. We disagree. Despite the different predictions of the three tracks, we are impressed by the extent to which all three have contributed to research of subsistence issues over the last fifteen years, which confirms the theoretical primacy traditionally given to infrastructure.

There have been two important changes within the materialist program in Great Basin archaeology since 1982. First, dialogue among the three tracks has emphasized mid-range issues. Contributions of mid-range theory have strengthened the materialist foundation of Great Basin archaeology since 1983. Mid-range theory has been influential in the analysis of faunal remains, the costs and benefits of resource procurement and transport, mobility, site structure, assemblage analyses, and sampling. If judged only by the popularity of mid-range issues, Great Basin archaeology has changed little regarding general theory, unless one concludes that mid-range theory is dead or a variant of general theory has deeper roots to general theory than D. Thomas (1982) advocated when he introduced the concept as a research strategy in Great Basin archaeology. General theory drives the formulation of research questions and programs. It is not a method applied to every site survey, everyone's summer dig, or all data sets simply because we have data. The influence of general theory is more subtle than the methodological concerns of mid-range theory and is apparent in how archaeologists now choose research questions and interpret data. From this perspective, the attention that archaeologists give general theory has increased since 1984, producing healthy debate. Any discussion of fundamental principles results in a certain posturing, a degree of ideological fervor, and perhaps the appearance that there is more happening than there really is. On the other hand, our discussion of subsistence research shows there is ample evidence of change in the way Great Basin prehistorians investigate subsistence that is attributable to shifts in general theory. As evidence, we cite the growing role that optimal foraging models play in the research of all three tracks.

The second change concerns the nature of general theory itself. Clearly, current theory in Great Basin prehistory reconciles the conflict between historical and ecological perspectives by reference to evolution. In this there are significant differences in the perspective of evolutionary ecology and cultural inheritance. Evolutionary ecology draws more from cultural ecology whereas cultural inheritance draws more from culture history; evolutionary ecology emphasizes selection at the level of individuals whereas cultural inheritance advocates a greater role for group selection in determining human behavior. However, both assume that natural selection is the mechanism explaining change and variation in patterns of behavior and culture. Both direct attention toward ultimate (evolutionary) causation along with proximate (functional) causation (Goldsmith 1980:121; Hallinan 1982). It is this shared perception that allows both to contribute to research, despite their theoretical differences (see Betteniger and Richerson 1990:221–223).

None of these changes, however, sever the historical connection to a materialist emphasis on matters of infrastructure manifested by an interest in generalization, describing and explaining evolutionary change, and a systems concept of culture. Issues of subsistence and settlement thus remain central to Great Basin archaeology, but not because these are the only kinds of data available in the region, or because foraging bands are somehow more understandable from a materialist perspective than are complex societies. Subsistence-settlement issues remain essential because without sophisticated
Cultural Inheritance and Evolutionary Ecology: The Benefits of Comparing Examples on Evolution

Evolutionary ecology and cultural inheritance mark two significant changes in the materialist foundations of general theory. Unfortunately, they are often recognized only by their mid-range manifestations, as in the case of optimal foraging models, or through association with specific problems in prehistory such as the Nunami spread. This problem has kept much of the theory at the levels of technique and mid-range theory. However, to resolve these changes in general theory with mid-range issues obscures the shifts in perception and research design that come from a transition to general theory.

The central change offered by evolutionary ecology and cultural inheritance is the appeal to a mechanism of change other than a description of cultural variability (i.e., substance varies due to cultural characteristics of preference causing the wa-and-who to vary from the such-and-such culture). A mechanism for change is missing in cultural materialism, but is often mistaken for descriptions of relationships between the parts of culture and realized by the recognition that culture is an adaptive system. The unfortunate result of materialism without mechanism is that everything matters, and theory fails to produce better understandings of human behavior.

Evolutionary ecology and cultural inheritance propose selection as the mechanism for cultural and behavioral change, and the impact of this thinking is apparent not only in many of the studies we cite here, but widely among the literature in the region. These theories direct attention to the relationship between variation and pattern. Without documentation of variation there is no basis for recognizing pattern. Variability in culture can be continuously produced, and patterning is selected from that variability. Evolutionary ecology and cultural inheritance aim to explain evolutionary change by explicitly incorporating selection as a mechanism to generate models, hypotheses, and predictions for exploration. They make assumptions about the process of change instead of making an empirical generalization that infrastructure determines structure and superstructure as it characterizes materialism as expressed in cultural ecology and culture history. However, the inclusion of the mechanisms of selection does not lead to the demise of materialism. The Darwinian legacy is as much a materialist perspective on nature as was Marx's materialist conception of history (Harris 1968, 1979).

Another characteristic shared by evolutionary ecology and cultural inheritance is an effort to broaden the concept of environment to better reflect the relationships central to the science of ecology. There is a clear move toward modeling environment as all constraints external to the organism, not in terms of culture vs. environment. Now it is understood as frequently modeled as a laundry list of traits or types. It is no longer possible to categorize two adjacent central Great Basin valleys as the "same" environment simply because they have the same plants and animals, or even the same life zones. Environment is increasingly seen in terms of structure, frequencies of different kinds of attributes described in terms such as parchy, fine-grained, homogenous, or redundant. Structure is understood internally relevant to the constraints placed upon the organism under study, such as the periodicity of production, risk, the costs of information collection, and importantly, relative costs and benefits among alternative behaviors.

There are also differences between evolutionary ecology and cultural inheritance. Cultural inheritance conceptualizes cultural behavior as a distinct form of transmission that highlights the existing structures of culture and thus places great importance on the roles of tradition and social boundaries. This requires an assumption of group selection, which pressure that change must occur at the level of the culture as a bounded unit. In archaeology this typically amounts to assuming that archaeological cultures are the units of selection. Environment is modeled in more sophisticated terms than in traditional cultural historical and ecological approaches, but continues to be externalized to some degree from culture. The assumption about cultural boundedness, persistence, and historical trajectory.

Evolutionary ecology also treats selection as a general mechanism for the transmission of learned behavior. The search for explanation begins at the level of the individual organized into groups, and it involves group selection by cases that cannot be explained more parsimoniously. As such, evolutionary ecology is more morphetic than cultural inheritance. It deemphasizes culture as a bounded system because it sees selection as operating at the level of individuals with competing interests within cultures that change through structure and membership. Evolutionary ecology places fewer restrictions on change and thus tends to see culture as plastic for the purposes of exploring the impact of selection. This approach recognizes there is behavioral variability within cultures (not just between them), and that competition within groups is one of the forces driving cultural change.

We have referred to explanatory paradigms several times to link the different interests and advantages of evolutionary ecology and cultural inheritance. Ocean's razor tells us that we must accept the simplest explanation when competing explanations equally account for particular phenomena. For this reason, selection at the
level of the individual remains the primary force in evolutionary explanations of nonhuman behavior, and group selection is reserved for cases where individual selection does not appear to work. This brings us to the central distinction between cultural inheritance and evolutionary ecology, and a topic that seems widely misunderstood in archaeology — group vs. individual selection.

The appeals to cultural coherence theory, explicitly discussed in Bettinger (1982a, 1985), are group selectionists. There is a valid argument that models of cultural change must consider forces that select for the maintenance and persistence of language, tradition, and social organization; that is, the pressures for "cultural inertia" (Elliot 1984:15). It would be difficult to find an anthropologist who advocated that we ignore such matters.

The research potential of group selection in complex social organisms is not dead to biology (e.g., Wilson 1980, 1988) tot the potential for group selection to occur in humans has been shown (e.g., Richerson and Boyd 1991:8-91). Bettinger and Richerson point out that scholars of evolutionary biology as significant as "Darwin, William Hamilton, and Richard Alexander... have all been prepared to see a role for group selection in the special case of humans" (1996:219). We humbly agree and observe that a willingness to see a role for group selection is quite different from assuming group selection because we have precluded exploitation of individual selection or worse, because our own Occidental cultural values presume humans to be "special." Rather than presume group selection cannot apply to humans, we have argued here using the example of travelers and processors that not only is it routine to consider group-level effects in terms of selection at the level of individual, but that logic demands it be ruled out before group selection is assumed.

The irony of the misunderstandings that humans have about group selection is that much of the distinction between the research tasks of evolutionary ecology and cultural inheritance built down to different facets of the research process. Both are capable of addressing social issues, and provide opportunities to explore circumstances under which cultural inertia would be expected to be strong or weak. Whether culture is conservative or plastic is as dependent on the tyranny of circumstance as any other pattern of culture. To assume culture is one way or another in all situations simply because tradition and history exist merely explains culture away. On the contrary, these matters comprise an important empirical issue. Finally, it is well documented (and we have reviewed this here) that the contrasts in which group selection can operate are limited. This, and parsimony, demand that evolutionary ecology be privileged in the research process, reserving cultural inheritance for instances where there is evidence that group selection is operating.

Harmonizing Theoretical Tacks

We see many cross-currents in general theory since 1984 that the process of identifying contrasts among perspectives often discount. Our examples of substance research illustrate links between mid-range research and more general theoretical statements of evolutionary ecology and cultural inheritance. Nowhere is this more apparent than with optimal foraging theory: we hope we have shown that substance research conducted within all three tasks has drawn from the logic of optimal foraging models and that no single tack qualifies as "the" optimal foraging approach. As part of our effort to harmonize theoretical tacks and show that general theory can only be judged by the questions and research stimuli, we return to criticisms leveled against optimal foraging theory. The optimal foraging debate is useful as an illustration of our general message because so many have commented on it, and so many seem to use it implicitly if not explicitly. We do not intend to select a "winner" in responding to criticisms of optimal foraging theory, frankly, we disagree with D. Thomas's (1990:280) courtroom metaphor for optimal foraging rhetoric, where "the jury is clearly split off." Perceiving the research process as just one more case on the dockets where the only thing that matters is judgment day obscures the process that has clearly evolved since 1982. We illustrate this point by returning to the shortcomings of optimal foraging theory.

One problem with applications of optimal foraging models to archaeological issues concerns the accuracy of replication estimates as estimates of prehistoric foraging returns. This criticism is valid, but not as a dismissal of the research process. Indeed, quantification is selective, and values from experimental studies (e.g., K. Jones and Madsen 1989; Sims 1987) have often been employed loosely without consideration of the principles underlying optimal foraging theory. However, general estimates of return rates are powerful when applied to the eight issues (Sims 1987:241): for example when they take expected variability or error in the estimates into account by modeling various scenarios (e.g., Sims 1982:141; Sims 1987:72-83), when they compare classes of resources at an ordinal level (Raven and Elton 1981:106; Sims 1987:210), and when a search for robust relationships finds that predictions remain the same even if the estimates are wrong by large factors (e.g., Sims 1987:78-79). One of the reasons for working with models with explicit assumptions is to explore the shortcomings but also to explore where relationships may be more robust that such errors are irrelevant. Too, our discussion of research pertaining to substance domains in Great Basin archaeology shows that theoretical discourse in these matters increasingly emphasizes the relative economic productivity of various resources and resource patches. Scharff's (1991:5) assertion
that foraging behavior of individuals when the archaeologi
cal record is one of groups. This criticism is important as
an illumination of some of the methodological problems
raised by evolutionary ecology and cultural inheritance
theory. This critique confuses the distinction between
individual and group behavior, with that between
individual and group selection, and concludes that to
consider aggregate behavior one must employ
'group selection.' Nor does individual selection lead to
an expectation that individuals in aggregate must exhibit
a lack of cooperation or adaptive strategies, making it
difficult to apply optimization models. On the contrary,
individual selection is shaping the resource choices of
different individuals and similar circumstances will
be under pressure to evolve similar choices. The
comparative pressure of the group will depend on competing
interests and the consequences of living in a group, including
the protection of the group from predators or
competition for resources. This means that the group
behavior and the consequences of living in a group will have
different evolutionary outcomes, as discussed by
Williams [166:39-44] for a fundamental model on
the subject in evolutionary biology that seems widely misunderstood in anthropology.

Confounding with subsistence variability inherent in
the archaeological record will lead evolutionary
archaeologists to conclusions that are not consistent with the
optimization of foraging models as a research strategy. Research of the last fifteen
years, it is demonstrable that there is little support
for the idea of an optimal foraging strategy in
the archaeological record. As we discuss previously,
the lack of evidence for the existence of optimal
foraging strategies in the archaeological record does not mean that
human subsistence is not shaped by evolutionary
processes. The problem of distinguishing human
subsistence is that we have traditionally treated small
macromans at Galisteo Shells, the process of mid-range
research moves new variables and issues into view and
knowledge forward. The contribution of evolutionary
ecology is a more explicit specification of the general
theoretical assumptions guiding mid-range research.
Therefore, we disagree with Broughton and Gragson's
[1993:314] argument that mid-range research is
"largely unknown, unknowable, and irrelevant" to
attempts to test optimal foraging hypotheses against
the archaeological record. Instead, we see it as a challenge
posing to the mid-range research needed to reduce
ambiguity in the archaeological record.

We also referred to the concern that testing optimal
foraging models makes too many demands of archaeo-
logical data because they require knowledge about
the
criticism seems more of an excuse than an exploration for a solution.

Consider the diet breadth model. It predicts the exclusion or extinction of resources in diets based on the quantity and quality of those resources in the environment. As such, the diet breadth model is appropriate to test hypotheses about the appearance and disappearance of subsistence resources in the archaeological record (Simms 1981:146, 1985:124:8, 1987:16). More sophisticated foraging models such as the patch choice model and the marginal value theorem, require evidence about the past relative abundance of resources, something much more difficult to come by in the archaeological record (Simms 1981:146), but certainly not insurmountable given the success of patch choice models for understanding subsistence in the Stillwater Marsh (R. Kelly 1992b, Raven and Elston 1989; Zeana 1996: Zeana et al. 1995). Thus, any criticism of the applicability of foraging models to archaeological contexts must specify the model used, the questions asked, and the data used to test hypotheses.

Finally, we address a criticism that we would interpret as one of the "more deeply hidden general theoretical suppositions" mentioned by Bettger (1993:16) to policed correctness, "I suspect that down deep most Great Basin archaeologists believe that there is more to human behavior than time and calories..." Although debate often produces characterizations and straw men, the advancement of Great Basin anthroepology has never relied upon a distinction between those who reduce human behavior to time and calories vs. those who appreciate social explanations. The issue for moving the science forward is how do we approach the complexity that is apparent to every researcher who ever seriously worked in the region. We have described research illustrating the value of an empirical rather than a priori approach. "Surely the reason for the century long boom of evolutionary inquiry touched off by Darwin and Wallace is precisely that Darwinian theory accommodates a virtually limitless range of interesting, fruitful projects" (Bettger and Richerson 1996:222).

As for modeling substance behavior in the Great Basin there has been real advance since 1981, and as most Bassinists recognize, without knowledge of subsistence we remain as naked as we would be without knowledge of the habitats and climates of the past.

Note:
1. Note that this is precisely the situation confronted by Simms (1987) when he found that the diet breadth model failed to predict why Great Basin foragers should harvest seeds. It is worth reiterating that, contrary to Bettger's concern, use of a contingency model did not lead to an erroneous conclusion that seed harvesting was optimal according to monetary return rates. Instead, it falsified the hypothesis that seed harvesting would prove optimal in Great Basin environments where large game were rare. This finding provided justification for nominating another constraint (over winter survival) as responsible for the evolution of the behavior.
2. It would not be helpful to identify the various egregious applications of foraging models in the Great Basin.
over the past 15 years. Science advances by using the works that move the inquiry forward, regardless of the size of the increment.

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