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Human Paleoecology and Foraging Theory in the Great Basin

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INTRODUCTION

When the "new archaeology" emerged during the 1960s, those aspects of it that emphasized settlement patterns, technology, and subsistence on the empirical side, and ecologically oriented explanations of the human past on the conceptual side, found fertile ground in the Great Basin. There were two prime reasons for this above and beyond the nature of the archaeological record itself. First, this area had a long-standing tradition of incorporating paleoenvironmental analysis into archaeological research programs, a tradition rooted in the chronologically oriented work conducted by Cressman and Antevs in the 1930s (e.g., Antevs 1938, 1940b; Cressman 1942; Cressman et al. 1940; see the discussions in Cressman 1988:267–268; Haynes 1990; Rhode, Chapter 3). Antevs's subsequent analyses of the relationships between Great Basin climatic and human histories (e.g., Antevs 1948, 1952, 1953a, 1953c) provided a dominant theme in Great Basin human paleoecology for years to come, and required that archaeologists consider at least general aspects of the environments occupied by prehistoric peoples (see, for instance, the discussion in Baumhoff and Heizer 1965).

Second, there was Steward (1938) and his argument that physical factors, "rainfall, soils, topography, and climate" (1938:256), determined the nature, distribution, and abundance of subsistence resources, while available technology limited the quantity of these that could be procured. All else followed from these critical relationships: "The subsistence habits required in each region largely determined the size, nature, and permanency of population aggregates [which] in turn predetermined many, though not all, features of social structures and political controls" (Steward 1938:257). Steward's cultural ecology, it seemed, held the potential for understanding the full sweep of prehistoric human adaptations in the Great Basin.

Thus, the Great Basin offered a lengthy history of paleoenvironmental analysis along with an ecologically oriented conceptual framework that seemed to hold the potential for explaining events of the human past. There

was even a long-standing debate between two of the icons of Great Basin prehistory, Jennings and Heizer, over the Desert Culture concept, a notion that tied both of these themes together (e.g., Aikens 1970; Baumhoff and Heizer 1965; Heizer 1956; Jennings 1957, 1964, 1968; Napton and Heizer 1970). Everything the "new archaeology" seemed to need or want was already in place in the Great Basin, or close to it.

Though the situation would seem to have been ripe for the detailed incorporation of paleoecological data into Stewardian cultural ecological models, this did not happen. There were several reasons for this. Archaeological approaches to the prehistoric Great Basin that began to emerge during the late 1960s were primarily regional in scope, whether they relied most heavily on surface data (e.g., D. Thomas 1971c; M. Weide 1968) or on excavations (e.g., O'Connell 1971, 1975). They were also conducted in areas that had not been the focus of much prior work, archaeological or otherwise. One person simply could not do both the basic archaeology and the necessary paleoenvironmental research.

More important, however, is the fact that it is relatively easy to apply Stewardian cultural ecology to the archaeological record in the absence of detailed local paleoecological information. While Steward (1938) was deeply interested in such things as kinship, inheritance, and political structure, these things did not seem readily visible in the hunter-gatherer archaeological record at hand. As a result, when Stewardian cultural ecology was applied archaeologically in the Great Basin, it was, and still is, applied to very general attributes of the archaeological record—to, for instance, the general distribution and abundance of kinds of sites at a given time or through time.[1] It did not require much knowledge of past environments to make this approach seem to work. Indeed, in some instances archaeologists simply predicted what past environmental conditions should have been like were a given set of archaeological interpretations correct (e.g., O'Connell 1975:44–45).

The problems with Steward's cultural ecology are now well-recognized (Bettinger 1991a; R. Kelly 1995a). These flaws were, in fact, becoming quite obvious by the early

1980s, as Bettinger's (1980) cogent review makes clear. In 1982, O'Connell and others explicitly rejected Stewardian cultural ecology on the grounds that it did little more than describe relationships between human behavior and particular environmental contexts while failing to explain why these relationships take the forms that they do. They called instead for the application of explanatory theory drawn from evolutionary ecology, and in particular that branch of it called foraging theory, to the kinds of problems that Great Basin archaeologists had previously addressed with Stewardian cultural ecology.

Analyses that in one sense or another adopted the premises of foraging theory began to appear in the Great Basin literature in some number during the 1980s (e.g., Bettinger and Baumhoff 1982, 1983; C. Fowler and Walter 1985; D. Madsen and Kirkman 1988; Simms 1984a, 1985a, 1985b, 1987). These approaches now seem to be replacing Stewardian descriptive cultural ecology as an interpretive framework in this region (e.g., Janetski 1996; R. Kelly 1995b, 1997a; Raven 1990; Raven and Elston 1989; Zeanah et al. 1995).

Since this is the case, and since these studies form our focus here, we first want to stress our agreement with Bettinger's observation that "Steward (1938) produced a technoenvironmental interpretation of ethnographic adaptation in the Great Basin that is almost certainly correct in broad outline, if not in particular detail" (Bettinger 1991a:145). It is also undeniable, as Bettinger (1991a) has argued, that archaeological applications of Stewardian cultural ecology have led directly to the production of a vast body of basic archaeological and paleoecological knowledge.

We also want to stress that we strongly agree with O'Connell and colleagues (1982) on the value of incorporating foraging theory into explanations of prehistoric subsistence and settlement change in the Great Basin. We agree with this position for many of the same reasons they discuss, especially the fact that foraging theory holds the hope of explaining change through time and across space in a way that is dependent not on analogy but on the application of principles drawn from evolutionary theory. As we see it, though, few of these potential benefits have been realized.

As Bettinger (1993) has noted, many foraging theorists tackling Great Basin prehistory have essentially reworked Jennings's Desert Culture concept through the incorporation of patch choice and diet breadth models. One can easily take this a step further. Given that the Desert Culture notion essentially represents Steward projected back through time, it can also be argued that these archaeologists have essentially asked why Steward generally seems to have been right. Dealing with this issue requires attending to the processes that produced the patterns Steward observed, and foraging theorists have done this by applying cost-benefit models that often appear to provide testable predictions.

Not only are the differences in practice between Stewardian cultural ecology and many applications of evolutionary ecology less substantial than is often implied, but it is also not always easy to tell where one conceptual framework stops and the other starts. The cost-benefit analysis of human territoriality provided by Dyson-Hudson and Smith (1978), for instance, concluded that "a territorial system is most likely under conditions of high density and predictability of critical resources" (1978:25), as long as those resources are not so dense as to exceed the requirements of the populations involved (see the discussion in R. Kelly 1995a). Dyson-Hudson and Smith (1978) parenthetically note that their model parallels some of the ideas presented by Steward (1938:22), but in fact this contribution is largely Steward amplified (see, for instance, Steward 1937c:629 or 1938:254), with the addition of a cost-benefit analysis to explore the processes whereby the results observed and explored by Steward might have been produced.

In saying this, we do not mean to criticize the significant contribution made by Dyson-Hudson and Smith (1978). We do, however, argue that most previous applications of evolutionary ecology, and in particular foraging theory, to human contexts in the Great Basin represent less of a rejection of Steward than a logical outgrowth of what he was attempting to do. In this outgrowth, the emphasis has shifted from results to processes and from description to often quantitative predictive analyses. While some aspects of archaeological and anthropological knowledge may not be cumulative (Dunnell 1979), this has been cumulative growth at its scientific best.

That said, because of their emphasis on process and on quantitative models and hypotheses, explanations of aspects of Great Basin prehistory that are drawn from foraging theory place far more rigorous demands on our knowledge of past environments and on past human use of those environments than do Stewardian accounts. It is to this issue that we now turn.

SINGLE SITES, SINGLE TAXA: LARGE MAMMAL RELATIVE SKELETAL ABUNDANCE

One of the earliest applications of a foraging theory model to Great Basin prehistory stemmed directly from the work of someone who disparaged the use of these models: Binford. In his critique of evolutionary ecological approaches, including those presented by O'Connell and Hawkes (1981) and Winterhalder and Smith (1981), Binford (1983b:220) sneered that "anthropologists should know better than this" (see also Binford 1983a:219–220).

Yet at the same time, as one of us observed a number of years ago (Grayson 1988b, 1989) and as Bettinger (1991a) and Broughton (1995) have discussed in more detail, Binford's (1978a) analysis of the interrelation-

ships between "economic anatomy" and body part frequency in bone assemblages assumes that individuals forage across the differential opportunities provided by the carcass of a large mammal in the same way that evolutionary ecologists maintain that people forage across larger landscapes. As a result, it is no accident that some of the most insightful reanalyses of the economic aspects of Binford's model have been provided by foraging theorists (e.g., K. Jones and Metcalfe 1988; D. Metcalfe and Barlow 1992; D. Metcalfe and Jones 1988).

Accordingly, and even though it was not discussed as such, one of the earliest, best-known, and most detailed applications of a foraging theory model in the Great Basin was provided by D. Thomas and Mayer's (1983) analysis of the interrelationship between relative skeletal abundance (RSA) and body part utility in the Gatecliff Shelter Horizon 2 mountain sheep *(Ovis canadensis)* assemblage.

Taken as a foraging theory model, it is now well recognized that the kind of RSA analysis proposed by Binford (1978a) was incomplete. As D. Metcalfe and Jones (1988) discuss, while Binford was explicitly aware of the importance of issues relating to processing and transport, he did not attempt to quantify and incorporate them into his model. O'Connell and others (1988, 1990), for instance, documented that the nature of bone transport among the Hadza of northern Tanzania is determined by a complex combination of variables, including animal size, the amount of meat removed at a kill or scavenging site, distance from the base camp, and the number of people involved in the transport episode (see the review in O'Connell 1995b). In addition, Bartram (1993) found no significant relationship between skeletal part utility and RSA in his ethnoarchaeological sample of gemsbok *(Oryx gazella)* kill sites produced by the Kua of the eastern Kalahari. The reason, he observed, was simple: The Kua often stripped much of the meat from their prey and left the bones behind, resulting in a strong positive correlation between the amount of time spent processing animals at kill sites and the number of bones left at those sites.

Interpreting interrelationships between large mammal RSA and body part utility within archaeological assemblages left by hunter-gatherers thus requires control over a wide variety of variables. It is not hard to produce a measure of RSA itself (see Marean and Frey 1997 for an important discussion of this issue). Current approaches to the measurement of body part utility also seem adequate to the task, although, as N. Sharp (1989, 1992) has discussed, "currencies" other than meat, marrow, and grease may have to be considered as well (for instance, value as tools). We must also, however, be able to determine whether the assemblage we are dealing with comes from a kill or scavenging site, a "base camp," or something in between (some in-betweens are discussed in O'Connell and Marshall 1989). This fol-

lows from the fact that the economic meaning of any pattern in RSA in this context is dependent on what D. Thomas and Mayer (1983) felicitously termed "monitoring perspective." Further, it must be shown that post-transport processes, such as density-mediated bone destruction, have not played a role in producing the patterns involved, as seems to have been the case in the Gatecliff setting (Grayson 1988b; Lyman 1984, 1985, 1994:223–293).

Once we have accomplished all this, the *easy* part is done. Unfortunately, key behavioral variables relating to processing and transport costs, relatively simple to measure ethnographically, are now, and likely will remain, beyond archaeological visibility. How far were the Gatecliff mountain sheep transported? How many people were involved in the transport? How much meat was stripped from the bones before the transport began? Answers to these purely behavioral issues are critical to understanding RSA-utility relationships in the archaeological contexts provided by hunter-gatherers, but they remain outside of our grasp.

Given that this is the case, it becomes appropriate to ask whether any information on hunter-gatherer paleoecology can be extracted from archaeological RSA-utility curves even when it can be shown that the patterns they provide have not been produced by such processes as post-transport density-mediated destruction and when issues concerning such things as monitoring perspective can be resolved.

The answer would appear to be that they can, but only in limited ways. In particular, RSA-utility relationships can clearly be used as one of a battery of tools to test hypotheses about hunter-gatherer paleoecology, just as Broughton (1995) has done in his analysis of resource intensification, and associated resource depression, in prehistoric coastal California. It is not clear, however, that, on their own, they will be of much paleoecological value since the secure interpretation of the patterns they provide would seem to require that we know the unknowable. Thus, it is no surprise that 20 years after their introduction into hunter-gatherer studies, the analysis of relationships between RSA and body part utility have led to a tremendous increase in our understanding of post-depositional processes and to a much deeper understanding of modern (and observable) human behavior, but do not seem to have contributed much to our understanding of subsistence, and of interactions with the environment, among prehistoric hunter-gatherers.

MULTIPLE SITES, MULTIPLE TAXA: LANDSCAPE MODELS

At the opposite end of the spectrum from analyses that examine single taxa from single sites are studies that attempt to model the prehistoric use of entire landscapes through consideration of the full set of subsistence resources that were, or that might have been, incorporated into

the diet. Because these studies focus on the interrelationship between resource distribution and abundance, on the one hand, and the distribution of people across the landscape, on the other, they are more tightly linked to traditional Great Basin interests than are the RSA approaches discussed above or the resource depression models discussed below. This, and the fact that so much of the Great Basin archaeological record resides on the surface, has made landscape models highly appealing to Great Basin prehistorians. A recent significant example of such a model, yet to be applied archaeologically (and hence not discussed here), is provided by R. Kelly (1995b).

The first detailed model of this sort to be applied archaeologically in the Great Basin was produced by Raven and Elston (1989) to predict the "shape" (Raven and Elston 1989:6) of the archaeological record in the Stillwater Wildlife Management Area (Carson Desert, western Nevada). Following a research design outlined in conjunction with their initial testing program (Raven and Elston 1988; see especially Raven et al. 1988), Raven and Elston (1989) made two basic assumptions concerning prehistoric human adaptations to this landscape. First, they assumed, in good Stewardian fashion (and we do *not* mean this to be pejorative), that "the spatial and temporal distribution of food resources are the preeminent environmental variables conditioning the location of most forager activities" (Raven and Elston 1989:6). Second, they assumed that "the forager who captures the most energy, and does it with the least expenditure of time or effort, has a competitive edge over his less efficient rivals" (1989:6). This, of course, is a driving principle of foraging theory, and Raven and Elston (1989) operationalized these twin assumptions in terms of a patch-choice model (e.g., Stephens and Krebs 1986).

They began by using soil, hydrologic, and biotic variables to divide their research area into 34 "habitat types." They then determined the set of locally available resources likely to have been used by prehistoric foragers as well as the likely distribution of these resources across habitat types by season; they also used the ethnographic literature to distinguish those resources likely to have been taken by men or women. Next, they depended heavily on the data provided by Simms (1987) to rank their habitat types by energetic return rates for 28 taxa (excluding large mammals) by season and by sex of forager. Finally, they used these rankings to predict the general kinds of archaeological phenomena that should be found in each of these habitat types, given the assumptions of their model. Then, and only then, did they test their predictions against the archaeological record (Raven 1990). Many of these predictions were met but many were not.

Raven and Elston's pioneering Stillwater model led directly to the similar, but far more ambitious, landscape model produced by Zeanah and others (1995) for the territory used ethnographically by the Toedokado

Paiute. This 2.3 million acre region centers on Stillwater Marsh, but also includes surrounding highlands, including the adjacent Stillwater Range, Dixie Valley, and much of the Clan Alpine Range. Zeanah and others (1995) defined 77 habitat types for this region, each meant to represent "a set of biotic and abiotic characteristics that constrain prehistoric hunter-gatherers seeking to make prudent foraging and settlement decisions" (Zeanah et al. 1995:39). They provided detailed characterizations of these habitat types, used a broader set of resources (68) than did Raven and Elston (1989) to rank their habitat types, and generated a more detailed set of predictions concerning the kinds of archaeological phenomena that should be associated with these habitat types. They tested these predictions by combining the data presented by Raven (1990) with an additional 5 percent sample and found that about 70 percent of their predictions were met.

Landscape models of this sort require substantial paleoenvironmental knowledge and precise control over the archaeological record. We consider this issue next.

Controlling Environmental Change

Raven and Elston (1989) knew from the outset that they would be unable to control for environmental change. They assumed that during the time the record they were investigating was accumulating, there had existed "a suite of environmental conditions essentially similar to those prevailing today" (Raven and Elston 1989:162). As they explicitly recognized, this assumption was incorrect. Indeed, Raven and Elston's own research program revealed substantial hydrological changes in the Carson Desert during the past few thousand years (Raven and Elston 1988; see also the discussion in Grayson 1993:222–226).

Similarly, Zeanah and others (1995:15) noted that their "model landscape becomes progressively less accurate the farther back in time it is applied." Accordingly, they provided a lengthy and important, though very speculative, discussion of the possible interactions between environmental change and human foraging strategies in this region (Elston et al. 1995), but this is left largely unrelated to the data they collected. They were constrained to do this for several reasons. First, as with the Raven (1990) sample, the surficial archaeological record revealed by their work did not provide sufficient information to allow it to be placed in time with any precision. For instance, of the 70 sites discovered by Zeanah and others (1995), only 5 provided more than three projectile points. In addition, and as with Raven (1990), they simply did not have the detailed paleoenvironmental information needed to assess changing distributions and abundances of all potential subsistence resources through time and across space.

Lacking such information—a detailed archaeological chronology and detailed paleoenvironmental data,

including data on changing distributions and abundances of potential resources—it is not possible to know why particular archaeological predictions of these models were, or were not, met. Likewise, lacking this information, it is also not possible to take advantage of the true archaeological potential of landscape-level foraging theory models: the ability to account for change not only across space but also through time.

These observations are hardly new (e.g., C. Beck and Jones 1992; D. Thomas 1986a, 1990), but the challenge remains. In the absence of information that allows us to array archaeological phenomena across space and through time and to relate those phenomena to changes in resource distribution and abundance in the way required by landscape models, we are left with static accounts that simply are not all that different from those produced in the days of descriptive Stewardian cultural ecology.

If this challenge cannot be met, then convincing landscape-level accounts of the past may remain as general as that proposed by O'Connell and others (1982; see also Grayson 1993 and C. Beck and Jones 1997) to account for the history of seed-grinding technology in the Great Basin: when things got bad, diets broadened and seeds began to be utilized more frequently.

Even this, of course, represents a gain over our previous understanding. For Jennings (1957), early grinding stones at Danger Cave suggested essentially ethnographic lifeways some 10,000 years ago. Now, on the other hand, we seem to understand why these early grinding stones were there and why they are not found in similar abundances at this time elsewhere. This is not, however, the level of precision that landscape-level modelers have in mind, as Raven and Elston (1989) and Zeanah and others (1995) make clear.

Experimental Data on Return Rates

The paleoenvironmental record is, at least, accessible, though it remains to be shown that it is sufficiently accessible to meet the stringent demands of landscape models derived from foraging theory. Things are quite different, however, as regards the information on resource return rates that current versions of these models require.

Three sources have been used to provide this information. For vertebrates, Broughton (1994a, 1994b, 1995, 1997; see also Bayham 1979) has used body size as a proxy measure of return rates for all but the very largest animals. While this might well be appropriate for vertebrates within the context of certain resource depression models (see below), it is of little help to landscape models, which require that all resources be taken into account.

There are also ethnographically derived return rates available for a fairly wide variety of plants and animals (see the review in R. Kelly 1995a). However, only a very

small number of such rates are available for Great Basin resources (e.g., C. Fowler and Walter 1985), and we lack accounts of how some resources were processed in this area (see Barlow and Metcalfe 1996).

As a result, Great Basin foraging theorists have made very heavy use of return rates derived experimentally (e.g., K. Jones and Madsen 1991; D. Madsen and Kirkman 1988; D. Madsen et al. 1997; Raymond and Sobel 1990), and particularly those provided by Simms (1984a, 1985a, 1987). A separate set of experiments has been directed toward estimating the costs involved in transporting those resources (e.g., Barlow et al. 1993; Barlow and Metcalfe 1996; Brannan 1992; K. Jones and Madsen 1989; Rhode 1990).

Many archaeologists have questioned experimental return rate data on the grounds that they are produced by inexperienced individuals (e.g., Bettinger 1991a; Weaver and Basgall 1986). Barlow and Metcalfe (1996), for instance, lost 47 percent of available pickleweed seeds during plant stripping and threshing and noted that if aboriginal foragers were better at it, their return rates after plant collection might have been double those which they calculated. As Bettinger (1991a:104) has observed, it is entirely possible that "resources wholly unproductive to the novice are capable of producing much higher yields than those resources that the novice finds most productive."

Simms (1988a) himself suggested that landscape-level foraging theory models meant for archaeological use might best be built using broad classes of resources (e.g., small seeds vs. large seeds and nuts). It is, in fact, one of the strengths of both the Raven and Elston (1989) and Zeanah and others (1995) models that they tried to avoid "misplaced specificity" (Raven and Elston 1989:136) by converting Simms's (1987) return rates into return rate classes, and that, as a result, their models rely only on ordinal level return-rate estimates. Bettinger's point, though, is that experimental data may even get the rank orders of return rates wrong (see also D. Madsen et al. 1997).

It is possible that some classes of resources can be correctly rank-ordered in this fashion, just as Zeanah and colleagues (1995) argue. For instance, even if Barlow and Metcalfe (1996) had not lost a single seed during their processing experiments, pickleweed return rates would still have been extremely low relative to other potential resources. Everything we know suggests that small seeds usually provide low returns to hunter-gatherers.

Even here, however, there may be problems. Once again, it is Bettinger (1993) who has identified the issue. "Often," he observed (1993:52), "it is the mode and circumstances of procurement" rather than the resource itself that determines return rates. He made this observation in the context of Danger Cave, noting that if the pickleweed seed so well represented there had been gathered from windrows, rather than having been harvested from individual plants, the return rates provided

by those seeds might have been very high. D. Madsen and Schmitt (1998) have recently made the same point in a more detailed way. The ranking of a resource is not intrinsic to the resource itself, but results from the interaction of resource, technology, and applications of that technology. In a general sense, of course, Steward (1938) was well aware of this: technology was key to his assessment of human-resource interactions, a point stressed by modern foraging theorists as well (e.g., E. A. Smith 1991).

The problem here is obvious. Both ethnographically and experimentally derived return rates have an unknown and unknowable relationship to return rates that characterized the past. Landscape-level models as they have been constructed to date require ordinal level measures of past return rates. This, in turn, requires that we know how the resources involved were taken, where there were taken, when they were taken, and how they were processed; they may even require that we know whether they were taken by men or women, and the season during which they were taken. This poses a significant hurdle for those models, whether or not they are based on surface data.

Controlling Technological Change

Closely related to these issues is the fact that technology itself evolves. Indeed, one of the great values of the archaeological record is that it allows us to study the evolution of technology in a rigorous way (e.g., C. Beck 1998). Subsistence technologies, we assume, compete with one another on the basis of extraction efficiency. If technologies change, extraction efficiencies—that is, return rates—must also change.

To take some obvious examples, the atlatl and dart were replaced by the bow and arrow in the Great Basin some 1,500 years ago (R. Kelly 1997a). Many have argued that Great Basin basketry technologies changed dramatically during later prehistoric times (e.g., Adovasio and Pedler 1994; Bettinger 1994; Bettinger and Baumhoff 1982), and only during later prehistoric times did ceramics arrive in the Great Basin (e.g., Rhode 1994). All of these significant changes may have had substantial impacts on return rates (on the bow and arrow, see S. Hughes 1998; R. Kelly 1988b, 1990, 1997a; Shott 1993; Speth and Scott 1989; on basketry, Bettinger and Baumhoff 1982; on ceramics, Janetski 1990b; Reid 1990; on the possible impacts of cooking technology in general, Gifford-Gonzalez 1993). Yet, to our knowledge, there have been no attempts to link any of these very visible changes in hardware to landscape-level foraging theory models.

Even this, however, would be the easy part, since different applications of the same technology to the same resources can produce very different returns from those resources. These behavioral—software, if you will—issues remain beyond us, as C. Beck and Jones (1992) have noted.

SINGLE OR MULTIPLE SITES, MULTIPLE TAXA: RESOURCE DEPRESSION AND INTENSIFICATION MODELS

Many Iroquois seemed to have moved their villages every decade or two in response, at least in part, to local firewood depletion (e.g., Tuck 1971; J. Wright 1966). In western Wisconsin and central Minnesota, Hickerson (1965) argued, hunting activities of Chippewa and Sioux led to the severe reduction of deer *(Odocoileus hemionus)* populations in all areas except "buffer zones" that were contested and hence dangerous to enter. Commercial harvesting of beaver *(Castor canadensis)* in the northeast, Hunt (1940) maintained, led to the local depletion of these animals and then to a burst of expansionist activities by Iroquoian groups (but see also C. Martin 1978). After a pronghorn *(Antilocapra americana)* drive in Antelope Valley, eastern Nevada, "the old men never expected to see another one at this place, for it would take many years for the animals to increase in sufficient numbers to make it pay to drive" (Egan 1917 in Steward 1938:35).

These examples, and hundreds of others like them, address issues of resource depression, the situation in which the activities of a predator lead to reduced capture rates of prey by that predator (Charnov et al. 1976). Although these reduced capture rates may be due to avoidance behaviors on the part of the prey ("behavioral" or "microhabitat" depression), most instances of resource depression addressed by archaeologists are likely due to harvesting, or "exploitation" depression.

While we term the approaches we discuss here "resource depression" models, the authors of these studies have referred to them as resource intensification models. In every instance, however, these authors have not only identified resource intensification, but have explained that intensification by rejecting all explanations other than resource depression to account for it. Because the patterns they have discovered can result from other processes—climatic change, for instance—we refer to them as resource depression models.

Foraging theory-based applications of the resource depression concept follow from the prey-choice model. This model demonstrates that, given certain assumptions, the most efficient strategy for a forager to pursue is to take high-ranked prey within a patch whenever they are encountered; the use of low-ranked prey will depend on encounter rates with higher ranked ones. Since encounter rates depend on prey abundance, lower-ranked resources will enter the diet as the abundances of higher ranked prey types decline. Mobile foragers might avoid this reduction in foraging efficiency by using more distant patches with greater abundances of high-return resources and incur the added travel costs, or, if central-based, by relocating on a more permanent basis, thus incurring relocation costs. If foragers are constrained in their mobility, however, or if travel or
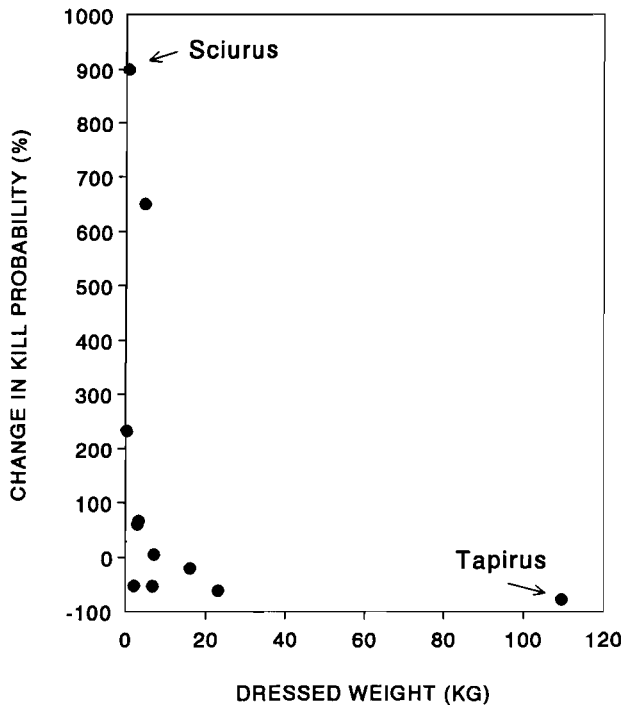
Figure 12.1. The relationship between dressed weight and change in kill probability for a Siona-Secoya village: mammals (data from Hames and Vickers 1982).
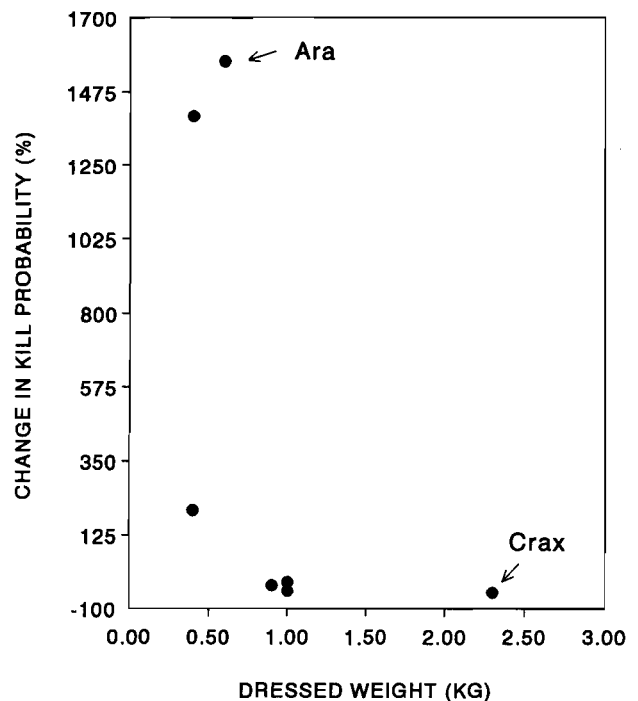

Figure 12.2. The relationship between dressed weight and change in kill probability for a Siona-Secoya village: birds (data from Hames and Vickers 1982).

relocation costs are too high, then resource depression among high-ranked prey types should result in resource intensification, or a greater amount of time or energy spent harvesting lower return resources.

Quantitative ethnographic examples of resource depression are not hard to find. What seems to be a classic example is evident in the data provided by Hames and Vickers (1982; see also Vickers 1989) from hunters working at two different periods from a Siona-Secoya village in northeastern Ecuador. The first of these periods pertains to a time (1973–1975) shortly after the village had been established; for the preceding 25 years, this area had seen only minor human hunting pressure. The second set of data they provide comes from some five years later (1979). For each period, Hames and Vickers (1982) provide "dressed weights" for hunted mammals and birds, the number of observed hunts, and the hunting success rate for each species.

We have converted these success rates to changes in kill probabilities; Table 12.1 provides selected data for mammals to illustrate this conversion. In Figures 12.1 (mammals) and 12.2 (birds), we plot the change in kill probability against the dressed weight of the taxa involved. In doing this, we follow Hames and Vickers (1982) in assuming that return rates scale to body size in these sets of animals, an assumption for which they provide anecdotal data and which, as we discuss below, now has more general support.

Figures 12.1 and 12.2 make it clear that for both mammals and birds, success rates for larger taxa de-

clined dramatically across the brief period of time involved. This would appear to be resource depression in action.

At the time they conducted their field work, Hames and Vickers (1982) were unaware that the prey choice model predicts that lower-ranked taxa should be incorporated into the diet as encounters with higher-ranked resources decline, and that diet breadth may expand as a result. Hence, they made no special attempt to collect data relevant to this prediction. They did observe, however, that one smaller artiodactyl, brocket deer (*Mazama* sp.), which was traditionally defined as inedible and not consumed during their first set of observations, had entered the diet in 1979. Thus, it does seem that diet breadth expanded as a result of, or at least in association with, declining encounters with larger prey.

If we picture this particular village from an archaeological perspective, there is good reason to believe that the faunal assemblages that accumulated here would provide access to these changes. If we can gain such access, then we might be able to better understand not just changing human use of the landscape but also the ways in which people structured that landscape and responded to changes that they themselves had caused.

Landscape models would seem to require that we integrate archaeological sites, paleoenvironmental data, and a full range of actual and potential subsistence resources across substantial amounts of space. Resource depression models, however, can be applied to either single stratified sites or sets of well-dated sites, and to

TABLE 12.1
CHANGING HUNT SUCCESS PROBABILITIES FOR SELECTED MAMMALS AT A SIONA-SECOYA VILLAGE

		Kill Probability		
	Dressed Weight (kg)	1973–1975*	1979†	Pct. Change
Tapir	109.5	4.5	1.0	-77.8
White-lipped peccary	23.1	31.8	12.1	-61.9
Collared peccary	16.0	28.7	22.7	-20.9
Howler monkey	7.0	3.8	4.0	+5.3
Agouti	3.0	3.8	6.1	+60.5
Squirrel	0.7	0.3	3.0	+900.0

* based on 286 individual one-day hunts
† based on 198 individual one-day hunts
data from Hames and Vickers 1982

subsets of the full resource base. It is thus not surprising that archaeological applications of foraging theory models have had the most success in this realm. This approach is fairly far-removed from Stewardian cultural ecology, though, and it is likely for this reason that is has not gained as much attention in the Great Basin as have landscape applications.

It also not surprising, given the difficulties involved in assessing the prehistoric use of plants (e.g., Barlow and Metcalfe 1996; Gremillion 1997), that these models have for the most part been applied only to vertebrate resources (e.g., Harvey and Broughton 1996 and Janetski 1997b from within the Great Basin; Hildebrandt and Jones 1992, Szuter and Bayham 1989, and, especially, Broughton 1994a, 1994b, 1995, 1997 from without). Related models have, however, been applied to plants outside of the Great Basin, spurred by Basgall's (1987) important discussion of the history of acorn use in California (e.g., Wohlgemuth 1996; see also Raab 1996).

None of this is to say that the application of resource depression models does not require substantial knowledge of the past or that these applications might not be problematic in a number of ways. For instance, such analyses require that we solve a wide variety of sampling, quantification, and taphonomic problems that plague archaeological faunal analyses in general (e.g., Grayson 1989; Hockett 1993; Schmitt and Lupo 1995). Besides these more basic issues, however, resource depression models come associated with a significant set of problems of their own.

Measuring Return Rates

As we have mentioned, landscape models require consideration of all resources that are, or might be, in the diet. Accordingly, they have been built largely by applying return rates derived experimentally. Quantitative resource depression models have so far been restricted to vertebrates, however, and have avoided the assump-

tion that experimental return rates can be applied prehistorically. They have done this by assuming that, up to a point, vertebrate return rates scale to body size, and that body size can, as a result, be used to rank order vertebrates in terms of their return rates. To our knowledge, this argument was first made by Bayham (1979) and it does have considerable support from both ethnographic and zoological research (Broughton 1994a, 1994b, 1995, 1997; Stephens and Krebs 1986). The relationship may not be universal, however, and often depends on characteristics of the individual prey taxa involved (e.g., Hawkes et al. 1982; E. A. Smith 1991). More problematic is the assumption that all vertebrates were taken individually or in equivalent packages of individuals, as we discuss shortly.

Measuring Harvesting Efficiency and Resource Depression

If we accept that, up to a point, larger animals provide higher return rates and that higher-return rate taxa will be the prime target of human predators, a harvesting efficiency-resource depression measure is readily built. Bayham (1979, 1982) again led the way here, suggesting that ratios of large to small mammals in archaeological faunal assemblages provide a measure of "selective foraging efficiency" and thus allow access to issues regarding resource depression (see the discussion in Szuter and Bayham 1989).

This measure assumes that decreases in the relative abundances of large mammals in archaeological assemblages through time must necessarily reflect decreases in their abundance in the surrounding landscape. Unfortunately, that is simply one of two options. The other option is that they reflect increases in the use of smaller taxa while encounter rates with, and use of, larger taxa remained the same, or even increased at a rate slower than the rate at which the use of smaller taxa increased. Precisely this response might occur if resources in nonvertebrate parts of the diet failed (thus increasing

the probability that smaller animals would be pursued) or if smaller animals began to be collected in groups (thus increasing their return rates). As a result, the behavior of this index *by itself* cannot tell us whether or not resource depression has actually affected the vertebrate component of the diet.

For instance, in their analyses of resource depression, neither Broughton (1994a, 1994b, 1995) nor Janetski (1996) demonstrates that changes in nonfaunal aspects of the diet could not have led to an increase in the use of small mammals, and thus could not have led to the appearance of resource depression in the absence of a true decline in the abundance of artiodactyls. One might argue that, since hunting is tightly associated with males and gathering with females, there are no interrelationships between returns from hunting and those from gathering. However, even if the relationship between gender and prehistoric resource procurement were known in a particular prehistoric case, this argument would still not find ethnographic support (e.g., Hawkes et al. 1982).

Thus, it is essential that resource depression hypotheses be tested using data unrelated to the efficiency index itself. This is exactly what Broughton (1995) did through an analysis of changing age structures of the Emeryville larger mammals (see also, Broughton 1997). Lacking such independent assessments, the meaning of changes in the values of harvesting efficiency indexes will remain unclear.

Measuring Diet Breadth

Resource depression models predict that lower-ranked resources will be incorporated into the diet as encounter rates with higher-ranked resources decrease, and that diet breadth may expand as a result. However, applications of these models do not require a measure of diet breadth, or "the total number of resources in the diet" (Kaplan and Hill 1992:171); they simply require showing that encounters with high-ranked resources declined through time. Indeed, published applications (see references above) do not include such measures and there are good reasons for this. While it does appear possible to measure some aspects of diet breadth archaeologically (Broughton and Grayson 1993; Grayson and Delpech 1998), it does not currently appear possible to measure those aspects of diet breadth that relate to plant utilization (see, for instance, the important discussion in Barlow and Metcalfe 1996). In addition, it does not necessarily follow that the number of resources actually incorporated into the diet will increase as high-ranked resources decrease in abundance (e.g., Winterhalder and Goland 1997). As a result, while an increase in diet breadth associated with a decrease in the archaeological representation of large-bodied taxa might be taken to support a resource depression model, the absence of such a response would be equivocal.

Controlling Environmental Change

In many parts of the world, changes in the abundance and distribution of larger mammals are well known to be correlated with climate change, sometimes over very short periods (see, for instance, Delpech 1997). Accordingly, it must be demonstrated that any changes in the archaeological relative abundance of high-ranked taxa are not caused by environmental change unrelated to human foraging activities.

To date, this has been accomplished by arraying changes in the resource depression index against various measures of climatic change. For the San Francisco Bay area, Broughton (1994a, 1995) examines correlations between precipitation and temperature indices derived from local pollen data with his measure of harvesting efficiency, and concludes that climate change cannot account for the patterns he has found. Janetski (1997b) examines Fremont sites (ca. A.D. 300–A.D. 1300) from the eastern Great Basin and northern Colorado Plateau, and thus deals with a far larger area than did Broughton. His evaluation of the possible interrelationships between climate change and artiodactyl abundance during this period of time relies largely on the assertion that climates were favorable for artiodactyls. Unfortunately, finer-scale paleoenvironmental data and a much deeper assessment of the relationship between artiodactyl abundance and climate change than Janetski (1996) provides are needed to make this conclusion compelling. Left unexamined is the possibility that environmental change made the collective harvesting of lagomorphs more profitable. At least, however, these kinds of data are potentially within our reach.

Controlling Technological Change

It is far more difficult to assess the impact of technological changes and changes in the applications of that technology. These issues are just as critical for resource depression models as they are for landscape approaches (see above). On the one hand, technological change can alter the post-encounter return rates of prey taken individually. Both Broughton (1994a, 1995) and Janetski (1996) address this issue, dealing explicitly with the introduction of the bow and arrow.

On the other hand, changes in technology, or in the way that technology is applied, might make it possible to take small prey items in mass that were previously taken individually, and thus increase their return rates dramatically. For example, Janetski (1996; see also Harvey and Broughton 1996) measures the relative abundances of artiodactyls as the ratio of artiodactyl specimens to the sum of artiodactyl and lagomorph specimens. Accordingly, he must, but does not, show that changes in the ways in which lagomorphs were hunted did not increase the return rates provided by

those animals (Bettinger 1993; D. Madsen and Schmitt 1998). If this occurred, then decreases in the relative abundance of artiodactyls might reflect the increased profitability of lagomorphs, and not artiodactyl resource depression.

Perhaps demographic analyses of the lagomorph assemblages themselves could be of assistance here, since communal hunting of these animals would likely produce a catastrophic demographic profile (e.g., Klein and Cruz-Uribe 1984). Perhaps the *Lepus* and *Sylvilagus* components of the lagomorph assemblage could be examined separately, since these animals differ in the degree to which they can be taken in mass.

As Janetski (1996) correctly notes, in the absence of secure knowledge as to how particular faunal resources were taken, this issue is problematic. Although some authors (e.g., Oetting 1994b) seem content to infer mass collecting from the abundance of the animals themselves, this approach simply assumes that which we should be trying to learn. That an animal is abundantly represented in an archaeological fauna means only that it is abundant: it does not tell us how it came to be that way.

Controlling Settlement System Change

In their treatment of resource depression in the prehistoric Southwest, Szuter and Bayham (1989) propose that the role played by Ventana Cave in settlement and subsistence strategies changed through time. Specifically, they argue that whereas more mobile Middle Archaic foragers used this site as a residential base camp, more sedentary Hohokam hunters used it as a logistic hunting camp.[2]

Regardless of whether or not Szuter and Bayham have actually demonstrated that such a change did occur, their analysis points out that potential shifts in site function through time must also be controlled in resource depression analyses. There is no reason to expect that either the kinds or the frequencies of resources represented at a site will remain the same if the role of that site in a settlement system changes. In this regard, resource depression analyses are just as dependent on establishing "monitoring perspective" as are RSA analyses. To the extent that this is the case, resource depression analyses begin to converge on landscape models, and become subject to the kinds of problems that we have outlined above with respect to those models.

CONCLUSIONS

We are very sympathetic to the application of models from foraging theory to hunter-gatherer prehistory. Indeed, we agree strongly with O'Connell (1995a) and Broughton and O'Connell (1999) that foraging theory provides the best, if not the only, means currently available to archaeologists for examining interactions be-

tween people and their environments within an evolutionary framework. The most rigorous archaeological applications of foraging theory have provided important insights into subsistence change and into the relationships between human impacts on the environment and human responses to those impacts (e.g., Broughton 1995).

However, we also find that current approaches to applying these models to the past are problematic in a variety of ways. In fact, the suite of problems that we have reviewed may explain why some of those who are most vocal about applying foraging theory models to the archaeological record have yet to do so in any detail. Our admiration for those who have made the attempt rises accordingly. We look forward to more rigorous archaeological applications of foraging theory models and to models that can deal more successfully with interrelationships between landscape change and landscape use.

While analyses of the relationship between relative skeletal abundance and body part utility may have run their course in Great Basin archaeology, except perhaps as part of a broad set of tools used to explore hypotheses about past subsistence, we are not quite so pessimistic about landscape models. It is possible that ways will be found to interrelate the archaeological record, information on changing distributions and abundances of potential subsistence resources, and estimates of resource return rates in a compelling fashion. As they stand now, however, these models are heavily dependent on projecting modern environmental conditions into the past and thus seem unable to account for (and in some cases even identify) landscape-level change through time. As the pioneering attempts to build and apply such models clearly show, the results are weak at the most critical times—when things were different.

We are far more optimistic about the archaeological application of what we have termed resource depression models. Indeed, we can see many obvious applications of these models to important Great Basin issues. For instance, we wonder if the significant decrease in the abundance of mountain sheep between the White Mountains previllage and village faunal assemblages, and concomitant increase in the utilization of marmots (*Marmota flaviventris*; see Grayson 1991b), might reflect the results of long-term human predation on mountain sheep. We wonder whether such a decrease might not also account for the fact that apparent hunting facilities in Great Basin upland settings tend to be associated with earlier Gatecliff and Elko points and not with later Cottonwood and Desert series points (Bettinger 1991b; Bettinger and Baumhoff 1982; Canaday 1997; Pendleton and Thomas 1983; Thomas 1982b).

Nonetheless, insofar as they rely on knowledge of matters that we cannot observe—for instance, the ways in which various resources were collected and processed—these studies are also problematic. In addition,

it seems essential to these approaches that they incorporate the potential effects of changes in nonvertebrate aspects of the diet on the use of small mammals. It also seems essential to develop means of examining whether or not lagomorphs, for instance, were taken in mass or individually.

We have taken as our task here the critical examination of archaeological applications of foraging theory models in the Great Basin. In doing this, we do not mean to imply that the approaches we have discussed constitute all that might be done archaeologically with the powerful framework provided by evolutionary ecology. There are clearly many additional kinds of problems that can be addressed both with existing foraging theory models and with models that might be developed in the future. To take but one example, some archaeologists working outside of the Great Basin have recently begun to address the evolution of agriculture, long recognized as an extreme form of resource intensification (e.g., Boserup 1965; Wills 1988), through the explicit use of diet breadth and risk minimization models (e.g., Gremillion 1996; Winterhalder and Goland 1997). Developments along such lines, however, must proceed hand-in-hand with attempts to address the kinds of issues we have outlined here.

We would not deny that foraging theory might be the wave of the future in Great Basin archaeology, nor would we argue that it should not be so. It is also surely expecting too much to think that after only a decade or so of applications by a very small group of practitioners, we would be much further along than we are now. Substantial problems remain to be solved, though, before the advantages that evolutionary ecology can provide to archaeology relative to Stewardian cultural ecology (O'Connell et al. 1982) can be more fully realized.

NOTES

1. In this regard, we note that it was Steward himself who suggested to Gordon Willey that Willey undertake a "settlement pattern survey"; the result was the Viru Valley Project (Willey 1974:149–157).

2. The Szuter and Bayham (1989) analysis of changing foraging efficiency through time is muddied by the fact that the Ventana Cave artiodactyl index is highly correlated with sample size ($r = 0.88$, $p < 0.01$). Further exploration of the causes of the behavior of this index would thus be appropriate.

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