10. Prehistoric Anthropogenic Impacts to Local and Regional Faunas Are Not Ubiquitous

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Abstract: Many zooarchaeologists have presented evidence indicating that prehistoric foragers had significant impacts on faunas. The evidence has prompted the suggestion that anthropogenic impacts are ubiquitous. In many times and places humans indeed influenced local faunal communities. Research should not, however, be initiated with the preconceived notion that evidence of that influence will be found in every collection of faunal remains studied. Some studies indicate minimal human influence and suggest that failure to find evidence of human influence can be the result of methodological artifacts, use of data of insufficient (spatiotemporal) resolution, and use of data representing cases in which humans had minimal influence on faunas. Reasons for lack of influence include archaeological monitoring perspective and harvest of individuals whose abundance does not influence prey availability.

Ecologically inclined archaeologists, or what we might today label ethnobiologists, ethnobotanists, zooarchaeologists, and the like, have long been interested in the interactions between human and nonhuman organisms. Beginning in the 1970s, some of these researchers adopted versions of optimal foraging theory (e.g., Krebs and Davies 1978; Pyke et al. 1977) to assist with understanding and explaining the interactions (Bayham 1979; Earle and Christenson 1980; Winterhalder and Smith 1981). Foraging theory ultimately came to provide a source of models that explicitly relate variables manifest by predators and their prey and the interactions of the two (Broughton and O’Connell 1999). The numerous studies that have to date been performed under the guidance of foraging theory...
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Detecting Anthropogenic Influence on Prehistoric Faunas

To identify reasons as to why evidence of anthropogenic influence might not be found, the zooarchaeological evidence usually called upon as indicative of anthropogenic influence on prehistoric faunas must be identified. Given

(Stephens and Krebs 1986) are now often grouped under the banner of human behavioral ecology (Winterhalder and Smith 2000), and while many such studies have provided significant insights to human behaviors in ethnographic settings, archaeology has its share of these studies (Bird and O’Connell 2006).

By the last decade of the twentieth century, the plethora of research on predator-prey interactions performed by zooarchaeologists under the guidance of human behavioral ecology prompted some researchers to synthesize evidence from large geographic areas (e.g., Cannon 2000; Grayson 2001; Hildebrandt and Jones 2002; Janteski 1997; Kay 1994; Martin and Szuter 1999a, 1999b; Stiner et al. 2000; Stiner et al. 1999; Szuter and Bayham 1989). The syntheses presented by these researchers seem to imply, if not by word then by spatiotemporal breadth of their coverage, that anthropogenic impacts are detectable nearly everywhere and everywhen in the zooarchaeological record. While not denying that humans in many times and places likely influenced local and regional faunas to a greater or lesser degree, I suggest that we should not go into our research with the preconceived notion that we will find evidence of that influence in each and every collection of faunal remains we study. We may find what I think of as false signals for any of several reasons. What those reasons are is one issue I explore here.

To date, few studies have failed to find evidence of anthropogenic influences within the zooarchaeological record but the number of such studies is slowly growing (Butler and Campbell 2004; Byers and Broughton 2004; Lyman 1995, 2003a, 2004a; Lyman and Wolverton 2002; Wolverton 2005). Interestingly, whereas some early reports that anthropogenic influence was not evident in a particular instance met with insistence that prehistoric people were indeed responsible for observed changes in taxonomic relative abundances over time (e.g., Hildebrandt and Jones 2002; Jones and Hildebrandt 1995; Laliberte and Ripple 2003; Martin and Szuter 2002), such disagreement has not yet surfaced regarding later findings of no significant anthropogenic influence. Perhaps this is because of the growing weight of the evidence, or because of the recency of the reports of no significant influence, or because analyses are more sophisticated theoretically and are backed by large data sets. Whatever the case, given the facts that, first, there are studies in which no evidence of anthropogenic influence on faunas has been found and, second, there is some (perhaps small) degree of doubt about those cases where evidence of anthropogenic influence is apparently lacking, it seems to me that the issue of why we might not always find evidence of anthropogenic influence on prehistoric faunas should be explored. Figuring this out may in turn reveal how false signals of anthropogenic influence (or false signals of the lack of such influence) may be created. These are the central issues I address here.

Detecting Anthropogenic Influence on Prehistoric Faunas

To identify reasons as to why evidence of anthropogenic influence might not be found, the zooarchaeological evidence usually called upon as indicative of anthropogenic influence on prehistoric faunas must be identified. Given
that the typical signature evidence was recognized as such in light of foraging theory and behavioral ecology. I start there. The nuances of the method and the evidence should by now be well known given the extensive pertinent literature, so I only outline the basics. The prey-choice model holds that human foragers will preferentially exploit the largest prey first because these taxa are the most valuable, all else being equal (Ugan 2005). If valuable (large) prey decrease in availability and thus the frequency at which they are encountered decreases, then foragers will turn to progressively more kinds of less valuable, generally smaller, prey—each individual considered less valuable than a single larger individual—to maintain a constant level of nutrition. Thus, the ratio of large prey to small plus large prey will fluctuate over time, decreasing (the proportion becoming progressively smaller than 1.0) as large prey become less available relative to small prey and increasing (becoming progressively closer to 1.0) as large prey become more available relative to small prey. Typically, animal prey abundances measured as the number of identified specimens (NISP) are rendered as an index value between 0 and 1.0 that expresses the proportion of large prey relative to small plus large prey. Plotting each index value against the temporal midpoint of the time span during which the assemblage of faunal remains on which the value is based was deposited—whether stratigraphically, radiometrically, or culturally determined—in a bivariate scatterplot visually reveals what are interpreted to be changes in prey return rates that in turn interpreted to reflect changes in prey availability or abundance. Interpretation of the scatterplot is sometimes aided by calulating a simple best-fit regression line through the point scatter in order to highlight temporal trends.

The model and analytical protocol just described identify an empirical manifestation of a particular signature criterion of anthropogenic influences on (paleo)faunas (Grayson 2001). As a result of their selective exploitation of prey taxa that provided either or both low costs and high returns, humans with primitive technologies caused changes in faunal taxonomic richness and evenness independent of changes in climate and technology. Humans therefore had to alter what they were exploiting as a response to a change in the availability of animal prey that they themselves had caused. The signature is that over time large prey decrease in relative abundance in the zooarchaeological record and small prey increase in relative abundance as humans adapt to an anthropogenic influence on prey availability.

The voice of caution requires that if one attributes increasing relative abundances of small, low-value prey taxa to human exploitation having depressed the availability of large, high-value prey taxa, alternative causes of depression of high-value prey must be disconfirmed. Alternative causes can include changes in technology (e.g., shift from atlatl and dart to bow and arrow), changes in how technology was used (e.g., shift from individual hunters to communal hunts), and environmentally or climatically driven changes in taxonomic abundances (Grayson and Cannon 1999). Disconfirmation of these alternate causes may not, however, be as easy as originally thought. Recent ethnoarchaeological research among foragers indicates that shifts in technology may be driven by seasonal shifts in prey variation in areas in relative to anthropogenic signatures where the sign that the check will demand r

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shifts in prey accessibility and variation in the age of foragers may also create variation in accumulated prey (Lupo and Schmitt 2005). Both can result in changes in relative abundances of high- and low-value prey, creating false signatures of anthropogenically influenced faunas. Similarly, recent research seeking biological signatures of climatic changes and means to distinguish these from instances where the signature has been forged by anthropogenic causes highlights the fact that the challenge of disconfirming climatic/environmental change as the cause will demand multiple lines of evidence (Parmesan 2006).

It has also been suggested that the kinds of units used in analysis may obscure a signal of resource depression. For example, faunal abundances are usually tallied by Linnaean taxonomic units, but these units may mask various kinds of resource depression such as if relatively slow-moving prey (e.g., turtles, tortoises, shellfish) are depleted and exploitation of fast-moving prey (e.g., hares and flying birds) is intensified (Stiner 2001; Stiner et al. 2000; Stiner et al. 1999). In addition, foraging models typically include temporal units such as foraging events with durations of several minutes, hours, days, or weeks. These brief temporal units are invisible in the archaeological record where a collection of prey remains typically represents an unknown number of foraging events (excluding mass-kill sites) and a duration (of formation) of multiple years (Grayson and Delpech 1998; Lyman 2003b). Relatively long-duration temporal units typically represented by archaeofaunal assemblages may mask signatures of anthropogenic influences, or they may create false signatures of those influences by, say, corresponding with undetected climatic phases, technological flux, or predator behavioral change.

Finally, spatial units in the ecologically based models may be treated as if they represent a predator's catchment area; archaeological methods for detection of shifts in catchment areas are slowly being developed but thus far are not consistently applied (Cannon 2003; Lyman 2003a; Munro 2005; Nagaoka 2005). Analytical spatial units need not be assumed to represent a catchment area. Given that environmental variability has a geographic (habitats vary across space) as well as a temporal dimension, inclusion in an analysis of faunal assemblages from multiple sites located across environmentally heterogeneous areas may mask or create false signatures of anthropogenic influences (e.g., Broughton 1994a, 1994b; Cannon 2000; Janetaki 1997). Importantly, we now know that a metapopulation (total population) of a migratory taxon might be depressed by anthropogenic predation in one area but not another, causing what appears to be local depression in the latter area (Bovy 2007).

The immediately preceding several paragraphs identify some areas where difficulties may reside when seeking evidence of anthropogenically influenced faunas. They also reveal reasons why we may or may not find what appears to be evidence of anthropogenic influence on a prehistoric faunal community. In the remainder of this discussion I present examples of various of these kinds of problems. This exercise reveals additional reasons that false signals might appear and underscores analytical and interpretive difficulties with detecting anthropogenically influenced faunas.
Faunal Change That Is Not(?) Anthropogenically Caused

As noted above, changes in technology, changes in how technology was used, and changes in environment or climate may cause changes in taxonomic abundances that fit the prediction of behavioral ecology that high-value, large prey will become less available and decrease in relative abundance while low-value, small prey become more abundant in the zooarchaeological record (Grayson and Cannon 1999). A growing number of studies find a correlation between environmental change and shifts in relative abundances of taxa. In some cases, environmental change caused high-value prey to decrease in abundance (e.g., Wolverton 2005); in other cases, environmental change caused high-value prey to increase in availability (e.g., Byers and Broughton 2004; Byers et al. 2005). I suspect that an instance of the latter will more readily catch the analyst’s attention than an instance of the former and result in additional detailed analyses aimed at determination of why the trend is the way that it is. I suspect this because an increase in high-value prey is the opposite of what behavioral ecology models suggest should be the result of human predation. An instance when environmental change causes high-value prey to become less available over time will catch the analyst’s attention but may not result in additional (confirmatory) analyses because its signature is precisely what many models suggest should result from human predation. Whether or not my suspicion is correct, it is clear that detailed paleoenvironmental records are required to evaluate whether changes in relative abundances of animal taxa are the result of anthropogenic factors or climatic factors. In some cases, as I note below, we may not be able to determine which set of factors is responsible.

Change in technology or how a particular technology is used has, as yet, seldom been documented. Remarkably, one of the seminal studies to explicitly utilize foraging theory models provided a detailed comparison of faunal change and technological change. In his (unfortunately unpublished and thus not widely available) doctoral dissertation, Frank Bayham (1982) studied covariation in the frequency of remains of high-value (artiodactyls) and low-value (leporid) prey and the frequency of what he categorized as hunting tools (projectile points, flake knives, and bifaces) and gathering tools (metates, manos, and pestles). When these frequencies were plotted against the 4,500-year-duration, seven-period stratigraphic sequence at Ventana Cave, Arizona, gathering tools were most abundant early and hunting tools were most abundant late in time. Not only that, Bayham observed that the relative abundance of artiodactyl remains increased more or less in correspondence with the increase in relative abundance of hunting tools.

To summarize Bayham’s (1982) insights in one graph, I used his “artiodactyl index” values, which are the proportion of artiodactyl remains in the combined sample of artiodactyl and leporid remains per stratum (Szuter and Bayham 1989). I also calculated a “hunting-tool index” as the proportion of hunting tools (as distinguished by Bayham) in the combined sample of hunting and gathering tools per stratum (Table 10-1). The two sets of index values are plotted against their stratigraphic provenience in Figure 10-1. The two sets of values are correlated
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Table 10-1. Ventana Cave Frequency Data for Leporid and Artiodactyl Faunal Remains and for Gathering Tools and Hunting Tools

<table>
<thead>
<tr>
<th>Stratum</th>
<th>Leporid NISP</th>
<th>Artiodactyl NISP</th>
<th>Artiodactyl Index</th>
<th>Gathering Tools</th>
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<th>Hunting Tool Index</th>
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<tbody>
<tr>
<td>1</td>
<td>179</td>
<td>321</td>
<td>.642</td>
<td>110</td>
<td>306</td>
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<td>2</td>
<td>259</td>
<td>371</td>
<td>.589</td>
<td>171</td>
<td>296</td>
<td>.634</td>
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<tr>
<td>3</td>
<td>125</td>
<td>91</td>
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<td>4</td>
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<td>5</td>
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<td>62</td>
<td>.300</td>
<td>607</td>
<td>138</td>
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<tr>
<td>6</td>
<td>177</td>
<td>51</td>
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<td>266</td>
<td>139</td>
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<tr>
<td>7</td>
<td>118</td>
<td>21</td>
<td>.151</td>
<td>58</td>
<td>45</td>
<td>.437</td>
</tr>
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Note: Data from Bayham (1982); NISP is number of identified specimens.

**Index Values**

![Graph](image)

**Figure 10-1.** Artiodactyl index (proportion of artiodactyl remains among artiodactyl plus leporid remains) and hunting-tool index (proportion of hunting tools among hunting plus gathering tools) across seven strata at Ventana Cave, Arizona (data from Bayham 1982). The two sets of index values are correlated (coefficient of determination, $r^2 = .57$, $p < .05$).
Atypical Anthropogenic Changes in Faunas

Although many studies of anthropogenic influences on paleofaunas concern human foragers, it is worth mentioning the influences of food producers—horticulturalists and agriculturalists—on faunas because some such influences do not present the typical signature of depression of high-value prey. These are archetypical examples of the demand for complex, multivariate explanations because they do not easily fit the traditional model.

On the one hand, the “garden-hunting hypothesis” was proposed by Olga Linares (1976) more than 30 years ago. This hypothesis holds that anthropogenic floras created by gardening and horticulture result in artificially increased animal biomass, especially animal taxa of small body size. The increased availability of small animals is thought to be taken advantage of by the farmers and is thought to show up in the zooarchaeological record as increases in the abundance of remainss of small animals (Stahl [2000] presents a related archaeological example). John Speth and Susan Scott (1989) suggested, on the other hand, that increasing dependence on cultivated plant foods could lead to increased reliance on large animals. An increase in the abundance of remains of large animals in the archaeological record of farml the hunters for economically known as any

Precisely faunal record debrandt anc the debate c change in fa or a human debate will n visible differ guished faur had accumu the taxa in e to explain th point raised the exact op demand con whether it v place to look nology, in ki demands nc in technolog taxa (e.g. Li
Iso Szuter and abundances of; specifically Ventana Cave took place. As was used less at various sites in the Thun, Ventana perspective it because the people altered anthropogenically intensive hunting but ion to remain t that the faune. Instead of through the stratigraphic 1982) and (1989). Major anthropogenic ed. These explanations provided by Olga anthropogeniced animal availability of s thought to of remains ample. John singing depending on age animals. Archaeological record of farmers could result from greater selectivity for large game on the part of the hunters for any of several reasons such as increased status from undertaking an economically risky endeavor or the presence of buffer zones (Hickerson 1965; also known as animal source areas; see Pulliam 1988) between sedentary farmers.

Precisely these two possible causes have appeared in a recent debate about a faunal record created by prehistoric foragers (Broughton and Bayham 2003; Hildebrandt and McGuire 2002, 2003). Using the words of some of the participants, the debate centers around the question, Was the (apparently) anthropogenic change in faunal taxonomic abundances driven by a human desire for calories or a human desire for prestige (McGuire and Hildebrandt 2005)? Resolving the debate will no doubt require innovative analyses meant to detect archaeologically visible differences between the two possibilities. Stahl (2000), for example, distinguished faunal remains accumulated and deposited by humans from remains that had accumulated naturally, and then considered the ecological predilections of the taxa in each subsample (culturally accumulated, and naturally accumulated) to explain the anthropogenic nature of the fauna he studied. This highlights the point raised earlier that identifying the cause of a faunal abundance trend that is the exact opposite of the standard signature of anthropogenic influence will likely demand complex multivariate analyses. For example, it has been suggested that whether it was a desire for calories or a desire for prestige may be the incorrect place to look; the pertinent faunal trends may have resulted from changes in technology, in kind or how it was used (Hockett 2005). Confirmation of this possibility demands not only detailed study of tools but also determination of whether shifts in technology were a cause or a consequence of shifts in the availability of prey taxa (e.g., Lupo and Schmitt 2005).

Instances of No Apparent Anthropogenic Influence

I turn now to instances in which the faunal record suggests there was no anthropogenic influence, or at least there was no evidence of such influence. The latter implies that the magnitude of influence may not impact a fauna because of low intensity or that the zooarchaeological signal of such influence is so weak as to be imperceptible. I am aware of two kinds of studies in which no evidence of anthropogenic influence was found. In one, the absence of a trend is readily explained by prey behavior; in the other, there is no obvious explanation for the absence of a trend in taxonomic abundances, and that is critical.

In an early study of pinniped remains from the late Holocene of the Oregon coast, I argued that depression of local populations of several taxa was not apparent because indices of abundances of preferred prey did not uniformly decrease as the foraging model suggested they should (Lyman 1995). Others thought those same data suggested that the metapopulation (the total population of a taxon) had indeed been depressed (read depleted) (Jones and Hildebrandt 1995). I subsequently found that the abundance of a local population of Steller sea lions (Eumetopias jubatus) did not seem to be depressed over the 700-year duration of site formation largely because about 90 percent of the remains were from adult
males—the demographic category that could be exploited without major influence on the local population given reproductive behaviors of this taxon (Lyman 2003a). In this case, the reason that no evidence of anthropogenic depression was found involved variation in the escape behaviors across age-sex classes of the population. Adult females and young of both sexes flee into the water at the first sign of danger on land while adult breeding-age males defend terrestrial breeding territories against all comers. Culling mostly adult males, if not excessive, would not influence the size of the population. And, contrary to some who say this represents prehistoric conservation (Alvard 1998), if it in fact does not because it did not involve the intentional exploitation of a certain fraction of the population such as is usually meant by the term conservation (Smith and Wishnie 2000). Instead, the lack of anthropogenic influence was an epiphenomenon (Hunn 1982) of which age-sex class of prey animals was exploited, which itself was a function of prey escape behavior and how available technology was used.

In another study, Butler and Campbell (2004) reviewed zooarchaeological data for the southern Northwest Coast and for the northern Columbia Plateau, environmentally and archaeologically distinct areas in the Pacific Northwest of North America. In both areas, they found no change in relative abundances of high-value and low-value fish through time, and they found no change in abundances of high-value mammals (artiodactyls) relative to abundances of low-value mammals. They suggest that the latter may be the result of environmental change in the Columbia Plateau, but that explanation does not account for the Northwest Coast data. They also suggest that the reproductive strategy and life cycle of salmon may render these fish somewhat immune to exploitation-related depression. They hypothesize as well that human populations in both areas may never have been sufficiently large to significantly influence fish and mammal populations. Finally, Butler and Campbell (2004:391) suggest their “scale of analysis” may have caused them to not find evidence of resource depression. In my view, the struggle and ultimate failure they have in finding an explanation for the lack of evidence of resource depression highlights two things. First, perhaps we too readily accept evidence of depression in the form of decreased abundance of high-value prey. And second, we tend to work hard at explaining the opposite of the signature trend in order to maintain some version of the resource-depression model. When there is no trend in taxonomic abundances, we may be left with a conundrum.

False Signals of Anthropogenic Influence

The case of the pinnipeds on the Oregon coast suggests that spatial grain may influence whether or not evidence of anthropogenic influence is detected. A coarse spatial grain is one in which a metapopulation is under study; a fine spatial grain is one in which an individual local population is under study. In the case of the Oregon pinnipeds, the coarse spatial grain suggested anthropogenic influence whereas a fine spatial grain suggested no anthropogenic influence in the location scrutinized. There is another example of how geographic resolution grain might influence results that warrants mention.
Kay (1994) lumped zooarchaeological data from seven western states and 10,000 years of prehistory. He found that the abundance of North American elk or wapiti (*Cervus elaphus*) remains for that spatiotemporal area relative to the abundance of wapiti in the modern Yellowstone ecosystem was exceptionally small (Figure 10-2). He then extrapolated from the Yellowstone ecosystem to the entire seven-state area, arguing that prehistoric hunters had depleted wapiti populations throughout the Holocene and the modern abundance of wapiti in Yellowstone is an artifact of twentieth-century wildlife and park management. When his data are divided into more spatiotemporally limited samples, that is, when the spatiotemporal units are changed to much finer grain units containing less time and less space, evidence for depression vanishes in some areas and in some time periods (Lyman 2004a).

Consider, for example, the data from Holocene archaeological sites in eastern Washington state. These data represent 86 spatiotemporally distinct assemblages of faunal remains. In an earlier analysis, I calculated the proportion of wapiti remains among summed wapiti and deer remains for each assemblage (Lyman 2004a). A bivariate plot of the proportion of wapiti remains in an assemblage against the age of the assemblage reveals no obvious trend (Figure 10-3). Further, using the then standard analytical techniques in my earlier analysis (Lyman 2004a), I found no correlation between the deer plus wapiti samples and the abundance of wapiti remains per assemblage ($r = .16, p = .14$). Sample size, it seemed at the time, was not driving any trend (or lack thereof) in wapiti abundances. I found no correlation between age and proportion of wapiti ($r = .006, p > .9$) and found that the slope of the best-fit regression line did not differ significantly from zero (Figure 10-3). This prompted me to conclude that there was no evidence for the depression (depletion) of wapiti over the 10,000 years represented. If, however, the wapiti and deer remains are summed by the 500-year period in which they occur, the proportion of wapiti remains for each 500-year period corresponds very nicely to the anthropogenic depression model (Figure 10-4). The simple best-fit regression line has a slope significantly different from zero and the two variables are correlated ($r = .489, p = .064$). The important lesson here is that a trend in taxonomic abundances may be an artifact of how those abundances are tallied. This observation demands that we consider the question, What is the correct way to tally those abundances, or is there a single correct way? At present, this question cannot be answered. And, sadly, there is yet another potential problem here.

To this point, I have used particular statistical analyses (simple correlation) and bivariate plots to search for evidence of anthropogenic depression; both are the traditional means of searching yet neither is particularly sensitive to detecting trends when percentages or proportions are used as measures of abundance. Calculating a correlation coefficient between relative abundances of taxa and sample size ($\sum$NISP) is not the best way to statistically search for trends (Cannon 2000, 2001). This is so because relative abundances do not register whether sample sizes are 5 or 5,000. Statistically, the difference between an absolute tally of 5 and an absolute tally of 5,000 is quite different from saying each comprises 5 percent of the total collection (100 and 100,000, respectively). The effect of small
Figure 10-2. Relative abundances of North American wapiti and other artiodactyls in the modern Yellowstone ecosystem compared to the relative abundances of wapiti remains and remains of other artiodactyls in archaeological sites in seven western states (data from Kay 1994).

Figure 10-3. Proportion of wapiti remains among summed wapiti and deer remains in 86 assemblages from eastern Washington state (data from Lyman 2004a). The slope of the simple best-fit regression line is not significantly different from zero, and there is no correlation between age and proportion of wapiti.
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Figure 10-4. Proportion of wapiti remains among summed wapiti and deer remains by 500-year period for 86 assemblages from eastern Washington state (data from Lyman 2004a). The slope of the simple best-fit regression line is significantly different from zero and the age and proportion of wapiti variables are correlated ($r = .489$, $p = .064$).

samples on correlations may simply be due to sampling error rather than any accurate reflection of abundance. Rare phenomena are particularly difficult to inventory; they will be absent from collections unless the samples are large. If the abundances of several rare taxa are not quite equal in the target population, but samples are small, the true relative abundances of those rare taxa likely will not be accurately reflected by small samples (Grayson 1984). Some taxa will be relatively more abundant, some less abundant, in the sample simply because of sampling error. Thus a correlation between the relative abundance of a taxon and $\Sigma$NISP across multiple assemblages may be driven by small samples because of sampling error.

Use of the regression approach to search for sample-size effects (as in Figures 10-3 and 10-4) may lead to commission of a Type I error (rejecting a true null hypothesis that there is no true trend in relative abundances when in fact there is no trend) or commission of a Type II error (accepting a false null hypothesis that there is no true trend in abundances when in fact there is a trend). In both cases, the null hypothesis is that no trend is present, but sampling error has produced samples that are not representative of the population. Traditionally, a significant correlation coefficient has been interpreted as indicating that sample size is the source of the correlation, and the absence of a significant correlation coefficient is
interpreted as indicating that sample size is not the source of the correlation. As Cannon (2001:185) observed, the first interpretation at best rests on an incomplete understanding of the relationship between relative abundances and sample size; the second interpretation presumes sample sizes are sufficiently large to warrant confidence but in fact they may be too small. In terms more pertinent to seeking evidence of anthropogenic depression, true trends in, say, the relative abundance of wapiti (in Figures 10-3 and 10-4), that is, trends that are not the result of sample size, may go undetected by correlation analysis because of conflation of those trends with sample-size effects.

Cannon (2000, 2001) suggests using Cochran’s test of linear trends, a form of chi-square analysis that tests for trends among multiple rank-ordered samples (Zar 1996:562–565). As Cannon (2000:332) notes, Cochran’s test is constructed such that “significant trends will not be found when samples are so small that random error cannot be ruled out at a specified confidence level as the cause of differences in relative abundance between samples.” Cochran’s test seeks trends in relative abundance in such a way as to more directly take absolute sample size into account than correlation-based analyses. One first calculates a standard chi-square statistic, and then determines how much of that statistic is the result of a linear trend; if the latter is sufficiently (statistically significant) large, then one concludes that there is indeed a linear trend in the data independent of any effects of sample size.

Let us reconsider the wapiti data in Figure 10-3. The overall chi-square statistic is large and significant ($\chi^2 = 4696.6, p < .0001$), suggesting there is a significant association between the frequency of deer remains and the frequency of wapiti remains. The chi-square statistic for a linear trend is also significant ($\chi^2 = 112.96, p < .0001$), suggesting there is a significant temporal trend in the relative abundance of wapiti remains across the 86 assemblages regardless of the sizes of the assemblages. The simple best-fit regression line in Figure 10-3 is of no help in determining which direction the trend is; do wapiti increase over time or decrease in abundance relative to the abundance of deer? The chi-square trend statistic fails to indicate the direction of change. Figure 10-4 provides an indication that the trend is for wapiti remains to decrease in abundance.

Whatever trends in taxonomic abundances there may be in Figure 10-3 and in Figure 10-4 must, for the present, be treated as methodological artifacts. They could reflect how remains were tallied, how assemblages were spatiotemporally lumped, or how they were graphically or statistically detected (see Lyman 2008 for additional discussion). Another example will demonstrate that such methodological artifacts are not unusual. Chatters (2004:73) found an inverse correlation between the number of sites in eastern Washington state “in which bison [Bison bison] number more than 10 percent of the mammalian archaeofauna and date between 1800 and 2400 BP” and a curve meant to estimate human population size based on the frequency of radiocarbon dates (Figure 10-5). Although this may well represent a case of anthropogenic depression and subsequent release of bison populations from predation, I perceive two potential methodological artifacts. First, there are only eight archaeological sites in the sample, so the appearance of a correlation between human population size and bison availability...
correlation. As an incomplete and sample size; large to warrant all to seeking to abundance result of sample correlation of those trends, a form ordered samples is constructed so small that as the cause of seeks trends absolute sample sites a standard tic is the result, t) large, then xendent of any chi-square sta-
there is a signi-ficant \( \chi^2 = 
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availability may be a function of the few sites comprising the sample. To counter this, Chatters could argue that the percentage of all ungulate remains that represent bison in all sites in eastern Washington dating between 2001 and 2500 B.P. is higher than during any other 500-year period, except during the terminal Pleistocene when human populations were undoubtedly small (Lyman 2004b).

The second possible methodological artifact in Chatters's analysis is his use of a curve, based on the frequency of radiocarbon dates, “adjusted for decay, as a proxy for regional [human] population size” (Chatters 2004:71; see also Chatters 1995). A smoothed version of the curve is shown in Figure 10-5. The curve may be a reasonable estimate of human population size, but it has not been tested with any independent evidence of population size. For example, the radiocarbon record for the area may be biased in one or more ways (Lyman 2000a, 2000b). Even if the sample of radiocarbon dates is representative of the radiocarbon record, neither the frequency of radiocarbon dates per 500-year increment nor the frequency of dated archaeological components correlates (positively or negatively) with the proportion of ungulate remains that represent bison (Lyman 2004b) or with the NISP of bison per 500-year increment (Figure 10-6).
frequency of radiocarbon dates is in fact directly correlated with human population size, the lack of correlation over the entire Holocene suggests that there is no regular relationship between the size of the human population and the local availability of bison.

As indicated in Figure 10-6, there does seem to be a strong inverse relationship between bison NISP and frequencies of dated sites and bison NISP and frequencies of radiocarbon dates between 3000 and 1500 B.P., more or less when Chatters suggests the two are inversely related. The remainder of the graph, however, seems to imply that this inverse relationship may be more apparent than real. In other time spans bison NISP and frequencies of dated sites and of radiocarbon dates both decrease (after 1000 B.P.) or both increase (1500 to 1000 B.P.; 6000 to 5000 B.P.; 9500 to 8500 B.P.). If the inverse relationship of the two variables shown in Figure 10-5 is real, it is virtually unique among the available data. I worry, therefore, that the relationship is a simple function of which portion of the entire time span shown in Figure 10-6 appears in Figure 10-5.
I am not arguing here that Chatters (or anyone else) has presented incorrect conclusions; what I am arguing is that we simply cannot at the present time determine whether he (or anyone else) is correct or not. The source of this difficulty is the problem of not knowing when we have analytically produced a trend (created a methodological artifact) rather than simply detected a trend of interpretive interest. And this is the heart of the point I wish to make about identifying prehistoric anthropogenically influenced faunas.

Discussion and Conclusion

There is no doubt that models based on foraging theory and used by zooarchaeologists have provided a wealth of unique insights to predator-prey interactions. One of those insights has been that anthropogenically influenced faunas were rather commonplace on prehistoric landscapes. A cynic such as myself might point out that we should have suspected as much given that humans are organisms that utilize plants and animals and thus will, when present on the landscape, have ecological influences on other organisms (e.g., Stahl 2008). Foraging theory-based models have indicated some of the empirical manifestations of those influences. But increases in our knowledge about foraging practices of people and about aspects of the zooarchaeological record both have in the past few years underscored that detecting such things as human-depressed faunas is hardly straightforward. Detection has, if anything, become not only more commonplace but also much more challenging analytically to validate.

The point I wish to make here can be reduced to two variables that have been discussed in the literature on modern anthropogenically induced climate change (Parmesan 2006). Those variables are detection and attribution. On the one hand, detection concerns discerning evidence of a true trend (one that is not a methodological artifact) in the variables of interest; in the cases I have discussed, the variables are relative abundances of animal taxa exploited by human foragers. I suggest we need more actualistic fidelity studies to determine which detection methods are most valid in particular settings. A fidelity study is one that determines the degree to which a collection of faunal remains accurately reflects the environment from which the collection derives (e.g., Behrensmeyer et al. 2000; Kidwell 2001). Ethnoarchaeologists and neotaphonomists could easily provide the requisite information here (e.g., Lyman and Lyman 2003).

Attribution, on the other hand, concerns assigning the correct cause to the detected trend; in the cases I have discussed, the cause should be anthropogenic predation. To build robust arguments of attribution, I suggest multiple lines of evidence must be brought to bear in any case in which anthropogenic influence is suspected. Because it may create a tautology to argue for anthropogenic influences, other causes of faunal change should be eliminated with independent evidence. Thus, floral or geological evidence for a lack of change in climate could eliminate that cause; analysis of technology could eliminate that potential cause; and so on. I have presented examples of what seem to be detection of false trends, ones that could be methodological artifacts. I have also noted that even when
trends in faunal abundances seem to reflect what is happening on the landscape, it is another matter to build a strong case that the trend is related to human predation—that is, to build a strongly warranted attribution.

Finally, the cases I have reviewed tend to confirm my suspicion that when a trend in taxonomic abundances is found that is the opposite of that predicted by models derived from foraging theory—when abundances of high-value prey increase over time—analysis increases in complexity until the faunal record can be aligned with a revised model. This in itself is not bad. I worry, however, that it might indicate that we are a bit too complacent when shifts in taxonomic abundances match the modeled signature of decreased abundances of high-value prey. We do not rigorously explore other possible causes for the changes in taxonomic abundances. The ubiquity of anthropogenically influenced faunas in the zooarchaeological record may thus be more apparent than real. We need to intensify scrutiny of that record for such evidence, but we also need to rigorously evaluate any indication of anthropogenically influenced faunas. As those faunas come to play a more prominent role in modern conservation biology (e.g., Lyman 2006; Stahl 2008), we will want to make sure that our identifications of them are valid, else we risk making management decisions on the basis of inaccurate inferences.

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