

## HISTORICAL CONTINGENCIES, ISSUES OF SCALE, AND FLIGHTLESS HYPOTHESES: A RESPONSE TO HILDEBRANDT ET AL.

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*Hildebrandt et al. offer this rather vitriolic challenge to our conclusions on the Diablo Canyon fauna in order to recast the data in favor of their view that major diachronic trends in western North American prehistory are the product of an increase in men's prestige hunting over time. Here we respond, first by discussing our view of the relationship between historical contingencies and behavioral ecological models, second by showing that the patterns they find in a regional faunal dataset result only from inappropriate aggregation of bone counts, third by questioning the potential prestige value of highly vulnerable species, and finally by making the case that standard behavioral ecological models, punctuated by historical contingencies, provide more parsimonious, albeit less fanciful, explanations for patterning in the western North American faunal record. We conclude by suggesting that when practitioners attempt to explain away empirical variability in order to support a favored hypothesis, it might be time to acknowledge that the hypothesis has failed to take flight.*

*Hildebrandt y colegas ofrecen este desafío bastante mordaz a nuestras conclusiones en la fauna de Cañón de Diablo para refundir los datos a favor de su vista que tendencias diacrónicas mayores en la prehistoria norteamericana occidental son el producto de un aumento en el prestigio de hombres que caza con el tiempo. Aquí respondemos, primero discutiendo nuestra vista de la relación entre contingencias y modelos históricas ecológicas conductistas, segundo mostrando que las pautas ellos encuentran en un resultado regional de dataset de faunal sólo de agregado inadecuada de condes de hueso, el tercero preguntando el valor potencial de prestigio de la especie sumamente vulnerable, y por último haciendo el caso que modelos ecológicos, conductistas y estándar, puntuado por contingencias históricas, proporciona más parco, aunque menos imaginario, Las explicaciones para modelar en el registro norteamericano occidental de faunal. Concluimos sugiriendo cuándo facultativos procuran justificar la variabilidad empírica para apoyar una hipótesis favorecida, quizás sea tiempo de reconocer que la hipótesis ha fallado de huir.*

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Given our ongoing debate with Hildebrandt, McGuire and colleagues over the role of costly signaling and prestige-based hunting in western North American prehistory (see Codding and Jones 2007a; Hildebrandt and McGuire 2002; McGuire and Hildebrandt 2005; McGuire et al. 2007; see also Broughton and Bayham 2003; Broughton et al. 2008; Byers and Broughton 2004; Byers et al. 2005; Hildebrandt and McGuire 2003; Hockett 2005; Zeanah 2004), it is not entirely surprising that these authors have elected to challenge our recent findings from Diablo Canyon (CA-SLO-2). We undertook the analysis of the Diablo materials because the collection was unusually robust,

had never been fully reported, and seemed potentially relevant to several major archaeological issues including arguments over a coastal colonization route into the New World and diachronic patterns of resource overexploitation and economic intensification. We were also anxious to compare the Diablo results with those from the southern California islands since the islands have figured prominently in discussion of these issues in the past, yet because they lack significant terrestrial resources, they are less than ideal for evaluating certain aspects of these questions. The issue of prestige hunting and the relative importance of rabbits versus deer was a small portion of our study that we addressed

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only because so much had been made of it in recent literature, and because the Diablo findings seemed to be at odds with previously identified patterns. Because the Diablo dataset did not support their prestige hunting hypothesis, Hildebrandt et al. have offered this rather vitriolic challenge to our theoretical perspective, methods, and findings—despite the fact that most of the conclusions we developed from the Diablo collection were rather unremarkable and well supported empirically. Specifically, these authors (1) are concerned by the explanatory role we give to historical contingency, (2) attempt to overwhelm our findings with a broader regional data set, and (3) use our findings to push the prestige hunting explanation even further by asserting, among other things, that the extinct flightless duck and sea otters were exploited primarily as prestige items. Here we respond, first by explaining again our view of the relationship between historical contingencies and behavioral ecological models, second by evaluating their enlarged data set to show that the patterns they identify only result from inappropriate aggregation of data, and finally by making the case that standard behavioral ecological models, punctuated by historical contingencies, provide more parsimonious, albeit less fanciful, explanations for patterning in the western North American faunal record without incorporating prestige as a major causal variable.

### Historical Contingencies and Behavioral Ecology

In approaching the Diablo data we attempted to employ an overall theoretical framework derived from behavioral ecology, specifically testing predictions from the prey choice model to evaluate synchronic and diachronic patterns. While our overall approach was not formally hypothetico-deductive, we did develop a ranking of major prey taxa based on their potential caloric return rates before evaluating site findings. As our analysis focused on the Diablo Canyon collections, the study was envisioned as an application of behavioral ecology rather than an attempt to significantly advance the approach itself. Our ultimate objective was to develop the most parsimonious explanation for patterning in the Diablo record; in circumstances where the model predictions were not met, we deferred to historical contingencies to explain cer-

tain aspects of the empirical record. Our view was that predictive models of the type derived from behavioral ecology provide tremendous insights into the past, but they are also unlikely to provide credible explanations for all of the cultural and behavioral variability represented in the record (see also Bird and O'Connell 2006). It seems important to us to acknowledge places where predictive models fail as well as the places where they succeed. Hildebrandt et al. fully acknowledge that events such as large-scale migrations, droughts, and extraterrestrial impacts had major effects on the trajectories of western North American prehistory, yet they feel that our use of historical explanations was somehow inappropriate. Despite their assertions otherwise, nowhere did we say that behavioral ecology and historical contingencies are incompatible. The combination of behavioral ecology and history that they advocate is the approach we in fact employed. This is one of several cases where Hildebrandt et al. misrepresent our statements. The major difference in our theoretical approaches is that in cases where data failed to meet the proposed predictions of the models, we inferred that certain historical contingencies best explained the mismatch; however, in similar situations, Hildebrandt et al. prefer to substitute one currency (caloric returns) for another (prestige), and in some cases, they substitute the latter where no mismatch between data and the standard models are apparent. Ultimately, however, their own hypotheses regarding diachronic change in western North America relies on historical events including changes in human population densities at specific times, population migrations and droughts (see Hildebrandt and McGuire 2002; McGuire and Hildebrandt 2005).

In our evaluation of the Diablo fauna we relied on historical contingencies to aid explanation in three areas: watercraft use coupled with a tradition of coastal exploitation by the initial site inhabitants, varied subsistence change during the late Holocene related to the Medieval Climatic Anomaly, and an historically robust deer population in the Diablo area that contributed to the unusually high frequency of deer bones at this location. The value of our combined behavioral ecology/historical contingency approach seemed most apparent in helping to conceptualize watercraft use. Comparing the Diablo fauna to the prey rankings based

on potential caloric yields, the earliest Diablo components showed an unexpectedly high frequency of marine birds. Such a low-ranked resource would not seem in itself to provide sufficient incentive to develop a new technology (e.g., watercraft) to aid in its pursuit. Rather, the exploitation of this resource would only make sense for people who had already developed boats. Based on that insight, we further suggested that the Diablo site was part a linearly oriented tradition of people adapted to the shoreline who used watercraft, fished, and collected shellfish but who also exploited a wide range of terrestrial resources from shoreline residential bases. We feel that our characterization, informed by comparison with optimization predictions, offers some new insights into early coastal adaptations that help to explain the subsistence energetics associated with a possible coastal migration route into the New World.

#### Zooarchaeological Patterning and Issues of Scale

For Hildebrandt et al., the Diablo Canyon faunal remains present a significant problem because they show a high proportion of deer bones throughout the Holocene. Particularly troublesome for their hypothesis are high frequencies of deer in the early-middle Holocene components. They approach this challenge in two ways, first by attempting to undermine the patterning at Diablo Canyon, and second, by developing a larger regional dataset in order to support an ostensive increase in large prey hunting during the middle Holocene. Here we address both of these issues to show that their concerns with the Diablo fauna are unfounded and that their regional dataset is selectively biased, and when carefully evaluated, reveals patterning completely different from what they suggest.

#### *Faunal Patterning at Diablo Canyon*

Hildebrandt et al. attempt to explain away the dominance of deer in the trans-Holocene record from Diablo Canyon in order to argue that the prestige-hunting model they introduced several years ago (Hildebrandt and McGuire 2002; McGuire and Hildebrandt 2005; McGuire et al. 2007) remains viable. According to their views, early Holocene populations foraged with an emphasis on mean daily caloric efficiency, focusing on small reliable

game such as rabbits, rather than more unpredictable larger species such as deer. They believe that these early populations consciously eschewed deer and other large game until later in the Holocene when larger populations and greater aggregations of people provided male hunters with enhanced social opportunities for prestige-signaling. In an earlier comment on their hypothesis (Coddling and Jones 2007a), we discussed how their approach (1) only selectively examined the ethnographic record, (2) simplistically and inappropriately represented the archaeological signatures of prestige hunting (see also Lupo 2007) and (3) misunderstood costly signaling theory itself. Their treatment here is little improved, particularly with reference to the ethnographic record: recent examinations have shown that foraging activities can have both subsistence and prestige benefits (e.g., Bliege Bird and Bird 2008; Gurven and Hill 2009; Weissner 2002), making the argument of subsistence vs. prestige largely an unnecessary polemic. But regardless of these problems, Hildebrandt et al. take issue with our stance that prestige did not play a more central role than subsistence in explaining the patterning of the Diablo Canyon fauna. In an attempt to explain away the empirical patterning, they argue that the early components are problematic, that our treatment of deer and rabbit bone counts are inadequate, and that a shift to logistic hunting in the middle Holocene supports their hypothesis. We discuss each of these issues in turn.

First, to support their hypothesis, Hildebrandt et al. identify the two earliest components at Diablo Canyon as problematic in order to recast them in favor of the prestige hunting model, but neither of their concerns with these components are empirically founded. They assume that the low numbers of bones in the earliest component bias its value, but a simple analysis shows that there is in fact no effect of sample size (Total NISP, see Jones, Porcasi, Gaeta and Coddling 2008: Table 5) on the temporal patterns in the abundance of deer relative to rabbits (Spearman's  $\rho = .8, p = .2$ ). This shows that the pattern of interest (the proportion of deer relative to rabbits) is not a function of sample size. To undermine the second component (bracketed with eight radiocarbon dates between 7000 and 5000 cal B.P.), they reinvent the published regional cultural chronology, asserting that the California Millingstone Culture ended not ca. 5000–5500 cal B.P. as

depicted in the established regional sequence (Jones et al. 2007:136; see also Jones 1993, 2003), but rather at 6200 cal B.P. To accomplish this, they employ date midpoints from other sites in the region, but characterizing component chronologies with date midpoints can be a highly misleading exercise, particularly when a significant range of time is represented. This methodological issue aside, all of the midpoints they present for the Early Period (ostensibly representing the components younger than the Millingstone period; see Hildebrandt et al.: Table 1) are in fact younger than 5500 cal B.P. except for component II at SLO-2 which is older. This is fully consistent with the established regional culture history that shows a major cultural division at 5500 cal B.P. and with component II at CA-SLO-2 (representing Millingstone) pre-dating that transition point. No reinvention of the published regional sequence was necessary or warranted.

Second, Hildebrandt et al. take issue with our taxonomic summation noting inconsistencies between deer/artiodactyl and rabbit/leporid bone counts in two separate tables (see Jones, Porcasi, Gaeta and Codding 2008: Table 5 and Table 6). We thank them for their close reading in catching those errors, but when corrected, the trends and their statistical significance remain the same as what we reported (see Table 1). This is true even when the trends are calculated at the species level (i.e., *Odocoileus hemionus* vs. *Sylvilagus* spp.; see Table 1). Moreover, when the species level trends (the *Odocoileus* index) is plotted against the family level trends (the Artiodactyl index), the correlation is significant (Spearman's  $\rho = 1$ ,  $p < .0001$ ), indicating that the trends are robust across levels of identification. Again, each of these trends do not support the prestige hunting hypothesis, but rather, confirm our previous findings from the Diablo Canyon site.

Third, Hildebrandt et al. suggest that the diachronic increase in the proportion of artiodactyls is accompanied by an increase in logistic mobility (*sensu* Binford 1980) which indicates higher acquisition costs through time. However, they provide *no evidence* for such an increase and the robust Diablo Canyon collection (Jones, Porcasi, Gaeta and Codding 2008: Table 8) provides no evidence for an increase in off-site butchering prior to transport which would indicate increased logistical hunting. Moreover, the types and statistically demonstrated

diversity of artifacts at CA-SLO-2 are consistent across all time periods, indicating no change in site function (Jones, Porcasi, Gaeta and Codding 2008: Table 4). Here again, Hildebrandt and colleagues attempt to explain *away* (rather than explain) empirical trends that run counter to their hypothesis.

#### *Issues of Scale in Zooarchaeological Analysis*

In an earlier comment on their prestige-hunting hypothesis (Codding and Jones 2007a), we expressed our concern that Hildebrandt and McGuire had not supported their model with much in the way of empirical evidence. That their current commentary on the Diablo findings includes an attempt to rectify that shortcoming with a broader regional record is commendable, but it does not negate the Diablo conclusions, and the flaws inherent in the development and application of this regional data set are numerous. First, their regional data set is far from comprehensive. Many published faunal collection from important sites in the region are ignored (e.g., CA-SLO-9, Codding and Jones 2007b; CA-SLO-267, Jones and Ferneau 2002); some findings from CRM projects are listed while others are excluded (Lebow, McKim, Harro et al. 2005; Lebow, Harro, McKim et al. 2005; Lebow et al. 2006), including many late Holocene components dominated by rabbit bones (e.g., Basgall 2006; Carpenter et al. 2007; Garlinghouse and Farquhar 2005; Harro et al. 2000; Lebow et al. 2007). The latter are particularly troublesome because their omission allows Hildebrandt et al. to create the impression that deer dominate the late Holocene record.

Despite these not insignificant problems, the main issue with this data set concerns their analytical procedures. This problem is primarily one of scale, which has become a central topic in ecology (see Levin 1992; Schneider 2001; Turner et al. 1989). Many studies have shown that the scale at which phenomena are examined truly matters because patterns visible at one spatial or temporal scale may disappear or even become the opposite at another scale. Related to this is an ecological fallacy; i.e. the thinking that populations are homogeneous and thus the characteristics of individuals in a population can be averaged together in order to understand the characteristics of the population as a whole. As averaging in this way changes the scale of the unit of analysis, however, it can take

Table 1. Artiodactyl and Leporid Bone Counts (NISP) and Index Values from Diablo Canyon (CA-SLO-2).

Taxon	Common Name	I	II	III	IV
		Late Period	Middle Period	Late Milling-stone	Early Milling-stone
<i>Odocoileus hemionus</i>	Black-tailed Deer	323	673	187	18
<i>Cervus elaphus</i>	Elk	1	1	1	0
Artiodactyl*	Even-toed ungulates	230	415	112	6
<i>Sylvilagus</i> spp.	Cottontail rabbit	70	221	66	8
<i>Lepus californicus</i>	Jack Rabbit	0	2	1	0
Leporid *	Rabbits and Hares	1	1	0	0
	Artiodactyl (all)*	554 (3.6)	1089 (-1.8)	300 (-1.4)	24 (-1.4)
	Leporid (all)*	71 (-3.6)	224 (1.8)	67 (1.4)	8 (1.4)
	Artiodactyl Index*	.89	.83	.82	.75
	<i>Odocoileus</i> Index*	.82	.75	.74	.69

\* Rows titled Artiodactyl and Leporid include all specimens only identifiable to the family level, rows titled Artiodactyl (all) and Leporid (all) include the sum of all specimens identifiable to the Family level plus those identifiable to a more specific level. Parenthetical values represent the adjusted residuals from a  $\chi^2$  test and the overall trend was assessed with a Cochran's test of linear trend ( $\chi^2=14.503$ ,  $p = .0023$ ;  $\chi^2_{trend} = 12.40$ ,  $p < .001$ ; see Cannon 2000, 2001). The artiodactyl index is calculated as the sum of all artiodactyl bones over the sum of all artiodactyl remains plus leporid remains. The *Odocoileus* index is similarly calculated as the sum of all *Odocoileus hemionus* remains over all *Odocoileus hemionus* plus *Sylvilagus* spp. remains.

on new characteristics of its own that do not represent the individuals in question (see Robinson 1950).

In a seminal publication on archaeological implications of the topic of scale, Lyman (2003) discusses the influence of time and space averaging on zooarchaeological applications of behavioral ecology. He shows that aggregating spatial and temporal variability in faunal remains can actually remove or reverse patterning apparent at other scales. Thus, researchers must choose to analyze data at the scale that matters to the questions at hand. With archaeological data, the characteristic scale is arguably the site or component, since this is the scale at which human behavior occurs and is reflected by the deposition of material remains. Moreover, since we know that averaging data from the site scale to the regional scale will obscure variation and may change the overall direction of the trend (Lyman 2003), perhaps analyses should begin at the smallest scale possible. By so doing, analyses will preserve the greatest amount of variability thereby providing the greatest potential to explain real trends without obscuring variation.

For example, Hildebrandt et al. use data provided by Rosenthal and Fitzgerald (2009) in an attempt to support their previous conclusions (Hildebrandt and McGuire 2002) and to counter the patterns suggested by Jones, Porcasi, Gaeta and

Codding (2008). These data consist of 24 components from 17 sites along the coasts of San Luis Obispo and Santa Barbara counties (Hildebrandt et al.: Table 1). When the proportion of large terrestrial mammals to small terrestrial mammals in each component are averaged by period and plotted through time, these data show a significant diachronic increase in the proportion of large prey through the Holocene (Spearman's  $\rho = 1$ ,  $p < .0001$ ; see Figure 1a). However, when these data are disaggregated to the component scale and plotted by time period, the significance of the pattern disappears (Spearman's  $\rho = .3077$ ,  $p = .1435$ ; see Figure 1b). When each component is plotted by the median value of its calibrated radiocarbon date B.P., a Spearman's rank order test shows that the trend is not significant at an alpha level of .05 ( $\rho = .3696$ ,  $p = .0755$ ). More importantly, a visual examination of Figure 1c shows that the trend cannot be fitted linearly as a data point occupies almost every area of potential space. This final disaggregation reveals tremendous spatial and temporal variation across the south central coast region of California. Such patterning cannot be fitted reasonably with a linear function as any attempt to do so would disregard tremendous residual variability. In many ways, this is not unexpected as it seems quite unrealistic to look for a single pattern characteristic of an entire region's prehistory.

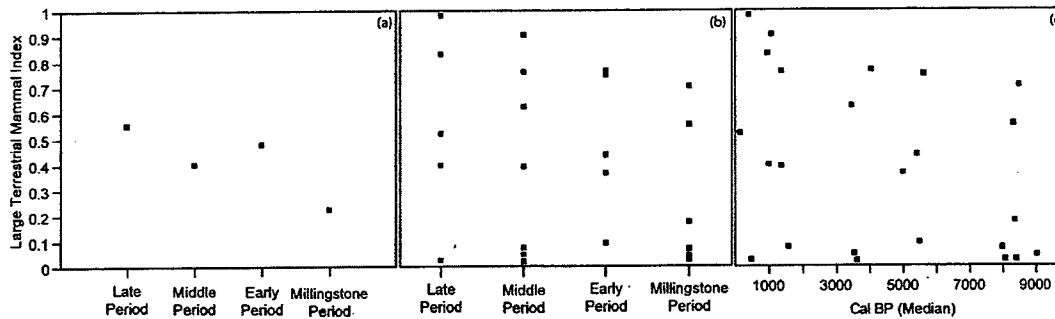


Figure 1. Examination of the effects of time and space averaging on the proportion of large terrestrial mammals relative to small terrestrial mammals along California's south central coast (data from Hildebrandt et al.: Table 1). Frame (a) presents that data as averaged to the level of the time period, frame (b) shows the data disaggregated by component and plotted by time period, frame (c) shows the data by component plotted by Cal. BP midpoint.

Variability in the abundance of large prey across California's south central coast is obscured by the spatio-temporal averaging that Hildebrandt et al. employ, and by their selective sampling of the regional record. As with most applications of behavioral ecology, it is the variability itself that is interesting and requires explanation. Regarding the patterns observed with the Diablo Canyon fauna, we suggest that temporal patterns in the relative abundance of deer are best explained by specific ecological conditions relating to deer population densities in the surrounding area. With a regional data set one could test the hypothesis that variation in frequencies of large prey along the south central coast of California correlates with ecological and historically contingent (in the meaning discussed above) prey densities.

Turning again to the data Hildebrandt et al. provide, a visual inspection of Figure 1c suggests that there may still be patterning in the data: many of the sites appear to have high index values while others seem to have low index values, with a gap in between the two. In order to determine if such groupings exist, we utilized a hierarchical cluster analysis using Ward's method, which examines grouping patterns within the data based on some variable or set of variables (see e.g., Shennan 1997). In this case, groups were clustered based on the proportion of large terrestrial prey relative to small terrestrial prey. Without aggregating the data, this technique allows all the variability to be retained while still attempting to find patterning in the data not limited to a linear trend. While previously obscured by spatial and temporal averaging, the results of this analysis show what appear to be two

completely different site (or component) types: those that show a low abundance of large prey (Type 1) and those that show a high abundance (Type 2) (Figure 2). Both site types occur throughout the Holocene and when grouped in this way, neither type show any significant variation in the abundance of large prey through time (Type 1: Spearman's  $\rho = .0545$ ,  $p = .8810$ ; Type 2: Spearman's  $\rho = .2308$ ,  $p = .4273$ ). All the components at Diablo Canyon (CA-SLO-2) are of the latter type, exhibiting a high proportion of large prey throughout the occupational sequence.

When viewed at this scale, the diachronic increase in the proportion of large terrestrial prey presented by Hildebrandt et al. is in fact a function of the number of site types that occur (or that have been included in the database) during each period and the linear appearance of the trend only results from aggregation. This indicates that the Holocene record in this region is characterized both by a high and low proportion of large terrestrial prey from the beginning to the end of the sequence and the variation between the two depends on some yet unmeasured variable. According to the logic of Hildebrandt et al. (see also Hildebrandt and McGuire 2002; McGuire and Hildebrandt 2005) this unexplained variable would be the degree of prestige hunting occurring at each site, which is linked to the size of social aggregations (although now a spatial rather than temporal trend). However, these groupings do not pair with Hildebrandt et al.'s (see also McGuire et al. 2007) dichotomy between residential bases vs. logistic camps (*sensu* Binford 1980) as Diablo Canyon is both a residential site and has a high proportion of large ter-

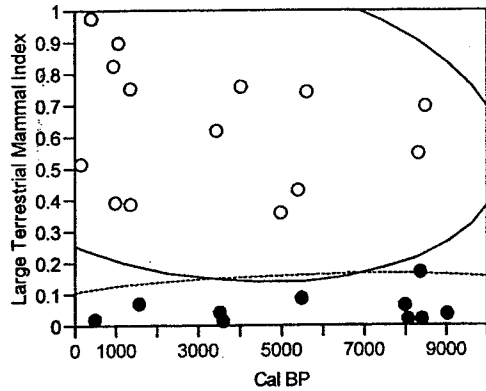


Figure 2. The proportion of large terrestrial mammals relative to small terrestrial mammals per component plotted by Cal. BP midpoint (same as Figure 1c; data from Hildebrandt et al.: Table 1). Groupings are the result of a hierarchical cluster analysis using Ward's Method, clusters based on the proportion of large terrestrial mammals relative to small mammals. The analysis shows that there are two distinct site (or component) types that occur throughout the Holocene record: Type 1 (closed circles) has a low proportion of large prey and Type 2 (open circles) has a large proportion of large prey. Each group is plotted with a 95% confidence ellipse which shows that there is almost no overlap between the two site types, indicating that these differences are statistically significant.

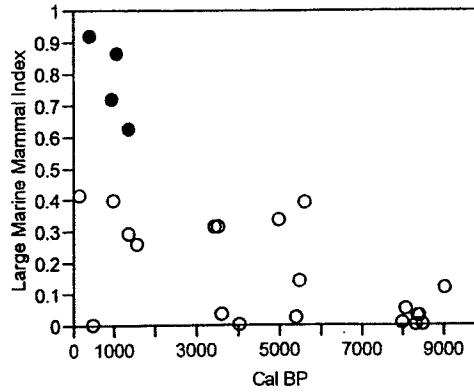


Figure 3. Proportion of marine mammals relative to small terrestrial mammals plotted by component (Cal. BP midpoint; Hildebrandt et al.: Table 1). Components are grouped by hierarchical cluster analysis using Ward's Method based on the proportion of large marine mammals to small mammals. The data show two groups of sites, those with a Marine Index lower than 50% (closed circles) and those higher than 50% (open circles); the latter only occur after ca. 2000 years Cal. BP.

restrial fauna. As an alternative, a more parsimonious conclusion is that this yet unmeasured variable is best characterized as historical contingency relating to prey population densities (see Jones, Porcasi, Gaeta and Coddling 2008). Sites located where deer densities are high show a high proportion of large prey contingent upon higher encounter rates with deer; sites with a low proportion result from lower encounter rates with deer resulting from lower deer densities (see also Hockett 2005; Zeanah 2004). However, these patterns should be considered preliminary until the relationships between pertinent ecological variables and faunal remains can be analyzed with a comprehensive regional data set.

### Foraging for Flightless Prey

In our analysis of the Diablo Canyon fauna, we suggested that the gradual decline in the abundance of the now extinct flightless duck (*Chendytes lawi*) represented a clear case of anthropogenic resource depression where a high ranked resource was over-exploited and gradually replaced by other, lower ranked resources in the same patch, specifically

sea otters (*Enhydra lutris*). Hildebrandt et al., however, suggest that such extinction events can only be explained by reference to a prestige-based model in which the costs of acquisition increased with declining prey density, but were mitigated by the prestige benefits associated with the preys' capture.<sup>1</sup> They argue this pattern continued with ever increasing prestige benefits until the flightless duck was completely eliminated. Simultaneously, they suggest that the prestige benefits once associated with the flightless duck were then passed on to sea otters and other marine mammals, whose exploitation increased. We suggest that these characterizations are at best fanciful for multiple reasons.

Assessing the potential caloric returns of an extinct species is certainly little more than guesswork, but the fact that this marine bird was goose-sized and flightless suggests that it probably would have been a fairly highly-ranked prey item. Recent quantitative ethnographic research has shown that the relative mobility of prey may better predict its ranking than size (Bird et al. 2009), so a bird that cannot fly would seem to us to have the potential to be fairly highly ranked. Further, the vulnerability of the species to overexploitation seems to

explain its eventual extinction. Indeed, flightless birds were rendered extinct all over the Pacific almost immediately following the arrival of human beings (Steadman 1995). The only unresolved issue with *C. Lawi* is why the process took so long in California since recent research has shown that Native people pursued the flightless duck for nearly 8,000 years before rendering it extinct (Jones, Porcasi, Erlandson, et al. 2008).

Hildebrandt et al. suggest that as the population of flightless ducks declined, the prestige associated with the pursuit of the increasingly rare duck motivated hunters to seek out the last remaining animals, eventually causing extinction. Of course, the flightless duck is very different from other species that have been associated by these authors with prestige hunting (e.g., species that are large, fast, elusive and/or dangerous to hunt) such as mountain sheep (McGuire et al. 2007), and large pelagic fish (Hildebrandt and McGuire 2002). While prestige-based hunting of the flightless duck (akin perhaps to the birdman rituals of Easter Island? [Diamond 2005:111]) might have contributed to the species' demise, extinction does not require that every single individual is sought out and destroyed; a species will go extinct when its population falls below a certain sustainable level. For the flightless duck, we suspect that populations were reduced to such levels by Native hunters who exploited nearshore nesting sites, collecting not only hens but their eggs (Jones, Porcasi, Erlandson, et al. 2008:4106). Native hunters in boats would have found these nesting colonies extremely easy to exploit, and by differentially targeting females and their young, would have readily impacted the overall breeding potential of the population over time. Relying on the logic of the prey choice model (with its standard currency), the decline in *C. lawi* remains at Diablo Canyon is fairly straightforward: with increasing predation pressure, *C. lawi* densities declined coincident with a reduction in hunters' encounter rate. While this would result in high search costs if hunters were differentially seeking out the flightless duck, there is no evidence for such behavior. Given the birds' low handling costs (particularly pursuit costs), it should still always have been taken on encounter. Archaeologically, this should result in a lower and lower proportional representation of *C. lawi* until none of the prey were left. We suggest this is the more parsimonious

explanation, and it is fully compatible with the incremental decrease reflected at Diablo Canyon.

Hildebrandt et al. further assert that sea otters were also hunted as prestige items because of the value of their pelts for trade. Rabbitskin blankets were also major trade items in Native California (Heizer 1978:691) and were historically associated with significant prestige (see Sanchez 1973), but Hildebrandt et al. do not mention this alternative incentive for exploitation of rabbits by early Holocene coastal foragers. Sea otters are one of the slowest of all marine mammals (Kenyon 1981) and ethnographic accounts indicate that they were exploited by the following technique:

When the parent otter left its young on the surface of the water, which it did only when it dived for food, the Indian hunter would slip up and tie a cord to the foot of the baby. Fastened to the cord, close to the body of the animal, would be placed a couple of fishhooks. Retiring in his canoe to a short distance, the Indian would pull his cord and thus hurt the small otter so that he would cry. The mother would rush to the rescue and could be easily approached, either because she was occupied in freeing her offspring or because she herself would become caught in the line and hooks [Ogden 1941:14].

In our view, this method of pursuit does not seem comparable to the risky, inefficient techniques used to pursue sea turtles with boats (Bliege Bird and Bird 1997), mountain sheep across rugged terrain (McGuire et al. 2007), or large pelagic fish in open waters (Hildebrandt and McGuire 2002). Rather, for people with watercraft, this seems more like an efficient method for obtaining a highly vulnerable resource.

Not only do Hildebrandt et al. appear incorrect in costs of marine resource exploitation, but as with their examination of terrestrial prey, they aggregate faunal data from their regional dataset so that it gives the appearance of a gradual linear increase in marine mammal hunting through time coincident with the increase in terrestrial mammal hunting (see Hildebrandt et al. Figure 2). When these data are disaggregated to the component level and plotted by date (cal. B.P. midpoint), it shows that foragers did intensify their subsistence efforts on marine mammals in some locations (a total of 4 components), but not in others; moreover, the com-



ponents with a high proportion of marine mammals occur only much later than suggested by Hildebrandt et al.'s prestige hunting hypothesis (compare Figure 3 with Hildebrandt et al.: Figure 2). Rather than emerging as part of a complex of prestige hunting, intensified marine mammal exploitation does not occur until the late Holocene, a time characterized by historically contingent perturbations to the terrestrial environment (see Jones et al. 1999), marked by shifts towards marine resources (see Codding and Jones 2007b; Kennett 2005). This is also consistent with Jones, Porcasi, Gaeta and Codding's (2008) explanation that foragers only turned to the intensive exploitation of marine mammals after the resource depression and eventual extinction of the *C. lawi*—something predicted by the basic prey choice model, and not by the prestige hunting hypothesis. As shown by Jones, Porcasi, Gaeta and Codding (2008: Figure 4), with the over-exploitation of one highly ranked prey species with boats (the flightless duck), foragers began to exploit another lower ranked prey item (sea otters). While these patterns should again be considered preliminary until they can be examined with a comprehensive data set, the preliminary trends are all readily explainable in traditional economic terms.

### Summary and Discussion

As in our earlier critiques of their prestige hunting hypothesis, we commend Hildebrandt, McGuire and colleagues for attempting to interject a new variable into ecological models of western North American prehistory, but we remain unconvinced that costly signaling provides the paramount, overriding explanation for major diachronic patterns in the prehistoric record from this region. Prestige certainly played a part in the daily lives of foragers throughout California's prehistory, but even if one were to ignore the issues of measurement and testability (see Codding and Jones 2007a; Lupo 2007), we are skeptical about the degree to which considerations of prestige (at the cost of subsistence) guided all human hunting decisions in the past. Unfortunately, by introducing this additional variable into explanatory models, these authors have created a situation where we have two alternative explanations for the same trend, both of which have some empirical support. In instances where secondary and tertiary data sources are unavailable,

this will prove problematic. However, in this case, we feel that Occam's razor gives an edge to our interpretations because they are simpler, logically consistent throughout, and explain all of the variability evident in broad regional trends.

Rather than attempting to force a single linear trend onto the past, we suggest that the empirical record, with all its variability, needs to be examined and explained for what it is. Searching for a single trend to explain all prehistoric variation in prey choice is to undermine the behavioral ecological approach altogether, and in so doing, reduces important differences in human subsistence to patterns that do not correspond to human behavior. Instead of attempting to explain away variability, we suggest that research should attempt to account for it. Often this will take place through reference to different historical contingencies that both constrain and allow for various human decisions to occur. Such contingencies include technological and environmental factors that we should expect to vary through time and space and fluctuate with human subsistence residues in important ways.

Competing hypotheses are an important if not necessary component of archaeological research and debates between multiple points of view ultimately enhance our understanding of the past. However, there comes a time when debates grow old. When practitioners attempt to explain away empirical variability in order to support a favored hypothesis, we suggest that it might be time to acknowledge that the hypothesis has failed to take flight.

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## Note

1. Hildebrandt et al. also suggest that the localized extinction of elk may have resulted from a similar process. However, this is the opposite from what they argue resulted from increased prestige hunting of deer. Specifically, they expect that prestige hunting will lead to an increase in the proportion of deer in archaeological sites, but that the proportion of elk will decrease. While deer and elk have very different life histories and behavioral characteristics that may differentially affect their susceptibility to overexploitation (see Whitaker 2008, 2009), this seems to represent a significant inconsistency in predictions derived from the same hypothesis. In addition, while Hildebrandt et al. would prefer that elk went extinct during the Middle Period in order to fit their model for a pan-Californian increase in large game hunting, recent data show that elk were still present in the region at least until ca. 1300 B.P. (see Lebow et al. 2005). Elk also occur in the Late Period component at Diablo Canyon (see Table 1).

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