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A FURTHER ASSESSMENT OF PALEOINDIAN SITE-USE AT BONFIRE SHELTER

Ryan M. Byerly, Judith R. Cooper, David J. Meltzer, Matthew E. Hill, and Jason M. LaBelle

In Byerly et al. (2005) we explored the hypothesis that the Paleoindian component at Bonfire Shelter was the result of a jump kill. Our efforts involved extensive mapping and GIS analysis, a re-examination of the Paleoindian-age bison assemblage, and consideration of the geomorphic history of the canyon in which the site is located. We concluded that the preponderance of evidence indicated the Paleoindian-age bison remains at Bonfire Shelter marked a processing site as Binford (1978) suggested, rather than a primary kill locality as originally interpreted (Dibble 1968). Bement (this issue) raises several concerns about our analysis and discussion, including that we omit pertinent information relevant to the interpretation of the site. His comments, however, result from a misreading of our discussion and a misconstrual of the data set, as we explain in this response.

En Byerly et al. (2005) exploramos la hipótesis que el componente paleoindio de Bonfire Shelter fuese el resultado de una matanza de salto. Nuestros esfuerzos incluyeron la preparación de extensivos mapas y un análisis SGI (GIS), una reexaminación de la asamblea bisonte de fecha paleoindia y la evaluación de la historia geomorfológica del cañón en que se encuentra el sitio. Concluimos que la mayor parte de la evidencia indica que los restos bisontes de fecha paleoindia en Bonfire Shelter demuestran que fue un sitio de procesamiento tal como sugirió Binford (1978), en vez de ser una localidad de una matanza primaria tal como originalmente se propuso (Dibble 1968). Bement (this issue) plantea varios problemas sobre nuestro análisis, entre otras cosas que omitimos la información pertinente que tiene que ver con la interpretación del sitio. Sin embargo, su comentario es el resultado de una mala interpretación de nuestra discusión y los datos, tal como explicamos en esta respuesta.

Byerly et al. (2005) explored whether the Paleoindian-age bison bone assemblage at Bonfire Shelter (41VV218) was the result of a jump kill at that spot, as originally interpreted (Dibble and Lorrain 1968), or alternatively was a locality where carcass parts of animals killed elsewhere were transported and processed (Binford 1978:475). Our study involved three elements: (1) field mapping and Geographic Information System (GIS) analysis of the terrain, in search of drive lanes or topographic features that might indicate how animals were driven toward the cliff edge, and the feasibility of doing so; (2) a thorough reanalysis of the bison remains recovered in the 1963–1964 excavations, to see if the skeletal element patterns indicated a primary kill or secondary processing; and (3) a study of the geomorphic history of the canyon in which the shelter is located, to determine

how much of the Late Glacial valley fill was since removed, a matter relevant to the question of whether bison carcass segments could have been hauled into this shelter, now ~18 m above the valley floor.

Our results, in brief, indicated this would have been an ideal spot for jumping bison relative to other areas in the vicinity of the shelter. However, it may not have been so used. Reanalysis of the bison remains indicated that skeletal parts present at the site were more suggestive of a processing locality. Importantly, we expressly stated this did not speak to *how* the bison were killed, only that these remains did not indicate this was the probable kill locus. And although we were unable to securely determine where the valley floor was in Late Glacial times, evidence suggested it was perhaps just a few vertical meters below the shelter entrance, and hence

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hunters who had made a kill nearby were not impeded by the currently steep slope.

Bement criticized portions of our study, leading with the suggestion we omitted "pertinent information that is directly relevant to the interpretation of the site and to an evaluation of [our] conclusions." As we had explicitly called for more evidence to resolve the use of this site in Paleoindian times (Byerly et al. 2005), Bement's comments, as a participant in the work at Bonfire Shelter in the early 1980s, are welcome. That said, they do not significantly advance the issue, since they are based on a misinterpretation of our arguments and available data. Although we are content to let our paper stand on its merits, we take this opportunity to clarify the nature of the argument and evidence, and expand on our results.

Bement (this issue) asserts we committed a logical fallacy by reasoning that the Paleoindian-age bonebed at Bonfire Shelter (BB2) "did not result from a jump because no other Paleoindian jump site has ever been found." Had we actually reasoned in that manner, he would have been right to make that claim. However, we did not do so. To be sure, we illustrated the distribution of bison jump kills in time and space (Byerly et al. 2005:Figures 1, 13). Yet, this merely reinforces the point, made long ago by Dibble (Bonfire Shelter's original investigator), that Paleoindian bison jumps are scarce on the Southern Plains (Dibble 1970:252; see also Forbis 1969:91). We then went on to agree with Dibble that just because a phenomenon is rare, that is *not* evidence against any particular locality, say BB2 at Bonfire Shelter, being an expression of that phenomenon (i.e., a bison jump; Byerly et al. 2005:597). Bement's comment about our reasoning is therefore incorrect and irrelevant.

Moreover, if using the lack of Paleoindian bison jumps as evidence *against* BB2 being a jump is a fallacy, then why is it logical for Bement to use the Late Archaic bison bonebed at Bonfire (BB3) as evidence *for* a Paleoindian jump? (e.g., "Byerly et al. fail to explore the implications of BB3 which is undeniably the result of Late Archaic bison jumps" [Bement this issue]). The logic cuts both ways, so too does the fallacy. For that reason, we also explicitly omitted BB3 from our discussion.

The scarcity of Paleoindian bison jumps does warrant two questions: first, is there an alternative hypothesis that might account for the facts of the

case? Second, if the facts are as they appear (i.e., BB2 resulted from a jump kill), why does such an anomalous pattern exist in the archaeological record, and how might it be explained (Byerly et al. 2005:626; see also Forbis 1969; Frison 1991)? Some obvious hypotheses explain the latter. For example, repeated jump kills on the Northern Plains (e.g., Frison 1991) were probably the result of intensification strategies geared towards the relief of seasonally constrained resource limitations, such that long-term nutrient storage in the form pemmican production was implemented to subsidize lean winter months (e.g., Reeves 1990; Todd 1987). The absence of such sites on the Southern Plains indicates that hunter-gatherers in the region were never faced with the same limitations, and therefore did not need to use similar acquisition strategies.

But, of course, one must resolve the first question before turning analytical attention to the second. That is why we analyzed the BB2 faunal assemblage, the surface topography, and the geomorphic history of the canyon, and maintained a firewall between the issue of the larger patterning in the archaeological record, and the empirical evidence for/against BB2 being the result of a jump kill.

In regard to that empirical evidence, we deemed it useful to reanalyze the bison from the 1963–1964 excavations rather than rely on published data, which would do little more than replicate other studies (e.g., Binford 1978:475). We realized these data are biased, most notably that they come from a relatively limited area of the site (Byerly et al. 2005:605). Those biases, of course, equally hinder original interpretations of the site. We also did not reanalyze the bison from the 1980s excavations but, as explained, that should not materially impact our conclusions, since that later sample was small (Number of Individual Specimens [NISP] = 51; Bement 1986:26) and represents a minor percentage (< 2.5 percent) of the total remains from BB2 (Byerly et al. 2005:605).

Based on that analysis, we calculated a Minimum Number of Individuals (MNI) of 24 to 27 *Bison antiquus* (Byerly et al. 2005:625). Nowhere do we suggest this is the "total or ultimate" (Bement this issue) number of animals in BB2. The original investigators arrived at a similar MNI estimate, but then arbitrarily multiplied it by four, on the undemonstrated assumption that only half the animals preserved and that unexcavated portions of the

site, particularly the talus cone, contained skeletons in the same or greater density and proportion (e.g., Lorrain 1968:84). This assumption rests on the contention that the bison entered the shelter via the notch in the cliff face. As argued (Binford 1978; Byerly et al. 2005), that may not be the case at all. BB2 bone and lithics (Byerly et al. 2005:Figure 1) are highly concentrated in the southern end of the shelter, and certainly greater numbers are not expected in the unexcavated northern portions of the shelter. Indeed, Dibble notes the bonebed thinned and element frequencies diminished away from the excavations (Dibble 1968:29), a pattern Bement likewise observed (1986:19). Regardless, the matter must be resolved by testing, not through unfounded assertions about a relationship between skeletal counts and site area.

Our analysis of dental patterns indicated BB2 probably represented a single summer kill (Byerly et al. 2005:612). Bement (this issue) points to the difficulty of making such a seasonal assignment, and asserts we fail to consider the criteria that would indicate more than one event took place, namely “the presence of stratified deposits, differential faunal taphonomies, or artifact assemblage differences.” We agree the identification of a single component via dental cohort clustering is a difficult assessment, especially with as small a sample as that from BB2. But it is hardly unheard of (e.g., Todd et al. 1992). Furthermore, we do not preclude the possibility that the dental data may indicate “multiple, closely timed death events” (Byerly et al. 2005:612), nor limit our interpretation to dental data.

We considered the possibility, raised by Dibble (1968) and asserted by Bement (this issue), that BB2 was subdivisible into several strata. However, Dibble (1968:29–30) observed that BB2 is divisible into three strata only on the southern slope of the talus cone and not at all in the “central and northern areas of its occurrence” (Dibble 1968:29). Lorrain (1968:92), in fact, cites the lack of distinguishable strata across BB2 as a reason why bones were not separately analyzed, and why seasonality was likewise not assessed per stratum. These facts unfortunately negate any investigation of “differential faunal taphonomies” (Bement this issue) in the extant assemblage.

For that matter, although Bement (this issue) suggests the “collapse of three strata into one does

not further our understanding of Bonfire Shelter site history or Folsom and Plainview hunting strategies,” it is the only way the available archaeological data can be evaluated. But then perhaps the issue is not so much the separation of these strata, but their overlap. Bement (this issue) asserts that the presence of “both Folsom and Plainview projectile points suggests at least two temporal and societal groups.” That’s hardly a compelling claim on either theoretical or empirical grounds. Projectile point styles are not nicely layer-caked in time; they can and do overlap (Sellet 2001). Moreover, they actually do at Bonfire Shelter. Granting that three depositional episodes compose BB2, Dibble reports that the lowest “stratum,” Component A, yielded both Folsom and Plainview points (Dibble 1968:Table 2; also Cooper and Byerly 2005). Bement’s (this issue) concerns about Folsom and Plainview are thus misguided, even accepting the tripartite stratigraphic division.

Moreover, accepting this division renders meaningless Bement’s estimate of the time spanning and separating the supposed multiple kills. Leaving aside the degree of confidence one can have in a series of radiocarbon ages with standard deviations as large as the ones from BB2 (Holliday 2000:251), and from a period characterized by significant radiocarbon plateaus (i.e., the latter portion of the Younger Dryas; see Blackwell et al. 2006; Kitagawa and van der Plicht 1998), such an estimate necessarily assumes sediment deposition is linear over time. This reasoning is inconsistent with the assertion that three separate depositional episodes occurred.

Although three BB2 deposits (A, B, and C; Bement 1986:Figure 5) were identified in the 1980s excavations, bone is only sparsely present in A, with the major concentration lying at the B/C contact; no differentiation by burning (as seen in the 1960s) is noted (Bement 1986). This, coupled with the reworking of Zone 2a under BB2 in the central and northern deposits (Dibble 1968:21), indicates that a *direct* correlation between Bement’s (1986) and Dibble’s (1968) stratigraphic sequences is perhaps unfounded, and that the BB2 separation could represent an isolated postdepositional burn feature buried by one or more series of talus slope wash events (Byerly et al. 2005:612).

Bement (this issue), following Lorrain (1968:100), further argues that crania and pha-

Table 1. SHEI for "High-Survival Elements" in BB2.

Element	TP ^b	SFUI	MNE	MAU	p_i	$p_i[\ln(p_i)]$	SHEI
Femur	100.0	100.0	33	16.5	.180	-.309	
Tibia	58.1	62.8	26	13.0	.142	-.277	
Metatarsal	15.9	37.0	14	7.0	.077	-.197	
Humerus	28.4	36.8	30	15.0	.164	-.296	
Radius	19.7	25.8	26	13.0	.142	-.277	
Mandible	10.4	11.5	26	13.0	.142	-.277	
Skull	10.4	9.1	5	5.0	.055	-.159	
Metacarpal	6.0	5.2	18	9.0	.098	-.228	
Total			178	91.5	1.000	-2.021	.972 ^a

^aThe evenness value fits only within the range of the bulk transport strategy modeled by Faith and Gordon (2006) at sample size (MNE total) = 150. It also does not fit within the range of values for an unconstrained or non-transported bone assemblage, implying selective transport (Faith and Gordon 2006). Element data from Byerly et al. (2005:Table 2).

^b p_i = Element MAU/ Σ MAU (Faith and Gordon 2006); SHEI = $-\Sigma \{p_i[\ln(p_i)]\}/\ln(\Sigma \text{Element})$, $\Sigma \text{Element} = 8$ in this instance (Faith and Gordon 2006); TP = Total Products (Byerly et al. 2005:Table 2); SFUI = Standardized Food Utility Index.

langes occur in equally higher frequency than they ought to were bison carcass parts differentially transported to this spot for processing. There are two problems with this argument: (1) the cranial MNI of 18 may be inaccurate, and (2) utility modeling incorporates *relative*, not *absolute*, element abundance.

In regard to the former, Lorrain's MNI derivations are problematic. In her Table 9 (Lorrain 1968:80) cranial data are presented according to specified or unspecified portion (maxilla and miscellaneous) and anatomical landmark (auditory bulla), per unit. These data are further summarized per element and element portion in her Tables 18 and 19 (Lorrain 1968:100). The maximum per portion cranial MNI is that for left maxilla (MNI = 11). The cranial MNI of 18 was apparently derived by adding the individually derived MNIs for left and right maxilla as well as that for auditory bulla (Lorrain 1968:80). Since auditory bulla are temporal features, not maxillary, and because it is unknown if maxilla were maximally distinguished by age or overlapping anatomical features (see Hill 2001) in Lorrain's analysis, we considered the maximum maxillary estimate of 11 (Byerly et al. 2005:Table 2) to be the best minimum estimate for BB2 crania, and not the originally reported 18.

Nonetheless, this would still imply that 11 of 24 (~ 46 percent) expected crania are present. This is indeed a large number given that crania ought to be rare at a processing locality. It is not, however, absolute cranial abundance that is important to our transport modeling, but rather the relative frequency (as percent Minimum Animal Units [MAU]) of

crania relative to that of other elements, in this case, high-utility limb bones. As noted, comparison of our derived relative element frequencies to standard bison utility indices best fit a bulk transport strategy ($r_s = .54$, $p = .05$).

However, traditional methods of utility modeling relying on statistical correlation and pattern "eye-balling" (Faith and Gordon 2006) are subjective means of evaluating transport strategies (Byerly et al. 2005:616–618; also Beaver 2004; Faith and Gordon 2006; Rogers 2000; Rogers and Broughton 2001). Faith and Gordon (2006) show that rank-order (r_s) correlations for bulk utility strategies, in particular, do not reliably indicate relationships between economic utility and element frequency at low sample sizes (i.e., Minimum Number of Elements totals [MNE]), such that Type II errors dramatically increase with low element abundance (Faith and Gordon 2006).

Accordingly, and following Faith and Gordon (2006), we reevaluate our original transport inferences here (Tables 1 and 2) in terms of the Shannon Evenness Index (SHEI), using our published MNE data (Byerly et al. 2005:Table 2). These data indicate a cranial MNE of five, based on left incisor counts (NISP = 5, MNE = 5), left maxilla counts (NISP = 7, MNE = 3), and left petrous counts (NISP = 5, MNE = 5). Unconstrained assemblages should be characterized by an evenness of one, indicating no differentiation in relative element abundance, with evenness declining across bulk and unbiased assemblages, and gourmet strategies characterized by highly uneven distributions of elements (Faith and Gordon 2006; also see Todd

Table 2. Spearman's (r_s) Correlations for "High-Survival Elements" in BB2.

Correlations	r_s	p	Inferred Strategy
%MAU vs. SFUI	.59	.13	Weak Bulk
%MAU vs. TPMR ^a	.74*	.04	Bulk
%MAU vs. TP	.83*	.02	Bulk

^aUses the cranial TP value for mandibles (Emerson 1993). Utility values presented in Table 1.

* $p < .05$

1983:Table 24).

At a sample size of 178 (total MNE) for "high-survival elements" (e.g., femora, tibiae, metatarsals, humeri, radii, metacarpals, mandibles, and crania; see Faith and Gordon 2006), the SHEI for BB2 is only consistent with a bulk transport strategy where high-utility upper limbs were selectively transported over low-utility crania and metapodials (Faith and Gordon 2006:Table 4). These data support the hypothesis that BB2 represents the remains of transported bison carcass portions (Byerly et al. 2005; also Binford 1978). Of course, such evidence is only one part of the equation of site-use; hence our exploration of skeletal element articulation and distribution, and our acknowledgment of fluvial activity as a possible taphonomic agent governing those variables within BB2 (Byerly et al. 2005:613).

In regard to the latter, Bement (this issue) cites element co-association, most notably the scapula concentration in N60/W50, and the skull and axial element concentration in N60/W50 and N50/W50-N50/W60 at the base of the talus cone (Lorrain 1968:Table 17, Figure 40), as testimony these animals died in place. Yet, these concentrations were not interpreted as such by Lorrain. Rather, they were taken as indicating deliberate stacking related to "intensive" butchery (Lorrain 1968:96). Lorrain comments that:

The articulations in the lower bed (Bone Bed 2), which were very few in number, usually consisted of only two bones, whereas the upper bone bed [BB3] often contained articulations of an entire limb or an entire vertebral column and, in at least one case, of an entire young bison [1968:93].

Indeed, only seven unspecified articulations are cited (Lorrain 1968:Table 16), including two unfused femur/innominate articulations (Lorrain

1968:Table 14).

However, the degree or lack of articulation may have little to do with butchery activity, and may speak to other taphonomic variables. Lorrain (1968:93) refers to the "mixing of the bones by water that poured through the notch" to explain the low abundance of mandibles in N50/W70. We considered the possible role of water in influencing the distribution and representation of skeletal elements (Byerly 2005:613). Although detailed data on the force and volume of flow from the notch during periods of high-intensity output do not exist, there are indications that water-flow occurred within the shelter and distributed portions of the BB2 deposit (Byerly et al. 2005:613–614).

Bement (this issue) asserts otherwise, but the archaeological patterns he cites are equivocal on this issue, including the "spoke-like" arrangement of bones from a small portion (just ~9 m²) of northern portion of the rockshelter excavated in 1983–1984. We tallied the orientation of the bone elements in this apparent feature (data derived from Bement 1986:Figure 13) in 10° increments between 180° and 360° (to remove mirror imaging; see Lyman 1994:178). Their distribution was then statistically compared to a random model (following Kreutzer 1988; Meltzer 2006). Expected values were derived by dividing the total sample size (in this case 24 elements) by the number of possible classes ($n = 18$), modeling an assumed even distribution. The resulting statistic ($\chi^2 = 13.50$, $p = .70$) shows that the difference between the observed and modeled expected values is insignificant. In non-statistical terms, bone orientations show no preferred alignment or pattern.

In any case, the critical taphonomic question of what pre- and postdepositional processes influenced bone distribution patterns will need to be resolved based on a much larger area than this small portion of the shelter. Here, too, it must be recognized that fluvial action was probably not uniform across the site since some areas, like those nearest the rear wall of the shelter, where the only hearth feature is preserved, or far from the pour-off, are better protected than others. Accordingly, we suggest the "bonebed as mosaic" concept (Todd and Rapson 1999), incorporating an exploration of all taphonomic variables, is a far more illuminating perspective than a simple search for "patterning in cultural remains" (Bement this issue). It is via the

Table 3. Significant Spearman's (r_s) Correlations for Utility among Sampled Bison Bonebeds.

Assemblage ^a	MNE ^b	TP ^c		TF		SKF		MAR		SFUI	
		r_s	p	r_s	p	r_s	p	r_s	p	r_s	p
CSP	643	-.64	.09	-.43	.40	-.43	.40	-.31	.54	-.73*	.03
GN(U)	308	-.90**	.00	-.94**	.00	-.94**	.00	-.83*	.04	-.71*	.05
GN(L)	840	-.83*	.01	-.89*	.02	-.89*	.02	-.77	.07	-.67	.07
AB(AB)	367	-.50	.21	-.90*	.02	-.90*	.02	-.75	.08	-.28	.51
BB2	178	.74*	.04	.84*	.04	.84*	.04	.70	.13	.59	.13

^aAB(AB) = Agate Basin, Agate Basin; BB2 = Bonfire Shelter, Bonebed 2; CSP = Casper; GN(L) = Glenrock, lower; GN(U) = Glenrock, upper. Data from Byerly et al. (2005:Table 2), Hill (2001:Table 3.21), and Todd (1987:Table 2).

^bTotal MNE for "high-survival elements" listed in Table 1.

^cUtility data summarized in Byerly et al. (2005:Table 2); TP = Total Products, TF = Total Fat, SKF = Skeletal Fat, MAR = Marrow Fat, SFUI = Standardized Food Utility Index.

* $p < .05$; ** $p < .01$

former that a truly "systematic investigation following the scientific method" is achieved in bonebed analyses.

Finally, Bement raises several questions about our Figures 14 and 15 (Byerly et al. 2005:622–623). These figures were intended to display BB2 element patterning and butchery activities in terms of the continuum of kill and processing strategies evidenced among Great Plains Paleoindian bonebeds, not to "show the site [BB2] is not a kill" (Bement this issue). We deliberately did not incorporate data from known jump sites because, as we discuss in detail (Byerly et al. 2005:620–621), the vast majority of jump kills are Late Prehistoric in age. We are quite explicit on what we consider to be "jumps" (Byerly et al. 2005:599) and recognize the many variants of this strategy, although (contra Bement this issue) neither Lipscomb nor Olsen-Chubbuck comply with what we would define as a jump kill. Plainview may be an exception, though data to resolve this are lacking (see Holliday 1997:107). Here, beyond the co-association of bison with a slope amenable to jumping, the bison assemblage is also associated with a marsh or pond (Holliday 1997:107; Sellards et al. 1947), which may better explain bison presence in the area and at the site. Thus, Bement's (this issue) comments notwithstanding, data from BB3 and other Late Holocene jumps have no bearing on our Figures 14 and 15.

Furthermore, butchery evidence (e.g., green-break and cutmark frequencies) is neither well-represented nor consistently recorded across Paleoindian assemblages (Todd 1987, 1991; Hill 2001). Our Figure 15 was thus limited by what data was available, hence the incorporation of both NISP and MNE data for green-broken elements from such a variety of sites. Following our previous

analyses, however, and much in line with Todd (1987), we can explore skeletal element abundance and utility across Great Plains kill-butcherries to illustrate numerically, rather than graphically, the position of Bonfire Shelter relative to other sites in terms of inferred activity, regardless of temporal or regional association.

Here, we present a broad-based comparison of element patterning among 13 Great Plains kill-butcherries, incorporating data from 16 components. Percent MAU data for "high-survival elements" (Faith and Gordon 2006) in Paleoindian, Late Prehistoric, and modern bison assemblages, summarized in Todd (1987:235, Table 2) and Hill (2001), are correlated (Spearman's r_s) to Emerson's (1993) bison utility values and Metcalfe and Jones' (1988) Standardized Food Utility Index (SFUI), as presented in Faith and Gordon (2006). Results indicate significant relationships for five of the sampled assemblages including BB2 (Table 3), implying good fits with Binford's (1978) transport models.

These data indicate strong reverse utility profiles for identified kill sites for either total products or fat, including the Late Prehistoric Glenrock bison jump kill and the Agate Basin component kill at Agate Basin (Table 4). A weak total product reverse utility profile also characterizes the Casper kill. Both components of Glenrock likewise show strong selection against high-total utility and high-fat elements, implying probable transport of these elements from the site, although carnivore-induced deletion may be an agent of this patterning (see Todd et al. 1997). The Agate Basin component of Agate Basin displays similar selection against high-fat utility elements. Bonfire Shelter BB2, however, exhibits a disparate pattern for high-total utility and high-skeletal fat limb bones, implying selective

Table 4. Strategies Inferred from Spearman's (r_s) Correlations and SHEI among Sampled Bison Bonebeds.

Assemblage	Strategy (r_s) ^a	SHEI	Strategy (SHEI) ^a
CSP	Reverse Bulk: TP	.967	Reverse Bulk
GN(U)	Reverse Bulk: TP & TF; Unbiased SKF & MAR	.950	Reverse Bulk / Unbiased
GN(L)	Reverse Bulk: TP & TF; Unbiased SKF & MAR	.925	Reverse Bulk / Unbiased
AB(AB)	Reverse Bulk: TF; Unbiased SKF & MAR	.949	Reverse Bulk / Unbiased
BB2	Bulk: TP & TF; Unbiased SKF	.972	Bulk

^aBased on data from Table 3.

transport of these parts to the site. These data do not *directly identify* BB2 as a processing site, but they *strongly suggest* that based on modeled transport behaviors, and in the absence of evidence of significant carnivore activity or density-mediated attrition, selective transport of high-utility elements was a factor in site formation (Byerly et al. 2005).

In this regard, we recognize that utility modeling is by no means a perfect analytical system; it assumes strict behavioral guidelines for activities known among modern hunter-gathers to be logistically highly variable (e.g., Egeland and Byerly 2005; Lupo 2006). Nonetheless, it serves as a reasonable analytical baseline to apply archaeological and mid-range data to understanding prehistoric human lifeways (e.g., Todd and Rapson 1999).

We further agree with Bement (this issue) that understanding Bonfire Shelter depends on knowing "the relationship of the animals to the surrounding terrain," of which the notch above the shelter is certainly one major component. But that is just one component, which makes it all the more puzzling why Bement ignores our comments on the geomorphic history of the canyon. Although our findings as to the elevation of the valley floor in Late Glacial times were inconclusive, that does not mean a plunge into the shelter is the most, or only, plausible mode of entry. Nor should the presence of the cliff and the notch overshadow other possible use(s) of this rockshelter. Rockshelters are well-recognized as landscape features employed in multiple capacities by prehistoric humans and we cannot discount entry into the shelter by either Paleoindians or bison via the canyon floor.

There is no doubt that Paleoindians were capable of and employed multiple strategies to dispatch large herds of bison (Frison 2004), and at no point do we contest this. Furthermore, our interpretation says nothing of where the animals in BB2 were killed (only that it does not appear to have been in the shelter), or what strategy was employed, whether

a jump, canyon trap, etc. (Byerly et al. 2005).

We also recognize the anomalies in our processing-site hypothesis; most obviously, that the BB2 lithic assemblage is dominated by projectile points (Byerly et al. 2005). A processing locality ought to yield more butchering tools and tool resharpening/production debris. In an effort to determine whether this scarcity is real, or possibly a result of fluvial reworking, or even archaeological bias, we have since conducted additional testing at the site and on extant collections. We found some indication of excavator bias (Byerly et al. 2007), but only continued testing and excavation will ultimately resolve this issue and others mentioned. Bement (this issue) indirectly offers a hypothesis in this regard, contending that flakes of rockshelter limestone could have been used by Paleoindian hunters in processing BB2 bison. Testing this hypothesis will require detailed analysis of the limestone spall from the shelter, which certainly presents a formidable task.

Pending such an analysis, or further investigation of taphonomic issues (including, but not limited to studies of excavation bias, rockshelter hydrology, and canyon paleohydrology), or perhaps additional excavations within BB2 itself, we continue to see far more compelling empirical support for the hypothesis that the BB2 bison remains represent a processing locality, as Binford (1978) suggested, rather than a kill, as originally interpreted (Dibble 1968).

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