Unreliable Knowledge About Economic Impacts of Large Carnivores on Bovine Calves

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ABSTRACT Sommers et al. (2010) reported that recolonizing predators increased bovine calf mortality rates in the Upper Green River Cattle Allotment in western Wyoming. However, Sommers et al. (2010) failed to consider multiple competing hypotheses explaining calf loss rates, increasing the likelihood that their results are actually spurious. I reanalyzed their data using a multiple competing hypotheses framework that considered effects of livestock density, summer precipitation, bias in reporting rates, and whether mortality by different predator species was compensatory. I found support for a confounded web of factors influencing calf losses. Calf losses increased with livestock density (which increased during the study), but also during drier summers and with increasing rancher reporting rates. Although both wolves (Canis lupus) and grizzly bears (Ursus arctos) did increase calf losses, the presence of just grizzly bears alone did not significantly increase calf losses. Unconditional estimates of the effects of wolves and grizzly bears on calf losses were only 2.0% (95% CI 0.53–3.81), compared to 3.6% reported by Sommers et al. (2010). Most importantly, however, I report bias in favor of livestock producers in the authors’ assumptions that cast further doubt on the rigor of their results. In conclusion, I recommend managers not consider the spurious predator compensation factors reported by Sommers et al. (2010) to be reliable. © 2011 The Wildlife Society.

KEY WORDS bias, cattle, grizzly bear, human–wildlife conflict, livestock depredation, study design, wolf.

METHODS

Sommers et al. (2010) treated the recovery of wolves in the Upper Green River Cattle Allotment (UGRCA) in Western Wyoming as a natural experimental comparison of the simple...
effects of increasing carnivore treatments (low carnivores, grizzly bears only, grizzly bears + wolves) on calf loss rates in 2 replicates on either side of a river (east and west sides). Sommers et al. (2010) then analyzed these data using an experimental ANOVA (SAS, Cary, NC; PROC GLM) design, testing the hypothesis that predator treatment (grizzly only, grizzly + wolf) increased calf loss rates. On first approximation, this seems like a sound experimental design (i.e., before–after). Wildlife researchers often use natural experiments to test hypotheses about predators (Sinclair 1991). However, a significant weakness is the lack of a contemporaneous control that would result in a before–after control-impact (BACI) design study (Underwood 1997). Indeed, I used a BACI design in a natural experimental comparing the effects of recolonizing wolves on elk (Cervus elaphus) population growth rate in Banff National Park (Hebblewhite et al. 2002). There, I found wolves had strong limiting or regulating effects on the elk population growth rate by comparing elk population dynamics at the same time in areas with and without wolves.

In contrast to Sommers et al. (2010), however, I also considered multiple competing hypotheses to explain elk population dynamics. Sommers et al. (2010) evaluated a single hypothesis: that the addition of grizzlies and wolves were the only factor responsible for increasing calf mortality. To make this argument, Sommers et al. (2010) implicitly assumed that other factors known to influence cattle productivity and calf survival remained constant during the study including climate, husbandry (e.g., stocking density), and reporting rates. Moreover, Sommers et al. (2010) made explicit assumptions about other factors by not considering them, including that both predator density and control for other predators (e.g., coyotes [Canis latrans]) remained constant. Armed with results from their ANOVA analysis, Sommers et al. (2010) then calculated expected calf loss rates by wolves and grizzly bears under the assumption of additive mortality between grizzly bears and wolves. Unfortunately, previous studies have shown complex interactions among climate, density, and predators on the survival of juvenile ungulates, domestic and wild alike (Oakeshield et al. 2003, Tveraa et al. 2003, Griffin et al. 2011). I statistically tested the assumptions of Sommers et al. (2010) and the main hypothesis of a predator effect on calf loss, in a multiple competing hypotheses framework.

First, I developed a set of 24 a priori candidate models to re-analyze the data from Sommers et al. (2010; Table 1). Certainly, it is logical to hypothesize that increasing carnivores would lead to increasing calf loss rates. I also explicitly tested assumptions made by Sommers et al. (2010; Table 1) by formulating the following additional hypotheses: 1) calf loss rates increased during drier spring and summers because reduced forage quality might make calves more susceptible to mortality, 2) calf loss rates were density dependent and increased with increasing stocking rates, perhaps because competition for food during summer (Gillen and Sims 2002), 3) increased awareness by members of the UGRCA of cattle depredation problems increased observer expectancy bias, thereby increasing calf loss reporting rates, and 4) mortality by wolves and grizzly bears was compensatory.

I obtained total summer (May–Aug) precipitation (cm) data for the years 1990–2004 from the closest weather station to the UGRCA (Kendall weather station, Latitude 43°10’/ 109°59’, elevation 2,306 m) from the western United States cooperative weather station program (Western Regional Climate Center 1986). Summer precipitation with the 3 other closest weather stations (Bondurant, Darwin Ranch, Pinedale) were highly correlated ($R > 0.85$, $P < 0.01$), suggesting that small-scale variation in climate variables was unimportant. I calculated the standardized calf stocking density (equivalent to total cattle density because of calf–cow pairing) for each area and used the standardized density as a covariate because area (km$^2$) of the east and west sub-areas (sides) were unreported by Sommers et al. (2010). I calculated reporting rate from Table 1 of Sommers et al. (2010) as the percentage of all ranches grazing cattle on UGRCA that participated in reporting calf losses. I predicted that if there was observer expectancy bias in reporting rate, reporting rate would increase reported calf loss rates. Finally, I used the same predator periods as reported in Sommers et al. (2010) for grizzly-only (1995–1999 west; 1998–2002 east) and wolf–grizzly (2000–2004 west; 2003–2004 east) predation periods. Including the grizzly-only and wolf–grizzly treatments in an additive fashion in an ANOVA, following Sommers et al. (2010), is equivalent to assuming additive mortality by both predators. Thus, if grizzlies and wolves were additive, I expected effects of each predator to be statistically different. To test the compensatory hypothesis, I pooled these 2 predator periods together to yield a combined all-predator treatment in each area (1995–2002 for combined grizzly and wolf treatment on the west side). If mortality between predators was compensatory, then

<table>
<thead>
<tr>
<th>Assumption</th>
<th>Direction (for/against)</th>
<th>Test</th>
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<tbody>
<tr>
<td>1) Mortality by wolves is additive to grizzly bears</td>
<td>Against</td>
<td>Rejected: Beta coefficients of wolf and grizzly not significantly different and Akaike Information Criteria (AIC) support</td>
</tr>
<tr>
<td>2) Livestock husbandry practices remained constant</td>
<td>Against</td>
<td>Rejected: there was substantial variation in cattle stocking density, and cattle loss rates increased with increasing cattle density ($R = 0.46$, $P &lt; 0.001$)</td>
</tr>
<tr>
<td>3) Climate same pre and post-predators</td>
<td>Against</td>
<td>Rejected: low precipitation increased calf loss rates, and higher frequency of low precipitation summers with higher predation</td>
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<tr>
<td>4) Reporting rates remained constant</td>
<td>Against</td>
<td>Rejected: reporting rates increased over the study ($R = 0.89$, $P &lt; 0.001$)</td>
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either the all-predator treatment would be expected to be better supported (i.e., wolves and grizzly mortality is compensatory), or only a single predator treatment (either grizzly or wolf-grizzly) would be expected to be the best-supported model. For example, if only the wolf-grizzly treatment was significant, then there was no statistical difference between the period before grizzlies and after grizzlies, suggesting calf mortality was compensatory with the addition of just grizzlies. It is important to note that this approach (and Sommers et al. 2010) ignores potential changes in wolf and grizzly density and other predators like coyotes and cougars (Puma concolor).

I first screened covariates for collinearity using a liberal correlation threshold of $|R| > 0.5$ because my goal was to examine assumptions of Sommers et al. (2010). I fit a set of 24 candidate models to the data and selected the top models using Akaike Information Criteria (AIC; Burnham and Anderson 1998). I report model selection results for the entire model set and parameter estimates from both the top model and, using multi-model inference, unconditional parameter estimates across all top (0-4 AIC) models (Burnham and Anderson 1998:135). I also used variable importance weights ($\Sigma w_j$) to rank support among factors influencing calf loss. I conducted all analyses using Stata 10.0 (Stata Corp, College Station, TX).

RESULTS

I found significant correlations among alternative covariates, indicating statistical confounding between calf loss, density, and reporting rates. Both cattle density and reporting rates increased significantly during the study from 1990 to 2004 (Fig. 1), as did the reported calf loss. This resulted in correlation coefficients that were close to the arbitrary threshold of $|R| > 0.5$; calf loss rates were highly correlated with density ($R = 0.49$, $P < 0.001$) and reporting rates ($R = 0.57$, $P < 0.001$). These correlations resulted in confounding in subsequent multiple linear regressions. For example, the addition of density to Sommers et al. (2010) model (Table 2) decreased the coefficients of grizzly depredation by 35% and wolf-grizzly depredation by 21%, which is above the 20% confounding threshold recommended by Hosmer and Lemeshow (2000) for inclusion in multiple regression models. Adding precipitation and density (i.e., the top model) decreased these coefficients by 40% and 25%, respectively. Despite these problems, because my goal was to understand multiple competing hypotheses in contrast to Sommers et al. (2010), I proceeded to fit the a priori model set to the data on calf loss. Given this web of confounding factors, however, results from any regression analyses should be viewed with skepticism.

The top model showed that calf loss rates differed between the 2 areas (west side had higher calf loss rates than the east side), decreased with increasing summer precipitation, increased with increasing stocking density, and increased with predation during grizzly-only and wolf–grizzly periods (Tables 2 and 3). The top model explained 67% of the variation in calf loss rates. However, only area, wolf–grizzly period, and precipitation were close to statistical significance in the top model (Table 3). Substantial model selection uncertainty in the model set further reflected this finding (Table 3; as expected because of confounding) and the Akaike weight ($w_j$) for the top model was only 0.30. In comparison, the Sommers et al. (2010) model was ranked 2 with an Akaike weight of only 0.12. Considering variable importance weights ($\Sigma w_j$) to gauge the relative importance of each variable, covariates were ranked in the following order: 1) area $\Sigma w_j = 1.0$, 2) wolf–grizzly predation $\Sigma w_j = 0.79$, 3) summer precipitation $\Sigma w_j = 0.78$, 4) density $\Sigma w_j = 0.65$, 5) grizzly bear $\Sigma w_j = 0.64$, 6) reporting rate $\Sigma w_j = 0.30$, and 7) all predators combined $\Sigma w_j = 0.12$. Unconditional parameter estimates across the top model set were similar (Table 3); only the effects of area, wolf–grizzly depredation treatments, and cattle density were statistically significant at $P \leq 0.05$.

![Figure 1. Univariate relationships between alternative hypothetical factors of (a) reporting rate, (b) standardized calf density, and (c) total summer (May–Aug) precipitation (cm) affecting cattle calf loss rates in the Upper Green River Cattle Grazing Allotment, 1990–2004, Wyoming.](image-url)
DISCUSSION

My reanalysis of the data presented by Sommers et al. (2010) demonstrated that a considerably more complex and statistically confounded set of factors influenced calf loss rates than suggested by the single-factor analysis by those authors. In contrast to the conclusions of Sommers et al. (2010), calf loss rates were a result of several effects including cattle stocking density, precipitation, area, reporting rate bias, and predation during the period when wolves and grizzly bears were present. I found evidence that the combined effects of wolves and grizzly bears only increased calf loss rates by 2.0% (from the original Sommers et al. (2010) model is ranked number 2.


<table>
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<th>Unconditional parameter estimates</th>
<th>Top model</th>
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<tr>
<td></td>
<td>$\beta$</td>
</tr>
<tr>
<td>Area</td>
<td>$-1.06^*$</td>
</tr>
<tr>
<td>Wolf + grizzly</td>
<td>2.07*</td>
</tr>
<tr>
<td>Grizzly</td>
<td>0.96</td>
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<tr>
<td>All predators</td>
<td>0.25</td>
</tr>
<tr>
<td>Precipitation</td>
<td>$-0.07^*$</td>
</tr>
<tr>
<td>Density</td>
<td>0.40*</td>
</tr>
<tr>
<td>Reporting</td>
<td>0.57</td>
</tr>
<tr>
<td>Constant</td>
<td>3.91*</td>
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</table>

* $P < 0.05$.  

(i.e., overlapping zero) or the combined wolf–grizzly treatment. This provided support for the compensatory mortality hypothesis because the addition of grizzly bears did not increase calf loss rate significantly compared to rates prior to grizzlies and because there was weak and statistically overlapping estimates of calf loss during both grizzly and grizzly–wolf combined periods. Recent meta-analyses of >2,000 elk calves across the northwestern United States revealed similar compensatory mortality between predator species in calf survival (Griffin et al. 2011). Therefore, because of the problems of model selection uncertainty, confounding, and rejection of the additive mortality assumptions, the reported compensation adjustment ratios (3.8:1 for grizzlies) reported by Sommers et al. (2010) are likely invalid.

Confounding effects of livestock (stocking) density and precipitation provided further evidence of complexity in factors influencing calf loss. Stocking density is perhaps
one of the most important livestock husbandry practices modified by ranchers, and affects both adult and calf productivity and weight gain (Gillen and Sims 2002, Olson 2005, Holechek et al. 2010). Stocking densities varied widely during the study period, 1,113–2,415 on the west side and 330–1,318 on the east side (Sommers et al. 2010; Table 1). With increasing stocking density came increased calf loss rates (Fig. 1). In hindsight, my a priori hypotheses may be too simplistic to explain this relationship merely through food competition because predation itself may also be density-dependent. For example, carnivores increase kill-rates in response to density and group size for both wild and domestic prey (Hebblewhite and Pletscher 2002, Bradley and Pletscher 2005). Disentangling mechanistic explanations for this relationship with observational data is challenging. Furthermore, despite its weak statistical strength in the top model and across the entire model set, low summer precipitation increased calf loss rates (Fig. 1). This suggested nutritional or behavioral effects of drought that might also render calves more susceptible to mortality. This is also consistent, however, with the predator-induced stress hypothesis argued by Sommers et al. (2010). The weaknesses of observational studies in revealing mechanistic explanations for mortality patterns are evident here because 3 potential mechanisms (cattle density, climate, predator-induced stress) are all consistent with increasing susceptibility of calves to predation. While Sommers et al. (2010) seem predisposed to consider only the predator-induced stress hypothesis, confounding with density and climate casts doubt on such claims and renders their results equivocal.

These problems cast doubt on the economic implications of Sommers et al. (2010) that deserve exploration. First, their anecdotal reporting of the effects of predator removal on calf loss rates should be questioned given their small sample size, lack of control, and confounding variables reported here. Second, the lack of consideration of variation in calf loss rates in calculation of compensation factors is a major problem, especially given the rejected assumption of additive mortality and the non-significant effects of only grizzly bears on calf mortality. Finally, the recommended correction factors of 3.8:1 and 6.3:1 for grizzly and wolf corrections are likely overestimates because of the problems revealed by my reanalysis. As an example of overestimation of the economic effects of predator losses, I calculated monetary compensation factors following calculations from Sommers et al. (2010; Table 3) using unconditional parameter estimates (Table 3) for non-significant effects of grizzly-only mortality, wolf + grizzly, and, for comparison, the effects of cattle density. Sommers et al. (2010) estimated rancher’s losses to grizzlies during 1995–2004 of $260,000 (based on $500 per calf at 2004 prices), and $88,500 of losses resulting from losses to wolves. In contrast, I estimated $69,000 (95% CI $23,000–$168,500) of losses from grizzly bears only and $66,500 (95% CI $25,500–$171,500) assuming compensatory mortality between wolves and grizzly bears as discussed above. Furthermore, for every 1 standard deviation increase in cattle density over long-term averages (an increase of 408 calves on the west side, and 262 on the east) cattle losses totaled $24,500 (95% CI $500–$48,000). Obviously, grizzlies and wolves likely do increase calf mortality rates, but this comparison reveals the unreliability of the estimates reported by Sommers et al. (2010) because of the weaknesses in their experimental design. Given that these estimates of economic losses include 0, further research is needed before economic compensation programs can be designed based on Sommers et al. (2010) recommendations.

However, my main criticism of Sommers et al. (2010) is the failure to consider alternate competing hypotheses. Simultaneously evaluating multiple competing hypotheses improves the scientific process (Chamberlain 1890). This is especially true in ecology and wildlife management because of the difficulty of achieving strict experimental controls, controlling for ancillary variables, and achieving balanced and random sampling (Underwood 1997, Burnham and Anderson 1998, Anderson et al. 2001). Indeed, in the almost 3 decades since Romesburg’s (1981) seminal paper, there have been dramatic changes in wildlife management where the standard is now to consider multiple working hypotheses in an a priori information theoretic framework (Burnham and Anderson 1998). The focus of Sommers et al. (2010) on only 1 simple hypothesis is a dangerous weakness, and the problems I revealed in the pseudo-experimental design included the classic problems of confounding with both ancillary variables assumed to be constant and therefore excluded from the study.

The second major weakness of Sommers et al. (2010) is failure to evaluate multiple competing models and the resultant bias of the model they considered. Reanalysis revealed that as the proportion of ranches reporting data increased during the study, so too did the rates of calf losses (Fig. 1), suggestive of observer expectancy bias on the part of livestock producers in the study area. This is not surprising, as recolonization of predators to cattle grazing areas often increases controversy, vigilance, and familiarity with identifying predator-killed cattle, as Sommers et al. (2010) acknowledged. Dickman (2010) noted that in human–wildlife conflicts the social, ethical, and cultural values of affected stakeholders can contribute to increased perceptions of conflict when none in fact exist. In a multiple competing hypotheses framework, I found some support for such observer expectancy bias (Table 3; reporting rate $\Sigma w_i = 0.30$). Future studies of carnivore–livestock losses should ensure observer expectancy bias is evaluated (Dickman 2010).

Finally, the results of Sommers et al. (2010) were biased more significantly in direction and consistency in favor of demonstrating a stronger effect of predators on calf loss rates due to biased assumptions (Table 1). For example, Sommers et al. (2010) assumed livestock husbandry practices to be constant, yet calf loss rates increased with increasing density. They also assumed climatic variables to be constant, yet my reanalysis revealed this assumption biased results to magnify effects of predation (Table 1). They required additive mortality between predators to turn calf loss rates into compensation factors, but I report support for compensatory mortality between predator species. These biases suggest
that Sommers et al. (2010) approached the factors influencing calf survival rates naively. Their assumptions raise the possibility that they were interested in providing evidence for a hypothesis they considered self-evident, perhaps because of a biopolitical perspective (cf., Urbigköt, 2008, Patterson 2010), or affiliation bias (Murphy 2001). Unfortunately, these approaches often reinforce rather than reduce human–wildlife conflict (Dickman 2010). This may sound like harsh criticism, but given the controversy surrounding carnivore–livestock conflicts, reliable knowledge is needed and studies must be able to withstand scientific scrutiny to guide management. Scientific journals and scientific societies need to strive for rigor in wildlife management and, especially because of the controversy surrounding predator management, editors and practitioners should uphold the highest scientific standards for experimental design, analysis methods, and objective and justifiable inference.

**MANAGEMENT IMPLICATIONS**

The compensation factors reported by Sommers et al. (2010) should not be used by wildlife or livestock management agencies because of the numerous flaws in analyses and biases uncovered by my reanalysis. Where livestock depredation compensation factors are being considered, I suggest use of recommendations from Oakleaf et al. (2003) which used known-fate radiotelemetry to determine cause-specific mortality of cattle calves. However, adjustment factors for live­stock compensation will likely be spatially and temporally variable across livestock–carnivore conflict areas (Rondinini and Boitani 2007). Therefore, simplistic application of adjustment factors from 1 study may not be ideal. Future researchers would do well to carefully weigh and consider alternative hypotheses, carefully test assumptions, and guard against observer expectancy bias when working with live­stock–carnivore conflicts. Instead of observational studies such as conducted by Sommers et al. (2010), to test the hypothesis that wolf and grizzly recovery increases livestock depredation, I recommend a replicated adaptive management experimental manipulation of predator densities in a BACI design. When combined with intensive radio­telemetry on calves to determine cause-specific mortality, it would be the strongest experimental design to test the hypothesis and mechanisms underlying carnivore–livestock depredation. Similar study designs can be found in the wildlife literature (Hayes et al. 2003, Bishop et al. 2009). While such a study may be expensive, the reliable knowledge gained would be extremely valuable, and sorely needed.

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**LITERATURE CITED**


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