Comment on Merz et al. 2025 corrected in 2026: Wolf-hunting in the USA does not seem to reduce domestic animal losses

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Merz et al. (*1*) report another test of the hypothesis that killing wolves prevents future domestic animal losses. At last count, we know of over a dozen such efforts, reviewed in (*2*), some of which we cite below. We appreciate the difficulty of obtaining these data from U.S. state governments and applaud the authors' stamina. Also, the paper is well written and easy to follow. Our concerns center on handling of uncertainty and data quality that suggest (1) standard error bars are much wider than (*1*) consider; and (2) unmeasured potentially confounding variables create a great deal more uncertainty about wolf-killing than the authors' inferences seem to acknowledge. We aim for constructive additions to a scientific debate. We also briefly mention our concern that pro-wolf-hunting interests will take an exaggerated view of its effectiveness in protecting domestic animals (*3*) and thereby distort the nuanced, mixed findings we think (*1*) report.

Study design is not robust to confounding variables or errors

We think we found a simple potential factual error when (*1*) wrote, "...public wolf hunting was illegal in Washington and Oregon throughout the whole period (with the exception of some tribal lands). As the counties in these two states were never 'treated' …(i.e., counties in which legal wolf hunting occurred)… we also ran our models without these two states as a robustness check." p.6 (*1*). A Washington State expert suggested otherwise, "Wolf hunting by the Colville's was limited to the reservation until 2019, when new "… Tribal hunting regulations were created, allowing for a year-round hunting season for wolves on both the North Half and South Half Reservation." (from the 2025 Annual Wolf Report)…The North Half" lies outside the reservation, on treaty lands that they retain hunting rights—it includes about half of Ferry County, a big chunk of Okanogan County, and a slice of Stevens County." (F. J. Santiago-Ávila, PhD, personal communication May 2026).(*1*) used county-periods as the replicates, so we are concerned several years may have been mischaracterized for several counties. Also, it's not clear if the inclusion/exclusion of Oregon and Washington fully addresses the potential error because it appeared that Montana and Idaho produced different patterns than the other states: "The TWFE models remain largely statistically significant when measuring livestock depredation separately for sheep/cattle or in AUM (fig. S6), but not when only including counties from Montana and Idaho in the models (fig. S8)." p.3 (*1*). The inconsistent results across models and states are a persistent worry about the inferred treatment effect in (*1*).

Relatedly, (*1*) wrote, “We use an approach similar to the logic of before/after/control/ impact (BACI) studies that compares geographical units that were affected by a management intervention (wolf hunting) to those that were not, both before and after said intervention.” p.2, (*1*). However, they lacked many variables and data at the scale of their replicates (county-periods); some of these variables are thought to directly influence wolf predation on domestic animals. (*1*) lacked data on livestock husbandry practices and nonlethal deterrents. This is important because the removal of livestock carcasses and other husbandry mechanisms can significantly reduce depredation rates (*4*). Likewise, elk foraging near pastures may affect the risk of wolf predation on livestock in the study area (*5*). Also, hunter take may not be a reliable unbiased estimate of wild ungulate abundance (*6*) and it may not correlate strongly with effort to kill wolves by those same hunters or others. \We are concerned that(*1*) mingled the treatment with numerous other potentially confounding independent variables and unmeasured influences on dependent variables that may differ from county to county or period to period but were not controlled at the fine scale of analysis. The local policies and ecologies that affect wolf-livestock-human interactions may be county-specific and period-specific; a change in either could potentially mimic a treatment effect.

Much has been written by statisticians about ways to improve on BACI designs (*7-9*), and the misleading results one gets from non-randomized designs for ecology, biodiversity, and predator control(*10-13*). Nevertheless, (*1*) did not seem to report livestock losses and effect sizes in their four condition groups: replicates that underwent change from control to treatment to those that underwent the converse, to those that underwent no change in condition. That is a two-by-two matrix structure amenable to reanalysis and hence replication by independent reviewers.

We realize this is demanding and few studies, if any, have achieved it. All the more reason for more caution. We recommend that caution is achieved by combining studies with similar methods and questions to generate consensus and a full discussion of the potentially misleading consequences of unmeasured variables, as we presented in our pre-print posted in mid-2025 (*2*). See below for more details.

Uncertainty about data quality

Furthermore, we worry that uncertainty and systematic bias in some variables were not accounted for completely. The authors rely on state data for wolf abundance and livestock losses, which are not peer-reviewed. Therefore, the published rebuttals to state data claims, both peer-reviewed and non-peer-reviewed, deserve mention, otherwise government claims are assumed true and non-government claims assumed false. That would be a breach of a hallmark of science that it does not respect authority. Therefore, the critiques of Idaho and Montana’s wolf abundance estimates should have been cited as undermining confidence in the covariate(*1*) used for wolf abundance (*14, 15*) and outcome variables including all measures of livestock loss (https://www.humaneworld.org/sites/default/files/docs/HSUS-Wolf-LivestockReport-Feb2019.pdf accessed 25 May 2026). None was cited by (*1*). Although the latter explicitly criticized federal counts of sheep and cattle losses, but not state counts, the reader is not given evidence the state and federal counts are independent or different.

This too might need an explicit, quantitative comparison and justification. Regardless, scientific critiques of input data are essential citations.

We do not suggest all claims be treated equally but those with clearly explained scientific methods deserve mention and those that have undergone peer review in journals that subscribe to COPE (Committee on Publication Ethics) deserve summary descriptions. If the scientific community fails in thorough consideration of data quality, we fail in one of our main missions to provide robust information.

Our concerns about data quality are not trivial issues of random imprecision or non-systematic error, but of statistical bias. Some states with anti-wolf or pro-hunting governments may have had an incentive to inflate costs of wolves so as to promote wolf hunting (*3*). Both inflations would affect wolf abundance and livestock loss estimates. Indeed, the potential over-estimation errors in wolf abundances and livestock losses were likely to vary across states and across time so they are not factors that affect all replicates equally, a potential source of error in inference that (*1*) noted for their models. Below we suggest why the standard errors are overly narrow and thus the mixed results reported by the authors actually represent the lack of a treatment effects.

Given uncertainty in covariates and in response (outcome) variables we mentioned above, the uncertainty around mean coefficient values needs further examination beyond a rigid adherence to statistical software outputs of the mean and the standard error (SE). As statistical packages treat input data as true, they cannot account for the uncertainty about over-estimates which in turn inflate SE around coefficients and regression slopes. If we are correct about bias in sheep or cattle losses, the error bars would expand which is highly relevant to the interpretation of the very small and inconsistent treatment effects reported in (*1*). Therefore, we suggest the conclusions in(*1*) could have been framed more cautiously with greater attention to the risk of false positives and the prevailing literature which seems to conclude that wolf killing has no consistent effect on domestic animal losses (*2*).

Had (*1*) noted theirs was a replication study with new data, readers might have been alerted to many efforts answering the same question(*16-18*) and some recent reviews of this question (*2*). (*11, 19, 20*). Readers may get the misimpression that (*1*) is one of only a few studies to examine this question. Meanwhile, it joins at least six other robust study designs finding mixed results for wolves (#3 below) and other species (*21-24*). Our simulation of study designs for the question asked by (*1*) indicates a high rate of false positives including mixed results with the reversal of sign of correlation and other confusing outputs (*13*).

In 2025, we published a pre-print of an approach to meta-analysis combining 5 replications (*2*). There we reviewed the missing literature mentioned above and discussed why there is no accepted standard for which models to trust more when evaluating the effect of interventions on future domestic animal losses — lagged time periods or alternative ways of measuring losses — because the subfield lacks a standard method (*2*). Therefore, all of (*1*)'s model outputs are interesting but the most important of them is not obvious. Possibly, the authors could have run even more variants of models and seen how this might have changed their conclusions. We think a more informative approach would have been to (a) show mean effect sizes in the two-by-two matrix of possible within replicate permutations described above and (b) enumerate all coefficients close to zero and those that exceed a well-justified threshold based on Bayes factors or other more robust thresholds than $p<0.05$.

In sum, we feel (*1*) leapt to a conclusion based on weak inference and preferential selection of some findings over others without clear justifications. Clear and comprehensive discussions of uncertainty and contradictory results seem especially important for the wolf as a controversial species fueling persistent social and political interests. We worry that advocates for wolf killing would rely on the original uncorrected claims by (*1*). and further erroneously perceive the milder corrected claims to promote wolf hunting.

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