Received: 28 October 2025 | Revised: 28 June 2026 | Accepted: 16 July 2026

DOI: 10.1111/csp2.70388

PERSPECTIVE

Conservation Science and Practice
A journal of the Society for Conservation Biology
Open Access

WILEY

Removing wolves did not reliably prevent domestic animal losses

Adrian Treves[1] | Dror Ben-Ami[2] | Ari M. Cornman[3] | Martin Dul'a[4,5] | Igor Khorozyan[6,7] | Miha Krofel[8] | Miroslav Kutal[4,5] | José V. López-Bao[9] | Rona Nadler Valency[2,10] | Shlomo Preiss-Bloom[2,10] | Francisco J. Santiago-Ávila[11,12] | Jurģis Šuba[13] | Agrita Žunna[13]

[1]Nelson Institute for Environmental Studies, University of Wisconsin–Madison, Madison, Wisconsin, USA

[2]Compassionate Conservation Middle East, The Steinhardt Museum of Natural History, Tel Aviv University, Tel Aviv, Israel

[3]California Fish and Game Commission, Sacramento, California, USA

[4]Department of Forest Ecology, Faculty of Forestry and Wood Technology, Mendel University in Brno, Brno, Czech Republic

[5]Carnivore Conservation Programme, Friends of the Earth Czech Republic, Olomouc, Czech Republic

[6]Scientific Center of Zoology and Hydroecology, Yerevan, Armenia

[7]World Wide Fund for Nature-Armenia, Yerevan, Armenia

[8]University of Ljubljana, Biotechnical Faculty, Ljubljana, Slovenia

[9]Biodiversity Research Institute (CSIC—Oviedo University—Principality of Asturias), Spanish National Research Council (CSIC), Mieres, Spain

[10]School of Zoology, Tel Aviv University, Tel Aviv, Israel

[11]Washington Wildlife First, Copalis Crossing, Washington, USA

[12]PAN Works, Wilbraham, Massachusetts, USA

[13]Latvian State Forest Research Institute “Silava,” Wildlife Management, Salaspils, Latvia

Correspondence
Adrian Treves, Nelson Institute for Environmental Studies, University of Wisconsin–Madison, Madison, WI, USA.
Email: atreves@wisc.edu

Funding information
Spanish Ministry of Science and Innovation, Grant/Award Number: PID2023-149634OB-I00; The Slovenian Research and Innovation Agency, Grant/Award Numbers: J1-50013, P4-0059; Organisation for Economic Cooperation and Development Co-operative Research Programme: Sustainable Agricultural and Food Systems; The Higher Education and Science Committee of Armenia, Grant/Award Number: 25DD-1F069

Abstract

Humans usually try to kill wild animals that they perceive as threats. However, many scientific findings have raised doubts that killing solves wildlife damage to property. With scant, low-quality data, uncertainty persists. We reviewed recent studies of killing gray wolves to protect domestic animals. Our inclusion criteria set a minimum threshold for robust study design and transparent data sharing. In five studies in five countries, killing wolves typically had no effect, may have had the desired effect, but more often killing wolves had the counter-productive undesirable effect of elevating domestic animal losses. We quantified the relative rates of the three outcomes and find the balance in favor of no effect to undesirable effects. We discuss potentially confounding variables and the need for an unbiased, reproducible, transparent, randomized, and controlled trial of predator removal. We find and summarize similarities to other situations from disease transmission to crop damage.

KEYWORDS
carnivores, conflict and coexistence, consensus reviews, livestock, non-randomized BACI, predator control, spatiotemporal recurrence and survival models

1 | INTRODUCTION

Humans usually try to kill wild animals that they perceive as threats. Lethal interventions are common policies in agriculture or biodiversity sectors, especially for managing organisms we value less to protect organisms or personal property we value more. However, scientific findings from around the world have recurrently raised doubts about lethal interventions for solving human-wildlife conflicts, such as disease transmission, crop damage, or predation (reviewed in SI 1).

Because high-quality data on the effectiveness of lethal interventions are scant, uncertainty persists. Scientific uncertainties about the effects of killing on survivors and their ecosystems raise reasonable doubts about lethal management, as it is an irreversible intervention. In the absence of scientific consensus, policy-makers often fail to implement effective interventions or vacillate between constituents' interests.

The ongoing recolonization by gray wolves (*Canis lupus*) of several regions in Eurasia and the USA has produced controversies and efforts to ameliorate coexistence (citations below) and has led many jurisdictions to implement wolf removal. Therefore, we present a consensus view on the effectiveness of killing gray wolves to protect domestic animals. We exploited a modest increase in data about killing gray wolves to protect domestic animals (DA) from five studies in five nations conducted independently in the following chronological order: Slovenia, whole country (Krofel et al., 2011), USA state of Michigan (Santiago-Ávila et al., 2018a, 2018b); Slovakia, whole country (Kutal et al., 2024); Latvia, whole country (Šuba et al., 2023); and Israel, Golan (Nadler Valency et al., 2026). Our inclusion and exclusion criteria followed statistical recommendations in (Allison et al., 2016; Christie et al., 2020; Santiago-Ávila et al., 2018a). Naturally, the exclusion of studies is controversial, so interested readers are provided with full citations to prior reviews and individual studies of note in SI 1. Nonetheless, numerous excluded studies parallel our findings (SI 1).

While varying in study design, all five studies in Table 1 strove to answer a very similar question about the effectiveness of removing wolves to protect DA. Therefore, we consolidated these five efforts as "technical replications with new data" (i.e., very similar methods for addressing the same causal mechanism) following a typology of replication efforts (Treves, Chinn, & Krofel, 2026; Treves, Khorozyan, et al., 2026). The observed killing methods included government agents shooting wolves or live-trapping followed by shooting. Agents also included private individuals or nongovernmental organizations issued permits to shoot wildlife. We use the acronym DA to avoid an anthropocentric bias attached to "stock" and to include dogs and animals that may not fit the former terms well (e.g., equids).

Our study and its findings address ongoing policy debates about wolf–human interactions across the USA and EU especially relating to interventions aimed at reducing conflict or raising tolerance of people for wolves (Chapron et al., 2014; Treves & Bruskotter, 2014; USFWS, 2020).

2 | MATERIALS AND METHODS

Because there has never, to our knowledge, been a reliable, randomized study of wolf killing (SI 1), we included all studies of lower standard that we deemed reliable by statistical and research design standards explained next. We avoided overwhelming bias. The phrase "overwhelming bias" refers to problems in design and analysis identified by the statisticians in (Allison et al., 2016; Christie et al., 2020; Santiago-Ávila et al., 2018a; Treves & Khorozyan, 2025); namely, mistaken analysis of clustering which inflates apparent treatment effects; inappropriate baselines; unexplained or arbitrary discarding of data; or unclear methods without presentation of data for replication efforts. We detail inclusion and exclusion criteria in SI 1. Several of the excluded studies reported mixed results as our included studies did, so decisions to exclude were made a priori based on methods (SI 1). We included studies that (a) used either non-randomized before-and-after control-impact (nBACI, see SI 1 for synonymous terms for these designs) or the analogous time-to-event analyses for recurrence of wolf attacks on DA that treated sites or herds as replicates and measured time to the next event of DA loss (hereafter spatiotemporal recurrence); and also (b) studies that shared data and all methods so we could evaluate if the study did not include researcher bias in selection of subjects, treatments, measurements, or reporting. If raw data were not presented in the original study, we include those data in SI 2.

We prioritized qualitative assessment of the treatment effects of killing wolves across the 5 studies rather than a formal meta-analysis for two reasons. We do not consider

TABLE 1 Five studies of wolf killing in five countries and the subsequent effect on domestic animal (DA) losses.

Site, source	Study design (and comment on handling of temporal autocorrelation TAC, dose D, and treatment bias TB[a])	Spatial and temporal scale of study and subset for the analysis at right	Increase in DA losses (% and net losses[b,c])	No change in DA losses (% and net losses[b,c])	Decrease in DA losses (% and net losses[b,c])
Slovenia (Krofel et al., 2011, SI 2 for data)	Correlation and nBACI (no control over TAC, D, TB)	Comparing DA losses nationwide in year t to those in year $t + 1$ from 1996 to 2009	20%–27% (net loss +39 DA predation incidents)	0%–20%	57%–73%
USA, Michigan (Santiago-Ávila et al., 2018a, 2018b)	Cox proportional hazards and frailty models, that is, spatiotemporal recurrence (no control over TB, tested D, but controlled TAC)	Comparing recurrence of DA losses after wolf killing events to that of no-kill interventions for periods <1 year at 3 spatial scales of the Upper Peninsula, Michigan from 1998 to 2014 ($n = 199$ buffers with or without wolf killing)	78% increased hazard within 92.2 km^2, (spill-over effects)	Inferred no net effect	27% decrease in hazard (2.56 km^2); 28% decreased hazard within 144 km^2
Slovakia (Kutal et al., 2024, SI 2 for data)	Correlation by negative binomial GLMM and a temporal autocorrelation structure ($n = 293$ district-years) and subset nBACI (no control over D, TB, but the subset controlled TAC)	Comparing DA losses, at district level, in year t to those in year $t + 1$ from 2014 to 2019 (General dataset: 25 pairs of sequential years in treatment sites vs. 44 pairs of sequential years in non-randomized set of controls; Refined dataset: 13 pairs in treatment vs. 29 pairs in control.) (SI 2)	Treatment 0–8%, control 0–2%	Treatment 62%, control 91%	Treatment 31–38%, control 7%–9%
Latvia (Šuba et al., 2023, SI 2 for data)	Correlation by negative binomial regression, $n = 21$ sequential years nationwide (no control over TAC, D, TB)	Comparing DA losses nationwide in year t to those in year $t + 1$ from 2004 to 2022 with variable wolf killings (never zero) nationwide	33%–47% (net loss +20 sheep verified lost, or + 195 DA reported killed)	0%–20%	53%–67%
Israel, Golan (Nadler Valency et al., 2026, SI 2 for data)	Cox proportional hazards and frailty models (spatiotemporal recurrence) $n = 468–11$ excluded records $= 457$ comprising 245 DA losses +212 shootings (no control over D, TB, but controlled TAC)	Comparing time to recurrence of DA losses after wolf killing to that of no-kill intervention for periods of 60 days at 2 spatial scales in the Golan Heights 2013–2021	70% increased hazard (unit and segment scales, spill-over effect)		30% decrease in hazard (unit and segment scales)

[a]Temporal autocorrelation (TAC) refers to sites in which losses in 1 year correlate to losses in the following year, even when no intervention was attempted. Dose (D) refers to how many wolves were killed or how much effort was made to kill wolves. Treatment bias (TB) refers to killing more wolves where more losses had occurred.

[b]%: estimated as the proportion of pairs of replicates for non-randomized before-and-after control impact (nBACI) or correlation studies or % change in hazard for spatiotemporal recurrence. For nBACI and correlation studies, we estimated the net DA loss across all replicates where + signifies an increase (undesirable, counter-productive effect).

[c]The 3 rightmost columns are a qualitative and ranked estimate of treatment effects for cross-study comparative measure analogous to what an observer might perceive as the outcome of a change in legal wolf killing from time t to time $t + 1$ across spatial units. The original within-study statistical analyses should be consulted to see estimates of treatment effects in each study. The ranges of values indicate whether one includes years with zero wolf killing or not, and whether the original study presented two measures of DA loss. SI 2 for description of methods.

the field ripe for a quantitative meta-analysis because the field lacks standardized measures of treatments let alone outcomes (SI 1 and 2). Without standardized method of intervention (methods for killing wolves differed somewhat in every study) and outcomes (DA losses were measured, verified, and reported slightly differently), we do not believe the field is ready for formal meta-analysis yet. We inferred the desired effect (DA losses diminished), no effect, or the undesirable effect of an increase in DA losses, separately for each study. Thus, we have a semi-quantitative measure of treatment effect analogous to +1, 0, or −1. We quantified treatment effects within studies in Table 1.

Here we describe a method for cross-study comparison of DA losses following wolf-removals. In Table 1, we present that cross-site comparison for each study's quantitative treatment effect for comparison purposes. For the two studies that used time-to-event analyses (Michigan, USA and Golan, Israel) we inferred a net treatment effect of removing wolves directly from hazard ratios in the published studies (Santiago-Ávila et al., 2018a, 2018b) and (Nadler Valency et al., 2026), respectively. For studies that used nBACI or simple correlation, we had to estimate similar, meaningful comparisons of treatment effects across studies (Table 1). For two datasets, we group the studies in Slovenia (Krofel et al., 2011), and Latvia (Šuba et al., 2023), as country-wide nBACI in which pairs of sequential years are treated as the before-and-after measures and the intervention changes from year to year fluctuating up and down including zero wolf-killing in some years (SI 2). For Slovenia and Latvia, we developed the following method that allows comparisons across studies. We calculated the change in the number of wolves killed (year $t+1$ – year t) and the change in the number of DA lost (year $t+1$ – year t) as they were reported in the original studies. Our handling of the Latvia and Slovenia studies is distinct from the Slovakia dataset (Kutal et al., 2024) in which, at the district level, the authors selected a non-randomized set of controls (districts with zero wolf-killing in a pair of sequential years) to compare to treatment sites (districts with >0 wolf-killing that occurred at the end of the first year "before", whereas the second year was considered as "after," see (Kutal et al., 2024) for details and SI 2 for data). Our challenge was to devise a method for cross-study comparisons across the three nBACI sites in Table 1, which could be interpretable for a qualitative conclusion about treatment effects. In the Slovakian case, it may be more appropriate to compare the outcome from Slovakia with the other studies focusing on treatment sites (i.e., with wolf-killing >0), because all the Slovakian control sites had zero wolf-killing. However, we consider it also informative to present the outcome from control sites separately (wolf-killing = 0), because it informs us about the heterogeneity in the outcomes of DA losses without intervention. Therefore, Table 1 for Slovakia presents results for both treatment and control (no wolves removed) conditions.

In short, we test the traditional hypothesis that increasing the reported wolf-killing will reduce DA losses or the newer hypothesis that killing more wolves will increase DA losses, "throwing water on the fire" in the former prediction or "adding fuel to the fire" in the latter prediction, following formal hypotheses proposed in (Treves et al., 2019). In both approaches (with or without years with no change in wolf-killing) we then tallied up the number of years in which the desired effect was seen (reduction in the number of DA lost), undesirable effect (increase in the number of DA lost), or no effect (no change in the number of DA lost). For the Slovakian study, we report the average and variability in DA loss in both control and treatment replicates (see Table 1).

In Tables S1–S6, we considered the potential problem that wolf-killing rates in one study were not measured equivalently as wolf-killing rates in another study. Relatedly, in the same section of SI 2 we examine the problem of undetected and unmeasured wolf-killing from illegal or unreported causes. These uncertainties in treatment dose could confound a potential cross-site assessment of treatment effects. We discuss the potentially confounding effect of unreported wolf-killing in SI 1, to justify our prediction that the error produced would be random, not systematically under- or over-estimating dose effects of wolf-killing. For these reasons, we assume our inferences about treatment effect are not comparing zero wolf-killing to some wolf-killing but comparing low wolf-killing to higher wolf-killing within the five studies for which we have raw data (SI 2). Such within-site comparisons further preclude a meta-analytic quantification of treatment effect sizes. We limit ourselves to qualitative cross-site comparisons and call for larger, randomized trials to overcome these uncertainties.

2.1 | Ethics

See the five original studies for ethics disclosures. For this review, we did not need research subjects or permits.

3 | RESULTS

We found that the effect of killing wolves to protect domestic animals (DA) is highly unpredictable, as recently practiced in five studies (Table 1). Killing wolves typically had no effect; less commonly had the desired effect; and also, less commonly had the counter-productive undesirable effect of elevating DA losses (hereafter undesirable effect).

All five studies showed some desirable effects and some undesirable effects (Table 1). The consensus result was no effect. We estimated the relative rates of the three treatment effects, using a cross-study metric described in SI 2. We conclude from that estimate that the balance falls in favor of no effect to undesirable effects.

Three studies originally reported statistically significant effects at $p<.05$ (Kutal et al., 2024; Nadler Valency et al., 2026; Šuba et al., 2023). Three found a preponderance of undesirable effects yielding higher DA losses (Kutal et al., 2024; Nadler Valency et al., 2026; Santiago-Ávila et al., 2018a, 2018b). The three nBACI studies allowed us to quantify the annual rates of different treatment effects at either national or subnational levels (Table 1, SI 2). The other spatiotemporal recurrence models do not permit such a straightforward description of desirable and undesirable effects, although they agreed on the occurrence of spill-over effects (Nadler Valency et al., 2026; Santiago-Ávila et al., 2018a, 2018b). The spill-over in this context refers to DA losses due to predation in neighboring livestock herds after wolves were killed. Potential spill-over effects emphasize the need for standardized spatiotemporal methods and control over variables that vary between herds, such as herd protection methods (Nadler Valency et al., 2026).

4 | DISCUSSION

From the five field studies with similar methods and shared data, we find consensus that killing wolves to protect domestic animal losses (DA), as currently practiced, was highly unpredictable. Killing wolves typically had no effect, may have had the desired effect but more often wolf-killing had the counter-productive undesirable effect of elevating DA losses. We quantified the relative rates of the three outcomes to infer qualitatively that the balance favors no effect to undesirable effects.

Our exclusion criteria did not slant our main conclusion of mixed treatment effects, because several of the prominent excluded studies also reported those mixed effects, albeit without sharing the data allowing us to replicate their findings (SI 1). In SI 1, we summarize several proposed explanations for the mixed treatment effects.

Strength of inference was not uniform across the studies (Table 1). Sample sizes differed and study designs differed in robustness (Christie et al., 2020; Treves & Khorozyan, 2025). We agree that the highest standard of a reliable, reproducible randomized controlled trial (RCT) on removal of wolves is needed. We recognize the obstacles to such an RCT include organizational will, funding, and stringent planning. Yet, the examples of reliable RCTs of lethal interventions against smaller carnivores, for example, badgers (*Meles meles*) in the UK to prevent transmission of bovine tuberculosis to cattle, and also, poisoning red foxes (*Vulpes vulpes*) and dingoes (*Canis familiaris dingo*) in Australia to prevent sheep losses (Bielby et al., 2016; Greentree et al., 2000; Campbell et al., 2019; Allen et al., 2020; SI 1 for summary of several other badger and dingo studies), suggest that at least some of the obstacles can be practically overcome when favorable conditions align. However, as with wolves, the badger, fox, and dingo studies also reported mixed and unpredictable outcomes of lethal interventions with slight desired effects and occasional undesirable effects (similar results for crop loss and other property are summarized in SI 1).

We report consensus on common, unmeasured confounding variables. We agree on the need to reduce uncertainty surrounding the identification of wolf kills versus scavenging or predation by other species including dogs (studies summarized in SI 1). We also agree on the importance of identifying the wolves killed that were uninvolved, relative to those actually involved in predation on DA. We also agree on the modest quality of the essential evidence collected by governmental agencies in the five studies we reviewed (SI 2). These sources of error may be systematic or random and appear in part or in whole in each of the five studies (Table 1). Following the below discussion of potentially confounding variables, we outline what we see as minimum record-keeping to remedy the current gaps in scientific data.

We also agree on and stress the importance of variability in wolf-DA-human interactions caused by, but not limited to, individual wolf behavior; pack density, dynamics, and distribution including social structures and behaviors before and after any intervention; vulnerability of DAs including human behavior in both husbandry and intervention; and the presence or abundance of surviving predators (of any species) to replace those removed (Summaries in SI 1). All of these variables and perhaps, particularly, wild food availability or appearance in wolf diet and DA protection methods (e.g., fencing, herders) may confound interpretations of treatment effects.

Wolf removal studies have sometimes tried but invariably failed to account for some or all of the above variables scientifically. That is not to say every single variable in a wolf-DA-human landscape should be rigorously measured. Rather, unbiased RCTs or better "platinum-standard" cross-over designs with blinding (Khorozyan, 2021; Mukherjee, 2010; Treves et al., 2019; Treves & Khorozyan, 2025) and adequate sample sizes might be needed to overcome much of the complexity of potentially confounding effects ab.

We emphasize three common potentially confounding variables (temporal autocorrelation TAC, dose effect D, and treatment bias TB) in Table 1, which often produce errors in non-randomized studies. Such errors include false positives (inferring a treatment when none exists) and reversal of the sign of relationship (inferring the opposite effect than the true effect). The three potential confounding conditions (TAC, D, and TB) deserve special mention because they may frequently produce false discoveries or diametrically opposite inferences about treatment effects in exactly the study designs and animal systems we review here (Treves & Khorozyan, 2025).

Temporal autocorrelation: A common potentially confounding variable in wildlife removal studies is spatial or temporal autocorrelation (TAC in Table 1) between the losses in domestic animal herds or sites in time t and $t+1$ or location x and adjacent locations (Treves & Khorozyan, 2025). If a herd or site is subject to repeated losses over time, or different herds face losses in the same places (hereafter hotspots), one has to account for autocorrelation independent of a treatment effect. Such autocorrelations are a common condition in wolf-human interactions and a well-known problem for non-randomized study designs (Christie et al., 2020; Treves & Khorozyan, 2025). Hotspots can pose several problems for wildlife removal studies. Hotspots can arise due to neglect of property or inadequate husbandry practices (e.g., grazing near core wolf areas without herd protection); poor health or vulnerable condition of DA; or repeated use of landscape features (e.g., rugged topography or dense vegetation). Under such conditions, losses may escalate independent of predator variables, and morbidity of DA may contribute to perceived predation and elevated scavenging rates. This may lead to temporal or spatial autocorrelation of losses independent of predators or their removals. All of the studies we summarize here reported hotspots of DA losses, yet it can be very challenging to statistically account for spatiotemporal autocorrelation of hotspots before evaluating treatment effects (Santiago-Ávila et al., 2018a). The latter study and (Nadler Valency et al., 2026) used a variant of survival models called frailty models, which accounted for farm identity in the overall pattern of recurrence of losses and also addressed spatial autocorrelation in losses (Nadler Valency et al., 2026; Santiago-Ávila et al., 2018a); one of those studies also excluded farms with chronic losses, (see SI in Santiago-Ávila et al., 2018a). Even in randomized studies, investigators may need to account for spillover effects and the wider spatiotemporal distribution of effects of wildlife removal. The correlation and nBACI studies in Table 1 could not correct for temporal autocorrelation except when selecting a subset of control and treatment sites (Kutal et al., 2024; Santiago-Ávila et al., 2018a).

4.1 | Dose (D)

A different, potentially confounding variable is the "dose," by which we mean the intensity of removal of wolves either measured as the number of wolves removed or the intensity of human effort to remove the same number of wolves. Increasing the dose in response to subjective perceptions of a need is a common human reaction to wildlife conflicts (Treves & Khorozyan, 2025, and SI 1). After suspected predation, people tend to kill more predators around sites or herds experiencing higher losses. Using a biomedical analogy, providing more medicine to those who suffer more frequent or more severe symptoms might be sensible. One can imagine when it is sensible (e.g., more serum for the patient who has lost more blood) and when it becomes an unreasonable dose (e.g., massive doses of aspirin for a severe headache). Regardless of how one takes medicines or values wolves, it is unclear if increasing the dose is appropriate when removing wild wolves. Correlations between the dose and prior losses produce high rates of error about the treatment effect of wolf removal if one cannot exploit a randomized study design, as shown by simulations (Treves & Khorozyan, 2025).

4.2 | Treatment bias (TB)

Varying a dose subjectively as above would constitute TB. Yet other variations of treatment bias exist, particularly in non-randomized studies. Because many wolf attacks on DA occur once without recurring (SI 1), the assumption that one should kill more wolves where more DA were reported lost cannot be practically validated without a control or counterfactual—what would have happened without an intervention? Lack of a "zero dose" or an appropriate baseline or control condition would also introduce TB, when sites with no wolves killed were not studied. All included studies measured outcomes after zero losses. For example, Nadler Valency et al. (2026) reported on two herds in farms contrasting sharply in husbandry. One farm implementing frequent wolf shooting faced persistent losses, while the other implementing no wolf shooting had the lowest rate of loss.

Furthermore, regarding TB, the methods used for removing wolves are important and rarely standardized or described scientifically. Namely, the design of interventions—such as the precise, verified locations of wildlife-removal related to locations of losses; the intervals of time elapsing between a loss and an intervention; the deployment and design of removal methods, and whether they are direct (e.g., shooting) or indirect (e.g., trapping)—are all essential to strong inferences about the effectiveness of wildlife-removal. Failing to describe interventions can elevate TB and other biases (Allison et al., 2016). Standard procedures for design and administration of treatments along with 'blinding' (concealing which subject received which condition) should reduce TB (Khorozyan, 2021; Mukherjee, 2010; Treves et al., 2019; Treves & Khorozyan, 2025).

4.3 | Limitations

We conclude that no single study can capture the ultimate effect of removing wolves at a site (or probably any other species, see SI 1). That inference seems especially valid when one lacks randomized controls, a large sample of independent replicates (n >100), and accounting for the two key confounding variables of TAC and TB discussed above. Even the results of RCTs can be site-dependent under clinical or field trials. For example, the timing of intervention may critically affect its success. This reminds us that contentious debates between teams of investigators with opposing conclusions demand more evidence because both teams can be correct or both can be wrong (Christie et al., 2020; Treves & Khorozyan, 2025).

5 | CONCLUSIONS

In conclusion, wolf removal to protect domestic animals (DA) as recently practiced in the five studies we reviewed (e.g., government agents arriving at the scene long after the loss of domestic animals, often removing wolves far from the original sites) seems poorly designed for the intended effects. Moreover, the risk of undesirable effects and spill-over effects into neighboring herds or subsequent years raise doubts about removing wolves. Simple adherence to the precautionary principle would warn against such an intervention, as recent Court of Justice of the European Union decisions on wolves declare (Trouwborst, 2026). Given the latter judgments that EU member countries will have to use the best scientific knowledge and precautions to maintain wolves at "favourable conservation status," we suggest government data collection about wolf-killing needs enhancement.

We recommend more care in data collection to avoid the waste of scarce public resources on ineffective interventions and insensitive research. We recommend government agencies train and deploy rapid response teams that would monitor complaints or respond to imminent risks, as follows: (a) visiting all farms in wolf range at least annually; (b) with a systematic protocol for data collection; (c) including zeros (no losses occurred, no wolves killed) in the latter protocol; (d) annually update records of the presence and condition (functional or neglected) of DA protection methods; and (e) herd manager perceptions of wolf presence and wild prey presence (absent, rare, frequent). When such teams respond to imminent risk, we recommend they enhance protections and monitor results faithfully thereafter. When such teams respond to losses, they should be trained and equipped to verify if predation and which predator was involved. Blind trials of the accuracy of verification by the personnel in such teams would go a long way to improving the scientific rigor of data on wolf-DA interactions. Hopefully, such simple data collection methods would improve the field's deficiencies in measuring some of the confounding variables we described above. Inevitably, some funders might balk at financing field teams visiting all DA properties in a large wolf range. Perhaps, this article and clear explanations of the impossibility of drawing inference about interventions without such data would be persuasive.

Given that wolves and DA lives and human property may be lost and human socio-economic and conservation values are at stake in this issue, this is not a question of wildlife removal versus animal welfare, rights, values or moral concerns. Rather, this is a scientific debate over the rationality and effectiveness of applications of lethal versus non-lethal interventions.

Regardless of scientific evidence, human preferences or value judgments could prevail in some jurisdictions. If that and other undesirable outcomes we have discussed above, were to surface in public policy, then we urge the involved policy-makers to transparently separate values from evidence. This could be done most simply by the final decision-maker stating, "Despite the evidence that removing wolves does not generally protect DA, we will remove wolves anyway because we value X." (Here, X is a normative, moral argument, not an assertion about evidence for effectiveness.) For example, X could be "public safety comes first," "hunters need more access," "we don't trust science" (Treves et al., 2024). In short, when decision-makers foreground values over evidence they should transparently disclose that priority rather than claim the value is evidence-based. Such transparency from decision-makers might help the public understand why decisions were made.

AUTHOR CONTRIBUTIONS

Conceptualization: A.T. *Methodology*: A.T., J.V.L.B., M.Ku. *Investigation*: all co-authors. *Funding acquisition*: A.T., D.B., R.N.V., S.P. *Project administration*: A.T. *Supervision*: A.T. *Writing—original draft*: A.T., I.K., M.Kr., M.Ku., J.V.L.B. *Writing—review and editing*: all co-authors.

ACKNOWLEDGMENTS

This work reflects the views of the authors and not necessarily of their institutions and affiliations. See original articles for acknowledgments and funding related to each of the five original studies. AT was supported by a fellowship from the OECD Co-operative Research Programme:

Sustainable Agricultural and Food Systems in 2022. DB-A, RNV, SP-B thank The Israel Nature and Parks Authority for sharing data S5. IK is currently supported by the Higher Education and Science Committee of Armenia (grant 25DD-1F069). MKr was supported by the Slovenian Research and Innovation Agency (grants no. J1-50013 and P4-0059). JVLB thanks the Spanish Ministry of Science and Innovation (grant PID2023-149634OB-I00).

CONFLICT OF INTEREST STATEMENT

Authors perceive no competing interests but share their CVs here https://faculty.nelson.wisc.edu/treves/CCC.php to allow readers to judge potentially competing interest whether financial or non-financial.

DATA AVAILABILITY STATEMENT

All data are available in SI 2, if the data were not published in the original sources.

ETHICS STATEMENT

See the five original papers for individual ethics statements. This review used no research subjects.

ORCID

Adrian Treves https://orcid.org/0000-0002-3052-4708
Dror Ben-Ami https://orcid.org/0000-0002-7004-1284
Ari M. Cornman https://orcid.org/0000-0002-4692-2874
Martin Dul'a https://orcid.org/0000-0003-2675-4575
Igor Khorozyan https://orcid.org/0000-0002-0657-7500
Miha Krofel https://orcid.org/0000-0002-2010-5219
Miroslav Kutal https://orcid.org/0000-0003-3857-5419
José V. López-Bao https://orcid.org/0000-0001-9213-998X
Rona Nadler Valency https://orcid.org/0009-0005-7025-0405
Shlomo Preiss-Bloom https://orcid.org/0000-0002-8946-931X
Francisco J. Santiago-Ávila https://orcid.org/0000-0003-4233-9128
Jurģis Šuba https://orcid.org/0000-0001-5639-5863
Agrita Žunna https://orcid.org/0000-0002-5830-5523

REFERENCES

Allen, L. R., Barnes, T. S., Fordyce, G., McCosker, K. D., & McGowan, M. R. (2020). Reproductive performance of northern Australia beef herds. 8. Impact of rainfall and wild dog control on percentage fetal and calf loss. *Animal Production Science*, *63*(4), 388–394. https://doi.org/10.1071/an19430

Allison, D. B., Brown, A. W., George, B. J., & Kaiser, K. A. (2016). Reproducibility: A tragedy of errors. *Nature*, *530*, 27–29. https://doi.org/10.1038/530027a

Bielby, J., Vial, F., Woodroffe, R., & Donnelly, C. A. (2016). Localised badger culling increases risk of herd breakdown on nearby, not focal, land. *PLoS One*, *11*(10), e0164618. https://doi.org/10.1371/journal.pone.0164618

Campbell, G., Coffey, A., Miller, H., Read, J. L., Brook, A., Fleming, P. J. S., Bird, P., Eldridge, S., & Allen, B. L. (2019). Dingo baiting did not reduce fetal/calf loss in beef cattle in northern South Australia. *Animal Production Science*, *59*(2), 319–330. https://doi.org/10.1071/an17008

Chapron, G., Kaczensky, P., Linnell, J. D. C., von Arx, M., Huber, D., Andrén, H., López-Bao, J. V., Adamec, M., Álvares, F., Anders, O., Balčiauskas, L., Balys, V., Bedő, P., Bego, F., Blanco, J. C., Breitenmoser, U., Brøseth, H., Bufka, L. k., Bunikyte, R., … Boitani, L. (2014). Recovery of large carnivores in Europe's modern human-dominated landscapes. *Science*, *346*(6216), 1517. https://doi.org/10.1126/science.1257553

Christie, A. P., Abecasis, D., Adjeroud, M., Alonso, J. C., Anton, A., Baldigo, B. P., Barrientos, R., Bicknell, J. E., Cebrian, J., Ceia, R. S., Cibils-Martina, L., Clarke, S., Claudet, J., Craig, M. D., Davoult, D., De Backer, A., Donovan, M. K., Eddy, T. D., França, F. M., … Buhl, D. A. (2020). Quantifying and addressing the prevalence and bias of study designs in the environmental and social sciences. *Nature Communications*, *11*, 6377. https://doi.org/10.1038/s41467-020-20142-y

Greentree, C., Saunders, G., McLeod, L., & Hone, J. (2000). Lamb predation and fox control in south-eastern Australia. *Journal of Applied Ecology*, *37*, 935–943. https://doi.org/10.1046/j.1365-2664.2000.00530.x

Khorozyan, I. (2021). Dealing with false positive risk as an indicator of misperceived effectiveness of conservation interventions. *PLoS One*, *16*(5), e0255784. https://doi.org/10.1371/journal.pone.0255784

Krofel, M., Černe, R., & Jerina, K. (2011). Učinkovitost odstrela volkov (*Canis lupus*) kot ukrepa za zmanjševanje škode na domačih živalih. Effectiveness of wolf (*Canis lupus*) culling as a measure to reduce livestock depredations. *Acta Silvae et Ligni*, *95*, 11–22. (Slovenian) https://www.researchgate.net/publication/233792224_Effectiveness_of_wolf_Canis_lupus_culling_to_reduce_livestock_depredations

Kutal, M., Duľa, M., Selivanova, A. R., & López-Bao, J. V. (2024). Testing a conservation compromise: No evidence that public wolf hunting in Slovakia reduced livestock losses. *Conservation Letters*, *17*(1), e12994. https://doi.org/10.1111/conl.12994

Mukherjee, S. (2010). *The emperor of all maladies: A biography of cancer*. Scribner.

Nadler Valency, R., Shavit, G., Preiss-Bloom, S., Margalit, S., Ben-Ami, D., & Dayan, T. (2026). Effects of lethal and non-lethal wolf (*Canis lupus*) management. *Ecological Solutions and Evidence*, *6*(4), e70157. https://doi.org/10.1002/2688-8319.70157

Santiago-Ávila, F. J., Cornman, A. M., & Treves, A. (2018a). Killing wolves to prevent predation on livestock may protect one farm but harm neighbors. *PLoS One*, *13*(1), e0189729. https://doi.org/10.1371/journal.pone.0189729

Santiago-Ávila, F., Cornman, A., & Treves, A. (2018b). Correction: Killing wolves to prevent predation on livestock may protect one farm but harm neighbors. *PLoS One*, *13*(2), e0209716. https://doi.org/10.1371/journal.pone.0209716

Šuba, J., Žunna, A., Bagrade, G., Done, G., Ornicāns, A., Pilāte, D., Stepanova, A., & Ozoliņš, J. (2023). Does Wolf Management in Latvia decrease livestock depredation? An analysis of available data. *Sustainability*, *15*(11), 8509. https://doi.org/10.3390/su15118509

Treves, A., & Bruskotter, J. T. (2014). Tolerance for predatory wildlife. *Science*, *344*(6183), 476–477. https://doi.org/10.1126/science.1252690

Treves, A., Bruskotter, J. T., & Elbroch, L. M. (2024). Evaluating fact claims accompanying policies to liberalize the killing of wolves. In G. Proulx (Ed.), *Wildlife Conservation & Management in the 21st century-issues, solutions, and new concepts* Chap. 6 (pp. 159–180). Alpha Wildlife Publications. https://faculty.nelson.wisc.edu/treves/pubs/Evaluate%20fact%20claims%20about%20killing%20wolves_2024.pdf accessed 2 August 2026

Treves, A., Chinn, S., & Krofel, M. (2026). Irreproducible research and a typology of replication efforts. *Research Integrity and Peer Review*, *1*, 19. https://doi.org/10.1186/s41073-026-00203-4

Treves, A., & Khorozyan, I. (2025). Robust inference and errors in studies of wildlife control. *Scientific Reports*, *15*, 33131. https://doi.org/10.1038/s41598-025-18497-7

Treves, A., Khorozyan, I., Krofel, M., López-Bao, J. V., & Proulx, G. (2026). Comment on Merz et al., 2025 corrected in 2026: Wolf-hunting in the USA does not seem to reduce domestic animal losses. *Science Advances* eLetter in review. https://faculty.nelson.wisc.edu/treves/

Treves, A., Krofel, M., Ohrens, O., & Van Eeden, L. M. (2019). Predator control needs a standard of unbiased randomized experiments with cross-over design. *Frontiers in Ecology and Evolution*, *7*, 402–413. https://doi.org/10.3389/fevo.2019.00462

Trouwborst, A. (2026). Favourable conservation status: EU court clarifies crucial yardstick for wildlife preservation and restoration in wave of wolf cases. *RECIEL*, *19*, 1–19. https://doi.org/10.1111/reel.70036

USFWS. (2020). Endangered and Threatened Wildlife and Plants; Removing the Gray Wolf (*Canis lupus*) From the List of Endangered and Threatened Wildlife (Final rule). Federal Register, 85(213), 69778–69895. https://www.govinfo.gov/content/pkg/FR-2020-11-03/pdf/2020-24171.pdf?utm_campaign=subscription+mailing+list&utm_source=federalregister.gov&utm_medium=email accessed 2 August 2026.

SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Treves, A., Ben-Ami, D., Cornman, A. M., Dul'a, M., Khorozyan, I., Krofel, M., Kutal, M., López-Bao, J. V., Nadler Valency, R., Preiss-Bloom, S., Santiago-Ávila, F. J., Šuba, J., & Žunna, A. (2026). Removing wolves did not reliably prevent domestic animal losses. *Conservation Science and Practice*, e70388. https://doi.org/10.1111/csp2.70388