Response to Roberts, Stenglein, Wydeven, and others

Adrian Treves[1]*, Suzanne W. Agan[2], Julia A. Langenberg[3], Jose V. Lopez-Bao[4], Naomi X. Louchouarn[1], Dave R. Parsons [5], Mark F. Rabenhorst[1], Francisco J. Santiago-Ávila[5]

[1] Carnivore Coexistence Lab, Nelson Institute for Environmental Studies, University of Wisconsin, Madison, WI 53706, USA, atreves@wisc.edu, ORCID ID 0000-0002-3052-4708, louchouarn@wisc.edu, mark.rabenhorst@gmail.com

[2] Department of Environmental Science, American Public University System, suzanneagan@gmail.com

[3] Langenberg Veterinary Services, W9365 State Rd 39, Mount Horeb WI 53572, USA, jlangenberg4@gmail.com

[4] Biodiversity Research Institute (CSIC), Oviedo University, Asturias, Spain, jv.lopezbao@gmail.com

[5] Project Coyote, P.O. Box 5007, Larkspur, CA 94977, USA & the Rewilding Institute, Albuquerque, NM, USA, dave@projectcoyote.org and fran@projectcoyote.org

*Corresponding author: Adrian Treves, 30A Science Hall. 550 North Park Street, University of Wisconsin, Madison, WI 53706, USA. Email: atreves@wisc.edu

Abstract

Human-caused mortality has been the major cause of death among wolves worldwide. In 2017, we summarized a 33-year dataset of >933 gray wolf deaths from Wisconsin, USA and estimated that poaching was the major source of mortality. Roberts et al. challenge our reinterpretation of data on causes of death and disappearances and urge us to use standard known-fates survival models rather than the combined time-to-event and total accounting methods we used. They do not cite subsequent time-to-event and competing risk and incidence models we published, raising an issue of selective citation of only their own work. Regarding reinterpretations, Roberts et al. neither present evidence for their claims nor revisit records of cause of death to argue their claims. Regarding traditional known-fate survival models, we review the violation of a critical assumption of such models. Namely, causes of death were not independent of censoring among Wisconsin collared wolves. Rates of disappearance approximating 42% of all collared animals are incompatible with the assumption that unknown-fate, collared wolves died of the same causes as known fate animals. We demonstrate that Roberts et al. made an erroneous claim that wolves frequently out-live the operational lives of their VHF collars. We present evidence of undisclosed competing interests among Roberts et al.'s co-authors. In scientific debates, the most transparent assumptions, methods, and data prevail, because outside reviewers can judge for themselves. We stand by the conclusions of our combined analyses 2017-2023.

Key words: fact by assertion, mortality, radio-collar, research integrity, time-to-event models, wolf

A. Summary of scientific debate and nature of the data for Wisconsin wolf mortality

Roberts et al. criticize our 7-year-old paper on gray wolf mortality in Wisconsin 1979-2012 (Treves et al. 2017a), challenging our reinterpretation of data on causes of death and disappearances. However, Roberts et al. neither present new data nor revisit specific records of cause of death. They do not engage with the high rate of errors we found in age estimation and low rate of questionable estimates of cause of death we meticulously described in the original (Sections B and C below). Given Roberts et al. and some of the co-authors persistently resist sharing data or specifying records of cause of death that deserve re-examination, we are not persuaded. We also point out a problem with information sharing as Roberts et al. refused to disclose potentially competing financial and non-financial interests (Section E below). Roberts et al. also urge us to use standard known-fates survival models rather than the combined time-to-event and total accounting methods we used in 2017. Mysteriously, they do not acknowledge subsequent time-to-event and competing risk and incidence models we published for Wisconsin wolves (Santiago-Ávila, Chappell and Treves 2020; Santiago-Ávila and Treves 2022) or three other populations (Louchouarn et al. 2021; Santiago-Ávila et al. 2022; Louchouarn 2023). We document several other instances of selective citation in Roberts et al., which feels to us like a manuscript that sat on a shelf for years and was not informed by our subsequent work.

While we agree that advanced survival models and hazard and competing risks models that treat disappearance as an endpoint are superior to our 2017 effort to combine simple time-to-event analysis with a total accounting method for wolves that died in Wisconsin, Roberts et al. make a blatant error by ignoring a feature of the dataset that violates a key assumption of traditional known-fate survival models (section D below). Namely, wolf disappearances they would censor are not independent of cause of death. In particular, cryptic poaching presents this problem as theft or destruction of transmitters or other concealment of poaching results in non-random censoring (Liberg et al. 2012). To dismiss this issue, Roberts et al. assert without evidence that we exaggerate cryptic poaching and that Wisconsin wolves “frequently out-live the operational life of their radio-collars” (line 3.58, p.3). We present evidence that renders that assertion untenable (Section D below). The bias Roberts et al. introduce is a negative bias for poaching and a positive bias for accurately recorded causes of death such as legal killing and vehicle collisions (Treves et al. 2016; Treves et al. 2017b). Before focusing where we agree or disagree, we summarize the relevant nature of the data, (which Roberts et al. omitted), because it is essential to understanding and resolving this scientific debate.

B. Nature of the Wisconsin wolf mortality data

The original sources of data for our paper are the same sources as cited by Roberts et al. with the possible differences of a handful of wolves included or excluded by either analysis (Stenglein et al. 2015). Namely, the original sources of information came from field agents of the Wisconsin Department of Natural Resources (WDNR) who collected or inspected >933 canid carcasses in the field. WDNR necropsied some of those carcasses (34.6%) and a subset of those were also radiographed (22.1% of 933 wolf carcasses). Therefore, causes of death for the majority were estimated without veterinary or pathologist input.

The methods used in the field to estimate cause of death systematically and categorically have never been described scientifically by (ex-)WDNR authors, many of whom are also co-authors in Roberts et al. Many different agents worked from 1979–2012 with carcasses in variable states of decomposition, adding subjectivity about causes of deaths. Consequently, most of the original mortality causes are not subject to re-analysis and the field-estimation methods cannot be reproduced. Therefore, all subsequent peer-reviewed analyses of wolf mortality are secondary sources that interpret the primary field data.

As with Stenglein et al. (2015), we attempted an objective, collaborative, standardized estimation from primary sources. Our study was also funded by the WDNR and US Fish & Wildlife Service (MSN136619 and MSN146937). However, our study stands out as more transparent by presenting line by line each wolf carcass and its interpretation for cause of death (detailed in our original paper in SD 1-3 and permanently archived here https://faculty.nelson.wisc.edu/treves/data_archives/Treves_etal_2017_with_SuppInfo.zip accessed 7 January 2024). It also stands out because we found and corrected errors in field estimates, which Roberts et al. do not address or refute, as below.

Roberts et al. misstate the veterinary record. JAL, our veterinary expert conducted many of the necropsies and interpreted radiographs for the WDNR for many years of the study period and also co-authored (Stenglein et al. 2015), JAL played a clear, explicit role: "JAL reviewed necropsy data with ..help…[from] K. Miller, N. Thomas, and B. Richards of the United States Geological Survey and National Wildlife Health Center for access to and review of pathology data." (p.30 and SD4A in Treves et al. 2017a). JAL was one of the original veterinarians who conducted necropsies and JAL reviewed a number of reinterpreted or reaffirmed necropsy reports before our analysis. For one example, JAL found scientific reasons to reinterpret cause of death to poaching for wolf #WI-2007-077," (p.25 and SD4A in Treves et al. 2017a). JAL's concerns about WDNR mortality reports at the time were not always heeded. Roberts et al. do not cite a single case to rebut one of our reinterpretations, relying instead on plausible hypotheticals.

Also, we found what we interpret as errors in age estimation in 13% of records, e.g., carcasses aged as pups but reported dead between November and April, a period when pups born the previous spring would be near adult size and therefore should have been estimated at an older age (Fuller 1989). No pup <7 months old has been recorded during that period in Wisconsin to our knowledge. Roberts et al. do not mention this rather sizable, apparent error we documented in official records. Further, 15% of the records had missing or ambiguous data fields and "a notable proportion did not account for necropsy or radiography data properly. For an example of the latter, our nonrandom subset of necropsies and radiographs indicated that 6% of nonhuman deaths and 37% of collision deaths included perimortem or premortem gunshot that was not a result of legal killing, and 16% of the cases we reevaluated in detail were found to be unreported poaching." (p.27, Treves et al. 2017a). Even considering the above reinterpretations and estimated error rates, the data we published (Treves et al. 2017a) and those used by Stenglein et al. (2015, 2018) are largely the same because of the large sample that were not re-interpreted by either set of authors. The reinterpretations of cause of death that we published constitute approximately 2% of the entire sample of ~933 carcasses with cause of death, see SD1-3 for specifying each reinterpretation published (Treves et al. 2017a). It's unclear if that amounts to much difference in outcomes. Furthermore, we have published the data and the original population reports by the WDNR (https://faculty.nelson.wisc.edu/treves/data_archives/ accessed 8 January 2024), which make our work reproducible. By contrast, the WDNR took these population reports down from its public-facing webpages and never posted mortality data. Likewise, Roberts et al.'s co-authors working for the WDNR could have presented those reports to their readers as a webpage but chose not to do so without explanation or citation to our webpages. Therefore, we stand by our transparent, line-by-line reports on each wolf carcass and the associated re-interpretations. Roberts et al.'s allusions to their "experience" with data are simply claims of authority, not a substitute for evidence or for and reproducible methods.

Our 2017 re-interpretation of the original estimates should come as no surprise to anyone working with long-term field data collected by dozens of diverse observers whose methods may have been buried over time and idiosyncratic. Skepticism about field estimations of age and cause of death are not only reasonable but should be aired to raise confidence in published results. We note a tendency to ignore or dismiss skepticism of WDNR methods or results germane to wolf monitoring, whether it is the independence of census methods from estimates of reproductive output first questioned by APW and AT in (Wydeven et al. 2004) or questions about the uncertainty around wolf abundance estimates at every scale of analysis from individual census-takers (within- and between-individual variability in wolf counts within survey blocks in the same year) to state-wide estimation techniques (Treves et al. 2021; Treves and Santiago-Ávila 2023). Roberts et al.'s co-authors have routinely ignored those and other peer-reviewed, published requests for transparency or explanations about data, methods, or even figures in Results (Chapron and Treves 2017; Santiago-Ávila, Chappell and Treves 2020). Dismissal of skepticism by Roberts et al. arose again in the current context. Specifically, our 2017 paper with its cautions and reinterpretations of the cause of death and age estimation in the field did not prompt Roberts et al.'s co-authors to a cautious reconsideration of each dead wolf record by record, but rather a wholesale assumption that all the records were correct as originally reported (Stenglein, Wydeven and Van Deelen 2018); i.e., their critique does not include a re-analysis of the data in question. We infer an undisclosed value judgment held communally among Roberts et al.'s co-authors that the unpublished mortality data have always been accurate and precise. A similar assertion of authority without evidence leads Roberts et al. to claim "familiarity" and "experience" before they make an unsupported and extreme claim about radio-collared wolves that were lost to monitoring (LTM) as we describe next.

C. LTM and the limits of traditional time-to-event analyses

Roberts et al. down-play cryptic poaching by emphasizing "…long-range dispersal [sic] and collar failure. Experience with long-term monitoring of wolves suggests that both of these alternative outcomes also occur frequently”. (line 2.80-2.81, p.2). The error about long-range dispersal is explained below. They also write that marked animals “frequently out-live the operational life of their radio-collars” (line 3.58, p.3). Again, they present no evidence. While it is not for us to explain what they mean by “frequently” in either quotation, we find their claim is not credible, so we will consider what “frequently” and “also” might mean, below.

Given we repeatedly mentioned that some LTM wolves died unmonitored without succumbing to cryptic poaching and some migrated out-of-state despite active collars, Roberts et al. do not mean that wolves with VHF collars *occasionally* eluded monitoring for innocuous reasons of out-of-state migration or transmitter failure because that would agree with us. Nor do we think they mean that migration out-of-state and collar failure together exceed 51% of LTM because the separate use of frequently for either case would make the sum of LTM exceed 100%. Therefore, we infer they mean migration out-of-state and collar failure exceed our estimates and cryptic poaching is therefore lower than we estimated. We addressed this possibility in the same journal in a second paper published in 2017 that Roberts et al. did not cite (Treves et al. 2017b) and in our first commentary on the topic (Treves et al. 2016). Nor did Roberts et al. summarize our meticulous enumerations of what was known about migration out-of-state among Wisconsin wolves in Treves et al. (2017a). Because their summary of the data and their summary of our work was incomplete, we review the issue of migration out-of-state and collar failures below.

First, any analyst of Wisconsin wolf radio-collar data must grapple with 236-238 LTM VHF collars (54.7-55.2% of the total 431 collared hereafter 55%), which consisted of 180-182 LTM never recovered and 56 collared wolves that were found dead without the benefits of radio-telemetry (11% of all collared wolves who were LTM for a finite period, Treves et al. 2017a). To understand Roberts et al.’s claim of “frequently”, we have to consider all of the 55% above.

Regarding migration, we have to clarify that only migration out of state can explain LTM in Wisconsin's aerial telemetry program — not dispersal necessarily as Roberts wrote — and that migration must have taken the wolves out of aerial telemetry monitoring range by WDNR and cooperating neighboring states — not simply long-range movements as Roberts et al. wrote. Long-range but in-state movements were documented (Treves et al. 2009) and did not necessarily result in LTM. Out-of-state migration by Wisconsin wolves being monitored provide insights: "Eight radiocollared Wisconsin wolves died in Michigan out of a total of 264 with known fates (3%; Supplementary Data SD1B and SD2). … Another 19 radiocollared Wisconsin wolves died in Minnesota (7% calculated as above)…" (Treves et al. 2017a:24). Our quoted text suggests a point estimate of 10% out-of-state migrants with known fates among Wisconsin collared wolves. Moreover, independently written and peer-reviewed research found wolf migration out of established wolf pack ranges into non-wolf-ranges followed by death are rare events which might conceivably have been overlooked during monitoring concentrated on the established range (Agan, Treves and Willey 2021; Louchouarn et al. 2021).

If we apply the 10% value for monitored wolves above to the unmonitored LTM also, we should expect approximately 24 of 236–238 LTM wolves also migrated out-of-state but died without collar recovery whatever their fate. Not assuming that some of these too would have been poached and not reported (thus our interpretation is conservative here), we are still left with ~212 LTM wolves in-state to consider (56 of which were found by other means, as above). Hence 156 LTM wolves that died in-state were never recovered after we estimate the migrant portion.

Second, we consider systematic studies of rates of VHF collar failure. Habib et al. (2014) reviewed the performance of VHF transmitters from animal telemetry studies in India. They inferred the common manufacturers used in the USA, "…Advanced Telemetry Systems, Wildlife Materials and Telonics were comparatively reliable, with success rates of 100%, 96% and 86%, respectively." (Habib et al. 2014), p.4. We accept the conservative value of 86% because it is also consistent with Table 8 in Habib et al. (2014) enumerating performance of 195 individual VHF transmitters (not implants or backpacks), in which 27 experienced "battery drained" (14%). Habib et al. blamed that high rate on the heat in Indian field telemetry studies, which differ from Wisconsin conditions. Using 14% for the 431 wolves ever radio-collared in our sample, one might expect 61 LTM without human interference. This seems low as 56 were recovered without the benefits of telemetry (some of which might have been tampered with by humans). Focus on nonhuman causes of death illuminates the issue further.

Nonhuman causes of death among LTM can be estimated accurately from nonhuman causes of death among known-fate collared wolves because humans were not involved in either the death or any tampering with transmitters. Therefore, for nonhuman causes of death (only), conditions for LTM fulfill the assumption of traditional known-fate survival models. Known-fate survival analyses estimated "Natural" causes of death for Wisconsin at 27% of all collared, dead wolves (Table 2 in Stenglein et al. 2015) or 29% for nonhuman causes among collared wolves (Treves et al. 2017a, Table 3). Although these are imprecise point estimates, the component relating to nonhuman causes of death are unbiased. As noted above, 56 LTM were recovered, of which 10 (18%) were nonhuman causes of death (Treves et al. 2017a, Table 4). Using 27-29% as the expected risk of nonhuman deaths, we expect 121 wolves died of nonhuman causes across the entire collared sample. Ten of these were recovered in the LTM sample, 79 were known-fate during monitoring, which left 32 LTM that were never recovered. That leaves the remainder of approximately 114 LTM wolves (26.5% of the total) that cannot be accounted for by migration out-of-state, collar failure or nonhuman death without recovery.

Some readers may wonder about vehicle collisions and legal killing (hundreds of wolves were killed annually in Minnesota each year of our study (Fritts et al. 1992) and Wisconsin killed a lesser number in some years of the study). These are causes of death that tend to be perfectly or almost perfectly reported that cannot have contributed many, if any, to the disappearances of collared wolves because the collars bore state identifiers and neighboring states were in close communication and collaborations (Beyer et al. 2009; Wydeven, Van Deelen and Heske 2009). Some drivers may have left road-killed, collared wolves behind and no subsequent motorist found and reported them but there are no data to support any estimate of this let alone a large number of such deaths. Instead, we consider the final piece of evidence from the time-to-death or time-to-disappearance analyses we ran in 2017.

Using time-to-event analysis, we compared time-to-death or disappearance for radio-collared wolves to reveal significant differences between time-to-endpoints: “the number of days between radiocollaring and known fate (means for legal causes 461±612 days, nonhuman 685±723, collisions 778±832, poached 558±539, and disappearances 529±762).” (Treves et al. 2017a, p.27). That was our prima facie evidence that disappearances resembled poaching and differed from legal causes, nonhuman, and vehicle collisions. Also, these data undermine the claim Roberts et al. made about wolves outliving the operational life of VHF collars. We counter wolves rarely did, although all of us lack a Wisconsin-specific measure of radio-collar operational life when poachers do not steal or destroy the transmitters. But an even more important point about time-to-event survival models surfaces from those data.

The similar time-to-poaching and time-to-disappearance demanded explanation and undermines a critical assumption of known-fate survival analyses, namely that censored animals are a random subset of all marked animals. Clearly LTM represented by time-to-disappearance above are not the most variable subset (collisions are) nor do they occur late in the time series (nonhuman and collision deaths do). Roberts et al. fail to mention this undisputed observation. Their failure to mention it to readers is misleading.

The critical assumption of known-fate survival models becomes increasingly unreliable as larger proportions of marked animals are censored by a subset of causes of death. Cryptic poaching is the simplest explanation and the above estimate of 26.5% adds to reported poaching to make total poaching the major cause of death in this population. In sum, another technique and dataset were needed to illuminate the population-wide mortality patterns and overcome bias in the radio-collar data. Sole reliance on time-to-event analysis for recovered radio-collars would be unwise because it depends on several assumptions that are not well supported in this dataset. First, collared wolves were not a random sample of the population at the time of collaring. Therefore, simple extrapolation to the population from collared wolves was unwise. We explained, "Had we estimated mortality patterns and rates from the actively monitored subset only, we would have estimated relative risk with a bias for older, female, territorial residents in core counties, which suffer differential hazard rates in other regions (Schmidt et al. 2015) and a bias against poached wolves whose transmitters were inactivated…[citing elsewhere in our article the source of that insight as (Liberg et al. 2012)]… An alternative approach is to estimate the number of missing wolves and then reconstruct their fates, a total accounting approach." (SD3, p.1 in Treves et al. 2017a). Roberts at al. also dismiss and a mischaracterize our work, when they write, "…reliance on naïve formulae to estimate mortality rates instead of conducting proper time-to-event survival analysis" (p.1 line 1.87). This is misleading because 'instead' implies we did not run proper time-to-event analyses but in fact we did (above). Then we realized the violation of the critical assumption demanded a supplemental approach we called total accounting, following work in other subfields. Our work owed much to an early insight by Liberg et al. (2012). The approach we followed is not common and clearly not the one preferred by Roberts et al., but, far from "naïve", it seemed both complementary and necessary given the assumption violations of traditional time-to-event models present in the data.

Also, Stenglein et al. (2018) attempted to correct the error in Stenglein et al. (2015) and credit us somewhat obliquely for the insight. It appears to us that Roberts et al. disparage our work despite having benefited from it. Moreover, we subsequently published more sophisticated time-to-event analyses than recommended by Roberts et al. all the while explicitly correcting for disappearance as a separate endpoint (Santiago-Ávila et al. 2020; Santiago-Ávila and Treves 2022). The inferences that rates of disappearances are not random but predictable from human behavior have been replicated repeatedly for independently peer-reviewed papers on various populations by different lead authors, see Mexican gray wolves (Louchouarn et al. 2021); red wolves (Agan, Treves and Willey 2021) (Santiago-Ávila et al. 2022); and Michigan gray wolves (Louchouarn 2023). These studies agree that periodic changes in policy correlate strongly to changes in the rate of disappearances in six states, which suggest collar failure is a relatively infrequent cause of the high rates of wolf disappearances. In the intensively monitored and geographically restricted red wolf and Mexican gray wolf populations, the relative risk of LTM was 23% (n=508) and almost 30% (n=223) respectively, whereas the Michigan gray wolf population that was monitored less frequently and at similar rates as Wisconsin (Beyer et al. 2009) had an LTM rate of 41% (n=487) analyzed in (Louchouarn 2023). Therefore, Wisconsin's

LTM rate is explainable in part by infrequent monitoring. Multiple studies corroborate the notion that cryptic poaching is the major source of LTM wolves.

The above studies were not cited by Roberts et al. despite their direct relevance; but they cite Chakrabarti et al. (2022) edited by a Roberts et al. co-author (TvD) and peer-reviewed by another co-author (JLS), but fail to cite the reanalysis of Chakrabarti by Oliynyk (Oliynyk 2023), who contradicted the main finding relating to poaching. Roberts et al.'s pattern of citing work they agree with only and dismissing or ignoring work contrary to their favored studies epitomizes selective citation, a practice described by the National Academies of Science Engineering and Medicine as a breach of research integrity (NAS National Academies of Sciences 2017) "…careless or negligent crediting of prior work violates the value of fairness" (NAS 2017:36). That and their ad hominem use of "naïve" speaks to a loss of objectivity.

Regarding our total accounting effort, Roberts et al. criticize our estimate of uncollared wolf mortality. We discussed the challenges in 2017, provided wide bounds of uncertainty from long-run probabilistic simulations, and expressed caution with a range of values for uncollared wolf mortality. By contrast, Roberts et al. did not acknowledge any uncertainty when citing a point estimate from (Stenglein et al. 2015), which "… concluded that at most, mortality rates on uncollared wolves exceeded that of collared wolves by 4.2%." (line 3.70, p.3 Roberts et al.). In reality, Stenglein et al. (2015) noted the SD was 2.2%, so Roberts et al.'s "at most" in the preceding quotation is inaccurate to the original. Moreover, we disagree with Stenglein et al.'s low estimate. Other studies support our skepticism. Among Alaskan gray wolves unmarked individuals had approximately 13% higher mortality rate than marked wolves (Schmidt et al. 2015). Also, Milleret et al. (2021) reported positive bias in survival of collared carnivores.

Relatedly, Roberts et al. criticized our use of population estimates for total accounting. That critique seems hypocritical, given Stenglein et al. (2015) used the same input data and wrote "The necessary data are a multi-year dataset with collared and non-collared carcass data with various causes of mortality, an annual estimate of the population size, and the annual number of radio-collared animals in the population…" p.1177. Although Stenglein et al. (2018) revised time-to-event analyses with more careful accounting for disappearances than in 2015, they did not re-evaluate their estimate of uncollared mortality rate. Instead, they estimated the annual rates for 'unknown censored' (21.8%, SD=2.1) and 'known censored' (5.2%, SD=1.2) and noted that the hazard of collar loss was highest in February and again in November (Stenglein et al. 2018). While the estimates are low in our view and handling of unknown causes of death was not informed by our work, their result partially corroborates our estimate of LTM cryptic poaching here and our prediction of highest rates of illegal killing when many deer hunters are afield in November. Both the November and February peaks were replicated by our later survival analyses showing high hazard of collar loss in November and in snow-cover months with hound training (Santiago-Ávila and Treves 2022). We find more in common with Stenglein's work than Roberts et al. seem inclined to admit.

E. Transparency

Overall, we encourage scientific debates be published in reputable journals. It is essential that the public gain confidence in science by seeing how disagreements are aired constructively, new evidence presented, errors corrected, and scientific consensus built over time. We appreciate that Roberts et al. have taken their disagreement to the pages of the original journal. We anticipate a healthier debate with the WDNR keeping an open mind about the science rather than predetermined policy preferences. When WDNR policy is based on contested claims, the best remedy is sunlight. However, we see several features of the Roberts et al. critique that do not live up to the ideal.

Disappointingly, Roberts et al. do not adequately substantiate numerous statements and make misleading ones, such as, "…we have serious reservations about the approach used to reclassify wolf mortalities *after the fact and without context*." (Line 1.43-1.45, p.1) emphasis added. This is misleading because all mortality studies were after the fact, as we explained above, and we provided much more detailed context than any other analysis of Wisconsin wolf mortality (SD1-3, Treves et al. 2017a). Indeed, since 2017 we and other lead authors have asked for transparency from the WDNR and the co-authors of Roberts et al. Our peer-reviewed requests and comments in scientific journals have addressed non-transparency about data and analyses (Chapron and Treves 2017; Santiago-Ávila et al. 2020; Treves et al. 2021, 2022; Treves and Louchouarn 2022a,b). We have also expressed concerns about non-disclosures of competing interests by co-authors of Roberts et al. (https://journals.plos.org/plosone/article/comment?id=10.1371/annotation/4d92a9da-dc73-41bb-ad83-837ed707c948 accessed 11 January 2024). Here we detail those concerns because thorough transparency about potentially competing interests is essential to reliable research and fair scientific debate (NAS 2017).

The journal policy states, “To enable our editors, peer reviewers, and readers to assess professional credentials of authors, as well as any potential biases, we ask that authors disclose all information about their employment affiliations and any financial interests relevant to the work that the author has submitted for publication in JM.” https://academic.oup.com/jmammal/pages/general_instructions#Manuscript%20Preparation accessed 8 August 2023). We also quote Roberts et al., "The authors declare no conflict of interests. Some have argued that agency affiliation is a conflict of interest. Such insinuations are without evidence and set a dangerous precedent that potentially undermines or silences agency scientists altogether." There are three problems with this statement in addition to non-disclosure of financial interests and non-disclosure of competing non-financial interests, all of which should concern readers as follows.

First, disclosure is not silencing; it is the opposite. Throughout this section, we explain why disclosure is airing and giving voice to the influences on assumptions, methods, and interpretations. Second, some of the co-authors of Roberts et al. have financial and non-financial interests in wildlife policy (documented in Supplementary Data SD1) and all of them have a connection to the WDNR, an agency with financial and non-financial interests in policy as we explain in the next paragraph. We shared these with the editors of this journal and they invited us to post the evidence in Supplementary Data SD1, rather than negotiate further with Roberts et al. Third, Roberts et al. confuse insinuate with state plainly. 'insinuate' means "To convey (a statement or notion) by indirect suggestion; to hint obliquely: now generally with implication of cunning or underhand action." https://www.oed.com/search/dictionary/?scope=Entries&q=insinuate, accessed 7 January 2024). Asking for transparency cannot undermine them unless they are concealing.

As to agency bias, just as everyone has personal bias, so too do organizations as research has revealed. There is substantial evidence that agency staff are not impartial or unbiased in their policy and management preferences (Koval and Mertig 2004; Karns et al. 2018; Manfredo et al. 2021). Additionally, co-authors employed by the Wisconsin DNR may be faced with pressure or even the approval of political appointees in the agency who also control career advancement and salary adjustments. A full and transparent disclosure of potentially competing interests would include a statement of whether the WDNR requires approval of manuscripts, whether such officials can or did alter wording, and which officials (by title not name) had to approve this submission, if any. Without clear protections for internal dissent or whistle-blowing, the undue political influence on science from agencies could be no different from the pressures on industry scientists. We emphasize “could” because without disclosure of WDNR processes relating to manuscripts, the reader cannot know if Roberts et al. enjoy freedoms similar to academics. By not disclosing the process this manuscript underwent at WDNR, readers are free to assume rightly or wrongly that their assumptions, methods, analyses, or interpretations are colored by partisan politics or personal career advancement. Moreover, WDNR has an explicit policy position that not only sanctions (mandated by law), but promotes the hunting/trapping of wolves, and, similar to a private firm, receives funding from the sale of such hunting/trapping licenses. Contrary to Roberts et al.’s statement, the above seems sufficient evidence that agencies and their scientists can feel bias from particular policy positions and monetary interests. It would be an unjust special privilege for the journal to allow agencies and their scientists to not have to explicitly disclose such clear conflicts of interest, while requiring academic, advocacy and industry scientists to do so.

Regardless of how one feels about agency staff, Roberts et al. co-authors omitted non-agency competing interests documented in SD1. At least one co-author of Roberts et al. received financial support from organizations other than their declared institutional affiliations (e.g., Au Sable Foundation). Three at least serve as representatives of Wisconsin Green Fire (WGF), provided an affidavit for litigation about wolves, or sat on WDNR advisory boards on wolf policy with their affiliation listed as WGF or Timber Wolf Alliance. All these organizations and other probable affiliations (e.g., The Wildlife Society) advocate positions on wolves and other wildlife. We share the evidence for readers to see that the acknowledgments statement made by Roberts et al. is inaccurate (SD1).

Affiliations are not problematic per se (everyone has one or more), but failure to disclose them is questionable as a breach of research integrity (NAS 2017). Non-disclosure of these interests robs readers of the ability to gauge bias. A culture of transparency has spread throughout the modern scientific community because it makes science more trustworthy and reproducible.

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Conflict of interest

We declare no conflicts of interest. However, as full transparency, we provide 'statements of competing interests' here.

All funding awarded to AT in the last 10 years as of January 2024 are listed here http://faculty.nelson.wisc.edu/treves/archive_BAS/funding.pdf, and a full CV here http://faculty.nelson.wisc.edu/treves/archive_BAS/Treves_vita_latest.pdf. SWA is a member of the USFWS Red Wolf Recovery Team. In 2020, the Carnivore Coexistence Lab at the University of Wisconsin Madison contributed to her dissertation publication fees. The Center for Biological Diversity supported SWA's research on attitudes to red wolves. The USFWS

shared data for her dissertation. JAL retired from the Wisconsin Department of Natural Resources and runs a private veterinary clinic. JVLB is affiliated to the Biodiversity Research Institute (CSIC), Oviedo University, Principality of Asturias, Spain, the IUCN/SSC Canid Specialist Group, Iberian Wolf Research Team, Claws & Laws, the Spanish Action Plan against illegal trafficking and international poaching of wild species. NXL's CV is available at https://faculty.nelson.wisc.edu/treves/archive_BAS/Louchouarn_CV_2024.pdf and currently works as a post-doctoral researcher and lecturer for the University of Wisconsin- Madison and as a river restoration project manager for the Grand Traverse Band of Ottawa and Chippewa Indians. Before that, she conducted her doctoral research on carnivores and people at UW-Madison, partly funded by the Animal Welfare Institute, the Natural Science and Engineering Research Council (NSERC) of Canada, The Yellowstone to Yukon Conservation Initiative (Y2Y), National Geographic Society Explorer program, and the UCLA Animal Law & Policy Grant Program. DRP perceives no potential competing interests. DRP is an independent pro-bono Conservation Biologist, retired from the U.S. Fish and Wildlife Service (1999), and serves as a Science Advisor to Project Coyote and the Carnivore Conservation Biologist for The Rewilding Institute. DRP is a member and/or financial contributor to the following non-profit organizations: Public Employees for Environmental Responsibility; Project Coyote; The Rewilding Institute; NM Wild; Cornell Laboratory of Ornithology; LifeNet Nature; Rio Grande Chapter of Sierra Club; Lobos of the Southwest; Friends of the Candelaria Nature Preserve; National Public Radio; Center for Biological Diversity; Wild Earth Guardians; and NM Conservation Voters. DRP is a plaintiff on litigation filed against the US Fish and Wildlife Service's Mexican Wolf Recovery Plan by Earth Justice. DRP receives no monetary compensation from any of these organizations. MFR conducted research in 2014 funded by

SESYNC (National Socio-Environmental Synthesis Center, from a National Science Foundation pass-through grant, worked for International Mapping (2011-2015), DigitalGlobe now called Maxar Technologies (2015-present), and RoyceGeo (2022). MFR declares a personal relationship to a principal of Colorado Wolf Adventures but has not affiliated with the company. FJS-A's CV is at https://faculty.nelson.wisc.edu/treves/archive_BAS/Santiago-Avila_CV_2023.pdf and currently works for Project Coyote. FJSA has and continues to serve as a Fellow and board member for PAN Works and provide expert witness for litigation in various cases regarding wolf policy. FSA is Science & Ethics Manager for Project Coyote, a national (US-based) non-profit organization whose mission is to promote compassionate conservation and coexistence between people and wildlife through education, science and advocacy, and which has recently engaged in litigation against the state's (WI) policies towards wolves (2021).

Author contributions

All authors contributed in part or equally to methods, data collection, analysis, and interpretation of articles that include them and were cited here. AT drafted and submitted the manuscript. All authors provided feedback and proof-reading of the final submission.

Supplementary Data

Supplementary Data available at *Journal of Mammalogy* online.

Supplementary Data SD1.— Non-disclosure of potentially competing interests in Roberts et al. 2024.

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