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Dear editors of PLoS One and PLoS ethics staff,

We sent a letter of concern on 28 March 2024 to the editor and ethics team at PLoS. We hope the issues we raise will be addressed before this pair of articles is finalized for publication. We are concerned about several apparent breaches of PLoS policy by Stauffer et al.

- undisclosed competing interests
- unshared data
- unprofessional defamatory language
- a double standard applied to us including the extremely short (10 day) turn-around time to respond when Stauffer et al. had two rounds of revision yet failed to disclose competing interests at all until we expressed concerns a second time (the first time was in our peer review of Stauffer et al. in Fall 2023).

We request a response to these concerns before Stauffer et al.'s manuscript goes live.

This rebuttal to Stauffer et al. is original and submitted nowhere else.

We included two supplementary Materials sections in the main text and a related manuscript under review as a separate supplemental file.

With respect and deep. Concern from an author and reviewer who has been a proud constituent of PLoS journals,

A. Treves, PhD

Reply to Stauffer et al.: Uncertainty and precaution in hunting wolves twice in a year: reanalysis of Treves and Louchouarn

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In TL 2022 and. our comments on it [1, 2], we attempted to fill a gap in information during a rushed policy process in Wisconsin 1 March–31 October 2021 that resulted in the implementation of a second public hunt with shooting, trapping, and hounding of wolves (wolf-hunt hereafter) in one year. Although a state court order ended that planned wolf-hunt, our article remained relevant because we had modeled the scenario of a zero-quota wolf-hunt to predict the state wolf population in April 2022. We used peer-reviewed data to simulate bounds of uncertainty about unmeasured or highly uncertain estimates of reproduction and survival to estimate a one-year change in wolf abundance. We began with a peer-reviewed estimate of the Wisconsin wolf population size in April 2021 [3] and estimated a one-step change by April 2022. We used the precautionary principle and legal thresholds as explicit value judgments to estimate the probabilities of crossing bounds set by society.

[4] presented a sweeping and acid critique, most of which we find unpersuasive for reasons detailed below. However, we seriously considered their concern about an arithmetic error and a claim about an inaccurate parameter value relating to pup production. We explored both, found one wanting evidence [2], which we detail below. For the other, we agree that [4] found an arithmetic error, which we repaired [5]. Although final values changed by 4% when corrected, our main conclusion did not change because the probability of crossing legal thresholds was two to three times greater in magnitude [5].

Scientific debates demand transparency

[4] did not present any data, nor did they provide a scientific description of field data collection on which their claims are based. Instead, they cite one source 14 times [6], written by their co-authors in a book edited by their co-authors. That chapter lacks scientific description of methods, presents only summary data on births, deaths and population abundances and only up until the year 2007. We pointed out those shortcomings in [6] for years, both in comments on their articles in PLoS One [7] and in separate articles since 2004 [8, 9]. The 2004 paper was led by APW -- a co-author in [4] and AT (lead author here) yet they ignore these and other doubts raised about [6] and other state of Wisconsin estimates of wolf life history rates. We detail below how that work

has too many shortcomings to support the sweeping claims made in [4]. Open debate with Stauffer and co-authors has been, and continues to be in [4], hampered by their resistance to sharing data or subjecting their own methods to independent, anonymous peer review as we have done with ours. Non-transparency in [4] begins with their incomplete disclosures of competing interests.

Non-disclosure and non-transparency

Before discussing the science behind this debate, we find that [4] did not fully disclose competing interests, when they wrote "no consequent competing interests exist. The analyses and conclusions presented here are those of the authors alone, and are not influenced by, nor represent official policy of any of the institutions or agencies with which the authors are affiliated." [4]; full text in SM1.

However, that disclosure fails on several grounds. Their statements in another journal directly contradict that claim. In [10] including most of the co-authors of [4] wrote, "For purposes of full disclosure, the authors provide the following additional affiliations that are not listed in the author bylines: … ERO and APW are advisory board members for the Timber Wolf Alliance of the Sigurd Olson Environmental Institute at Northland College; ERO, APW, and TRV are scientific advisory board members for Wisconsin's Green Fire." Even that disclosure is incomplete regrettably, as we substantiate in SM1. We find an appearance of five categories of additional competing interests, both financial and non-financial in [4]. Furthermore, the disclosure accompanying [4] above neglects to mention if their submissions to journals had to pass agency approval and by whom.

Nor was their non-disclosure an innocent, unintentional omission because AT reviewed the second revision of [4]. in Fall of 2023 and pointed out the omissions to the editors. Revisions 2-3 of [4] bluntly claimed "no competing interests". The current disclosure, incomplete as it is (above), was sent to us after we were told that [4] had been accepted. Now, we ask if peer reviewers at any stage of the three reviews were fully informed?

Disclosures are important for reviewers and readers to be aware of the potential for financial and non-financial interests to have influenced the approach, tone, and interpretations of data in [4]. Moreover, [4] criticized the peer reviewers of our paper [1] as biased. Here we turn the mirror on [4], for failing to advise their peer reviewers, failing to share data, and unfairly summarizing the state of scientific debate.

Scientific debate

Contrary to the claim ins [4] that we, "…wrongly halved the number of pups that survived to November…" and that we were, "…counting harvested wolves twice among the dead", we did neither. This might be a simple misunderstanding in [4], but as they did not quote us, we cannot determine where the confusion arose.

Abundance estimates and handling of February 2021 wolf-hunt mortality: First, [4] did not present a peer-reviewed estimate of the wolf population in April 2021 contrary to their claim. Namely, the state estimate of the Wisconsin wolf population in 2021 bears an unclear relationship to Stauffer's method [11]. In particular, Stauffer's scaled-occupancy model [11] was

not validated for years with wolf-hunts [12]. Therefore, its application to 2021 should be validated properly. Ignoring an ongoing debate, [4] does not mention that [11] has been questioned for half a dozen shortcomings that lead to inaccuracy, imprecision, insensitivity to changing conditions, irreproducibility (not explaining several steps in methods), and not sharing data [12]. Regardless, we did not double-count wolves killed legally in February 2021. We surmise the confusion on their part came from this passage,

> "The state's justification for interrupting the new census method before 14 April 2021, when it would have been terminated as in previous years…, was that the wolf-hunt of 22–24 February made accurate and precise data collection impossible. Therefore, the wolf population estimate derived from the new census method in 2021 lacked non-hunt mortality from 25 February to 14 April 2021, which is a season of high mortality from winter conditions and illegal killing historically.… We are not aware of any effort to correct the new census method estimate, therefore it seems to be a systematic over-estimate of N_{2021}. Furthermore, the state did not provide bounds on N_{2021} but given the reported value (1195) of N_{2021} equaled the central tendency of N_{2020} (also 1195), we assume here the same bounds minus the 218 wolves killed legally in the February wolf-hunt, hence 977 (739–1355)." citations omitted, [1].

[4] misunderstood that we had deducted February 2021 wolf-hunt mortality from both population estimates (traditional and new scaled-occupancy-model approach). We did not. We deducted those only from the new occupancy estimate.

We found no evidence that the new occupancy estimate accounted for wolf-hunt mortality. If the state did so, [4] could clarify the record for us and the public rather than accusing us of double-counting. However, we doubt the state did so. Given the wolf census of 2021 ended prematurely on the day the wolf-hunt began, the state estimate of the wolf population could not have included data during and after the wolf-hunt and therefore seems to give nonzero probabilities of occupancy by dead wolves across much of the state. The state implementation does not include a term for deduction of wolf deaths during the census period and explicitly risks counting dead wolves by using previous years of data on wolf census [12]. Therefore, the burden seems to us to fall on [4], to show that the state deducted dead wolves, how they did so while still using the Stauffer model, and with what modifications.

Births and Pup survival: Contrary to a claim in [4], we did not double-count pup mortality either. As far as we can tell, they simply misunderstood Eq.3 to represent the first half of the year when it actually represented the second half of the wolf-year. Only the second half of the wolf-year exposed pups to adult mortality hazards. For hazard from birth to November, we had already taken into account pup mortality, using data from [13].

The debate over [13] has been addressed at some length in [2]. [4] claims [6] – a late winter observational estimate of pup production - provides a better estimate of the number of pups reaching independence than a mark-recapture study of pups in [13]. We have no competing interest in that debate between former Wisconsin DNR staff and current ones, but the history of questions raised about [6] bear repeating.

In 2004, [8] by APW and AT, considered a problem with the field methods used for estimating pup production or survival. For the first time, [8] noted that estimates of pup production were

statistically non-independent of pack size estimates collected at the same time. Namely, estimates of young of the year and pack size were made from aerial sighting of on average 13% of wolf packs [6], a few summertime howl surveys, and snow tracking in most other packs , [8]. Such methods are generally considered imprecise and inaccurate compared to mark-recapture studies like that of [13]. Also, because migration into and out of packs is only crudely understood in this population [14], pack size cannot be used to infer pup survival (or migration) without circular reasoning. Moreover, field-based estimates of age by size of wolves has a high error rate [15]; therefore, aerial sighting and especially snow track surveys include a large but unmeasured uncertainty about pup survival into late winter. Furthermore, experimental tests of accuracy of experts estimating pup numbers during summertime howl surveys suggest high rates of imprecision [16]. The method for counting wolf pups in Wisconsin has not been subjected to an experimental test with known pack sizes. Instead of acknowledging these shortcomings of [6], [4] bluntly assert their correctness. Moreover, [6] and [13] were both published in chapters of the same book edited by two of Stauffer's co-authors. Yet, no scientific corrections, no description of methods, and no data have been shared to support the claims in [4].

Nor did APW and TVD publish their evidence that counts of wolves in the same census block by different types of census-takers (agency staff versus community volunteers) generated significantly different estimates of the number of wolves present [17]. We called that out in 2021 [9]. Despite the weakness of their methods for estimating pack size and pup survival independently, [4] make bald assertions of fact such as this one, "Moreover, the differences in mortality rates postulated in Treves et al. [14] are implausible given the estimated annual population growth rates, estimated empirically from **extensive snow-tracking data…"** [4] citing [6]. If so, present those data in raw FROM and show their validity for 2021.

Reluctance to share data was made obvious in 2022 when PLoS One compelled JLS and TVD to share reproductive data for an article published in 2016. AT requested the data almost 5 years ago. When it was shared by JLS and TVD in 2022, we found it was not only incomplete in several particulars [7] but they once again cited [6] without acknowledging the many weaknesses we describe above that date back to 2003-2004 cited above and have been repeatedly questioned by multiple authors. For example, [18] pointed out that JLS [19] had omitted the evidence for density-dependent reproductions and rely instead on a crude line graph to assert such dependence existed. That same paper [19] made inaccurate and misleading statements about policy periods to model population growth, which we meticulously address in supplementary material in [9]. We still await corrections or clarifications of data, methods, and models. We alerted them again in 2022 [7]. Yet, [4] repeats the same claims. We perceive a house of cards abetted by inadequate data-sharing. Where are the life history rates and data particularized to 2020-2022?

In the absence of data and scientific descriptions of methods that overcome the above weaknesses, claims in [4] about population growth and reproduction are indefensible. Similar problems afflict wolf mortality data. Claimed in [4].

Adult mortality: The debate over Wisconsin wolf mortality has also persisted since 2015 [20]. Our specific case by case questions raised about mortality data remain unanswered [20]. We presented estimates of errors for every recovered wolf carcass, collared or not in that paper. Stauffer's colleagues had the wherewithal to address them line by line because APW recorded those data when he was in charge of reporting wolf deaths [21]. Instead of addressing wolf

deaths case by case to clear up the record, another vague rebuttal without sharing data has been published [10].

Aggravating the problem, JLS [22] repeated a widespread error in modeling vital rates that we described in two separate articles [15, 23]. Although JLS in [24] corrected estimates of hazard, that correction was incomplete as our colleagues demonstrated by treating collared wolf disappearances as an independent endpoint that interacts with legal and other known causes, using competing risks and incidence analyses over time as policies changed [25, 26]. JLS has never acknowledged errors or transparently discussed what else might change in her results [22, 24].

[4] does not fairly summarize the contrary findings. Instead, [4] repeats an unsupported claim that cryptic poaching is rare, "…only minor adjustment was needed (i.e., annual mortality was 25% instead of 24%).", repeating varied assertions by [10, 25, 26]. Nevertheless all fail to substantiate the claim with data on collared wolf mortality, as we have done to model appropriate data-sharing [15, 20].

Assertions about mortality in [4] are untenable because disappearance of radio-collared wolves in four US populations range from 25-50% of all collared wolves approximately (depending on the intensity of monitoring where the Mexican gray wolves and red wolves had lower rates of disappearance and more frequent monitoring while the less-monitored Wisconsin and Michigan populations had higher rates of disappearance). These rates are markedly higher than other marked wildlife estimated at 6-13% [27-29]; also [30] provided a possible maximum estimate of 13-14% for collar failures leading to disappearance. Neither [4] nor Stauffer's co-authors in other articles acknowledge the high rates of collared Wisconsin wolf disappearances or explain how they justify the conclusion that disappearances do not add more than 1% to hazard rates? JLS' [24, 31] inference that cryptic poaching adds little to Wisconsin wolf mortality rates conflicts with recent survival, competing risk and incidence analyses for the same population and approximately the same time period [25, 26]. Those authors passed more stringent peer review processes (registered reports) and have been replicated four times now [25, 26, 32-34]. The claims about mortality in [4] have not been replicated with independent data sets. In fact, [4] does not cite Santiago-Ávila [25, 26, 32] and Louchouarn [33, 34]. That citations practiced by [4] is considered a breach of scientific integrity principle called fairness by the National Academies [35]. We call for an end to the obfuscations in [4].

Conclusions

We are not persuaded by any claims in [4] or those of allies because they lack the transparency needed for modern scientific debate, whether it is transparency in disclosures of competing interests (SM1) or the dozen or so unsubstantiated claims in [4] (SM 2). Our critique exposes why state agency data are so uncertain and patchy that [4] seem to avoid the disinfecting power of sunlight and transparency.

U.S. state wildlife agencies face financial and non-financial competing interests [36] that often oppose wolf population growth and that promote wolf-killing by various means. These interests can lead affiliated researchers to under-estimate mortality and the effects of wolf-hunting and over-estimate population size, growth potential, and births. Higher burdens of proof are required

for the agenda of killing. [4] frames this as two equivalent policy options but in so doing, [4] missteps again. The precautionary principle places a higher burden of proof on those who would kill more wolves — especially when society and legal thresholds affirm precautions and when agencies have a legal duty to preserve the trust in perpetuity [37].

Failures to disclose (SM 1) are breaches of scientific integrity [35]. We sense a disdain for open science in that and in the tone of [4] that asserts correctness yet refuse to share the scientific descriptions of methods or the basic data that flow from those methods (SM 2). Of late, we have called for the state of Wisconsin and specifically authors in [4] to share all data and explain methods and unscientific value judgments interfering with scientific inference [3, 9, 12, 15, 18, 23, 25, 26, 38-40]. We are still waiting.

Funding and competing interests

University of Wisconsin Foundation account AAB7963 paid NXL salary. The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript. We are the authors of the work being discussed [1, 2]. For readers to judge potentially competing interests for themselves, we offer the following: AT funding history http://faculty.nelson.wisc.edu/treves/archive_BAS/funding.pdf and complete CV at http://faculty.nelson.wisc.edu/treves/archive_BAS/Treves_vita_latest.pdf, accessed 30 March 2024. For NXL's CV : Dr. Naomi Louchouarn CV from 2024. AT has a long-standing professional rivalry with Stauffer et al.

Supplementary Material 1

Financial and non-financial competing interests of authors of [4]. We substantiate all claims in an archive of documents at the following site:
https://faculty.nelson.wisc.edu/treves/data_archives/SD1.pdf

Summary
1. In a different journal, many of these same authors were more transparent, writing "For purposes of full disclosure, the authors provide the following additional affiliations that are not listed in the author bylines:.… ERO and APW are advisory board members for the Timber Wolf Alliance of the Sigurd Olson Environmental Institute at Northland College; ERO, APW, and TRV are scientific advisory board members for Wisconsin's Green Fire." Such a declaration for PLoS would come closer to transparency than their current non-disclosure. But the above disclosure is still incomplete as the following points and the data in the URL above show.

2. Employment and career advancement in a wildlife agency that makes revenue from hunters can produce potential competing interests when conducting research on game animals that generate permit fees from hunters. Also, political pressure to reduce the wolf population in Wisconsin has been evidenced for over a decade (many of the citations here and in [4]. The pressure on state scientists has been intense with closure of science services and suppression of science from 2013-2019. At the very least, in our opinion, the authors should disclose that the state receives permit fees for hunting wolves and higher quotas lead to more revenue, which can motivate researchers to over-estimate the sustainability of quotas, survival, birth rates, and population estimates. Here's an example of a more transparent disclosure from an unrelated

article: " ADF&G is primarily funded by hunting and fishing licenses and matching those funds to an excise tax on firearms and ammunition (Pittman-Robertson Act) for wildlife management activities. However, as a public agency ADF&G manages wildlife resources for the benefit of all Alaskans and employees must adhere to conduct standards in scientific objectivity. NPS authors are similarly expected to meet standards of scientific integrity and objectivity. Accordingly, this manuscript underwent formal administrative review and was approved for submission by ADF&G and NPS."

3. The NGOs hosting Stauffer's co-authors are not value-neutral. A recent email exchange reveals how one co-author (APW) used his organizational affiliation when advocating for federal delisting of wolves which would return wolves to the status of state game species. Also, some of the NGO representatives among Stauffer's co-authors receive funds from the Wisconsin DNR, sit on wolf harvest advisory committees, or represent stakeholder interest groups publicly (APW, TVD, ERO). TVD does all three simultaneously (see next).

4. At least one co-author has a financial interest in one or more organizations that funded him for wildlife policy. That co-author also represented an NGO (WGF) on the Wisconsin DNR wolf harvest advisory committee while the coauthor simultaneously held a grant from the Wisconsin DNR for research. Financial interests must be disclosed to PLoS readers.

5. By PLoS editors' own assertion, having published papers that conflict with ours, the authors of [4] have a competing interest to affirm their own work and deny ours. A fully transparent disclosure would mention they have multiple, ongoing professional disagreements with us and our collaborators.

Supplementary Material 2

In addition to the three major sets of unsupported claims made in [4], which we addressed in main text, we find the following additional instances.

"TL posted a comment on their paper (7 Aug 2022) acknowledging the error, stating therein that their conclusions nonetheless remain unchanged [1]. " lines 104-105, [4]. This statement misleads in two ways. First it misleads the reader because there was no error but an alternative estimate that we explained was not defensible. Despite Stauffer's co-authors and [4] assertions that their estimates are more defensible, we disagree for the reasons we explained in main text. The second misleading aspect of this commentary is to treat a correction as if it were a flaw in our work. What do Stauffer et al. do when they need to consider an error? We showed in main text that they do nothing to consider their own possible errors. Moreover, [4] fails to cite our comment on our own paper [2], suggesting they would rather score points than transparently inform readers.

"TL stated that Wisconsin's new occupancy approach to estimated wolf 361 population size was unpublished and not peer-reviewed. This is false because the general methodology was published before the submission of TL [5]."lines 361-364, [4]. We explained why Stauffer et al. are inaccurate here. First, they conflate their peer-reviewed method with the state application of it. Second, their method cannot handle what happened in Wisconsin in 2021 for the reasons we

detailed at length [6]. Unless and until the state of Wisconsin actually subjects its application of their method to data collected in the unusual way it was collected in 2021, we remain unpersuaded.

Lines 390-397 [4]: It is routine in the field of estimating population effects of human hunting to assume additivity (refs). The evidence for compensatory mortality is sometimes mention but as often super-additivity is implicated (refs).In general we found [4] did not pose questions but rather asserts their claimed facts.

Lines 413-414 [4]: "pick and choose the evidence they wish to use based on their personal or organizational values" [4]. By omitting our citations, Stauffer et al. mislead readers into believing it is an insinuation that is not grounded in evidence. Had they actually quoted in context with citations, readers would have seen we quoted peer-reviewed studies. We aren't making an insinuation. We are presenting an interpretation of our published studies that have passed peer review for years. Our interpretation may be inaccurate or whatever, but it isn't an insinuation and instead Stauffer et al. practice selective citation and quotation out of context.

Line 34, [4]: "…[TL]… advocated for zero harvest in fall 2021." This statement is false.

"…but they do not define this distribution analytically." Lines 107-108, [4]. We don't understand what this means and suggest Stauffer et al. would like to have their cake and eat it too. They demand we use [6]but then disagree with us when we use it.

Although [4] uses the word postulated 4 times about our work, [4] never summarizes how the estimates were made in the citation to our so-called postulates [6]. That peer-reviewed estimate of population size in April 2021 [3] is transparent so subject to falsification. Stauffer et al.'s are not because they remain undisclosed. Ours are predictions not postulates.

"There is perhaps nothing inherently wrong with such an approach, but there also is nothing Bayesian about it." (Line 388-389, [4]. Actually, we wrote "…we use Bayesian concepts and terminology but not formal Bayesian algorithms…" Stauffer et al. can quibble but here they sound petty.

"Citing several controversial papers…" line 402. Stauffer et al. like to throw around controversial as if it meant wrong because in their establishment worldview the controversy over WDNR research is wrong. But the papers we cited are in better scientific journals and have withstood replication whereas [4] that we cite in main text from 2008-2021 cannot be replicated because the methods are not transparent and the data were not shared.

"…we 78 specified more defensible parameter values." Lines 78-79, [4]. Scientifically defensible parameters require data.

"Fourth, the distribution presented by TL as representing the new census method (the scaled occupancy approach from [5]) places too much probability mass in the distribution tails, and thus exaggerates uncertainty. In our reanalysis, we specified a distribution that 121 closely matches the empirical scaled occupancy estimate from [7] for N_t (Table 1)" lines 118-122, [4]. If readers had been made aware that Stauffer et al.'s application of the occupancy model is irreproducible, inaccurate, imprecise, and insensitive to changing conditions, plus the selection of validation

tests is biased so the results cannot be generalized [6], the passage in quotations is revealed as a house of cards.

Lines 414-418, [4]: "greatly exaggerated risk of wolf population decline", "harvest as low as 359 wolves" Readers beware of adverbs and adjectives littering [4].

[4] claims of compensatory mortality or reproduction are inadequate without a full review of the contrary evidence. We did so but they chose to ignore it. We summarized both and explained why we modeled neither additive nor compensatory effects in the one year interval of our study as follows:

"Our simple model in Eq 1 assumes no net migration into or out of the state during the study period at a rate relative to deaths or births substantial enough to affect our results. Assuming no net migration is a precaution because it would be hopeful to imagine rescue from outside the state if legal thresholds were crossed in the state. Our assumption seems reasonable given long-distance migration leading to pack establishment has been rare [50]. Also, the assumption of no net migration has been used by others modeling this population [51,52]. Also, Eqs. 1–3 assume linear effects. We assumed no compensatory increases in birth or pup survival other than those encompassed by the range of values in [53]. We do not ignore Allee effects, compensation or negative density-dependence [54,58,59], but we do not model them because too many questions remain for Wisconsin wolves [3,41,43]. Nor do we model non-linear effects that would caution against high death tolls in a second wolf-hunt. For example, depensatory or super-additive effects as described by numerous studies of wolves including in the Wisconsin wolf population [33,36,45,60,61]. We defend the simplicity of our approach as follows: pending evidence that non-linear effects would play out detectably in the short period of our study and pending an analysis of net compensatory and depensatory effects, we simply assume the good conditions studied by [56] encompass any nonlinear effects for wolves in an environment with fewer competitors than before. "

"TL also purported to present the first estimate of annual survival for the period 15 Apr 2020 to 14 Apr 2021, but they did not do so." lines 365-366, [4]. This is a strange sentence. Regardless of how Stauffer et al. feel about our methods and estimate we did provide one. We did not purport to do so, we did.
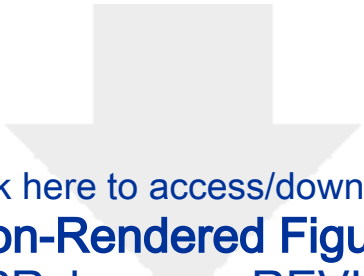
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