Rebutting the WDNR's pseudo-science

Adrian Treves
Nelson Institute for Environmental Studies, University of Wisconsin-Madison, Madison, WI, USA, atreves@wic.edu

[1], hereafter S 2024, criticized TL 2022 [2], in which we attempted to fix a shortage of data during a policy process in Wisconsin 1 March–31 October 2021. The policy process resulted in the implementation of a second wolf-hunting season in one year that a state court halted. Late in 2021, we concluded that even low quotas for a second public wolf-hunt in one year generated detectable probabilities of crossing undesirable legal thresholds for the wolf abundance statewide. Although a state court order ended that planned wolf-hunt, TL 2022 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. Note that estimating a one-year step change in wolf abundance can be modeled in several ways. S 2024 apparently did not like how we estimated births and deaths, but that does not mean we are wrong as S 2024 seem to think.

We began with a peer-reviewed estimate of the Wisconsin wolf population in April 2021 [3] but S 2024 do not have a peer-reviewed estimate for any of the relevant years 2020-2022 yet they insist theirs was "actual" p.5. That is fact by assertion and I devote some text to explaining why the state abundance estimate has serious shortcomings (below).

We also acted in good faith when we followed up with a correction and an evaluation of an alternative life history parameter value, neither of which changed our main conclusions [4]. We question why S 2024 did not cite our comment or our correction. This seems bad faith. Readers might be misled. Therefore, here I repeat those explanations for why Wisconsin estimates of wolf life history and population parameters merit skepticism. I emphasize that the input data (wolf counts, mortality, birth estimates) deserve the most attention not the issue of which model one prefers for a one-year population change.

S 2024 claim that TL 2022 was (a) biased, in error, and incorrect in several passages; and their estimates are (b) correct, actual, and accurate in several assertions without evidence. I rebut (a). I also show point by point why (b) is misleading, reveals an establishment mindset in which authority should not be questioned. Instead, I show why questions have arisen for years about how Wisconsin estimates population and life history parameters.

The only error they found was an arithmetic one we already acknowledged and corrected in [4]. We seriously considered their concern about an arithmetic error and a claim about an inaccurate parameter value relating to pup production. We remedied it in late 2023 and it did not change our conclusions qualitatively. They also claimed a different parameter value for

reproduction, with which we disagree. They present no data to substantiate their claim as I reiterate below. Nevertheless, we explored both, found one wanting evidence [4]; and we agreed about the arithmetic error, which we repaired [4]. Although final values changed by 4% when corrected, our main conclusion did not change. That is the only criticism in S 2024 with which I agree. I return to parameter values after addressing (b) about abundance estimation.

State estimate of wolf abundance:

S 2024 wrote, “The actual estimated spring 2022 population size, after realized zero harvest in fall 2021, was 972 (95% credible interval = 812–1,193) [8].” p.5-6 S 2024. This quote is telling because S 2024 believe their own estimates are “actual” truth. Their claim rests on citation 8 to “Wisconsin DNR. Wisconsin Gray Wolf Monitoring Report 15 April 2021 through 14 April 2022. Bureau of Wildlife Management. 2022.”, Which is not peer reviewed, does not contain even summary data on each survey and does not detail methods. I explain below why the thin veneer of science attached to that estimate is not credible [5]. My reasoning is based on a peer-reviewed rebuttal of the state methods. We found them imprecise, inaccurate, insensitive to changing conditions and irreproducible due to subjective decisions about data handling [5]. To understand why, I need to review briefly the history of scientific debate over Wisconsin wolves.

Scientific debate over Wisconsin wolf life history and abundance estimation

To understand this scientific debate, readers need some description of the limitations of methods Wisconsin used for their estimates of wolf life history and abundance. The state estimate of wolf abundance that S 2024 prefer is a method that depends on annual winter snow-tracking, a method with the following shortcomings.

First, identification of wolf tracks in snow has not been subject to validation since 2000 and that validation by Wisconsin suggested substantial differences between state agency staff and civilian volunteers [6]. To this day, civilian volunteers conduct much of the wolf tracking in the snow. Counts of pack size done at this time and in a subsequent curation of such data, which has never been described in a peer-reviewed article, is verified for only a small percentage of wolf packs by aerial radio-telemetry (fewer than 13% of packs [7]. Therefore, most wolf packs in Wisconsin are identified by an imprecise and uncertain method without scientific accounting for the identity of the trackers or possible double-counting of the same wolves among other possible imprecisions. Nor are all areas surveyed in this way every year as they once were [5].

Second, those input data on wolf presence have not been subject to peer review specific to wolf-counting methods, since the methods were altered in the period 2000-2004 [8]. During that period, we showed why estimates of pack size and estimates of pup survival to winter were confounded [9]. Raw data on wolf pack size and pup survival have never been published [10]; the summaries of such data only cover until 2007[7]; and when models used those data they

neglected to include scripts, data, and clear figures [11]. Although S 2024 claim to have "extensive snow-tracking data" p.8 S 2024, those data are not presented in S 2024 or any other peer-reviewed scientific journal. In sum, wolf life history in Wisconsin as it relates to reproduction stands on thin ice and I dispute each allusion to such data in S 2024 [1].

Third, the method for abundance estimation raises additional concerns. The wolf presence data from mainly snow tracking, described above, are incorporated into a scaled occupancy model published by many of the same authors [12]. We have addressed inaccuracy, imprecision, insensitivity to changing conditions, and irreproducibility of the curation of wolf presence data and the model that uses those curated data in a prior paper [5]. Those concerns remain unanswered and will continue to be disputed until the state make the data fully and transparently available with detailed methods. This is not a new problem as we previously dissected how a lack of transparency in state wolf population data and models was causing problems for state claims [13].

Fourth, the 2024 state estimate of wolf abundance underpins the S 2024 claims about quotas exceeding 300. S 2024 would like us to believe that Stauffer [12] had previously presented data or at least moves readers from summaries of data to final estimate. It does not as we have demonstrated in exhaustive detail [5]. Stauffer et al.'s scaled-occupancy model [12] was not validated for years with wolf-hunts [5]. The state implementation of that model does not seem to include a term for deduction of such deaths and explicitly risks counting dead wolves by using previous years of data on wolf census [5]. Therefore, the burden seems to fall on S 2024 to show that the state counted hunted wolves, how they did so, and what the scientific justification was for using census data from prior years when an unprecedented February wolf-hunt with high mortality interrupted the 2021 census [5]. Similar concerns apply to the 2022 wolf abundance estimate because the scaled occupancy method relies on several prior years' data. I note that S 2024 did not make this plain. This seems a bad faith omission. Therefore, I remain skeptical of the state estimates of abundance based on the scaled occupancy [12] model which S 2024 relies upon and which we previously debunked [5].

Also, S 2024 misunderstood our methods for the one-year step estimate of wolf population change in TL 2022. I find it ironic they accuse us of double-counting mortalities when our analyses summarized above and detailed in [5] indicate the state counts dead wolves as alive. Regarding double-counting the wolf-hunt mortality, I suspect the confusion on their part came from this passage in TL 2022,

> "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, TL 2022).

It is not my job to interpret S 2024, but I guess that they misunderstood that we had deducted February 2021 wolf-hunt mortality from both population estimates (traditional and new scaled-occupancy-model approach) but we did not. We deducted those only from the new approach. We find no evidence that the new occupancy estimate accounted for wolf-hunt mortality [12]. If it did, S 2024 could clarify the public record rather than accusing us of double-counting. However, their position is untenable without transparent presentation of raw data on wolf counts by census block. Given the wolf census of 2021 ended prematurely on the day before 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 assign probabilities >0 of occupancy by dead wolves across much of the state. That seems like a fatal flaw in the scaled occupancy model underpinning S 2024’s supposedly actual population estimate [12] rebutted by [5]. We remind S 2024 and readers that some carnivore populations are under- or over-estimated for political purposes [14]. In the current context, the state of Wisconsin has a financial motivation to overestimate wolves, so as to sell more wolf-kill permits.

Reproduction

S 2024 also question our pup survival and birth rate parametrization. Contrary to their claim in that we, “…wrongly halved the number of pups that survived to November…” and “…counting harvested wolves twice among the dead” — our methods did neither. This might be a simple misunderstanding, but as S 2024 did not quote us, I cannot determine where the confusion arose. As far as I can tell, they simply misunderstood Eq.3 to represent the first half of the year when it actually represents 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 [15]. The debate over [15] remains unresolved [16]. S 2024 revive it without explaining to readers what basis they have for claiming [7] provides a better estimate of pups reaching independence from that given by [15]. That debate between former Wisconsin DNR staff and current ones should have been explained in S 2024. Yet, [7] methods are generally considered imprecise and inaccurate compared to mark-

recapture studies like that of [15]. Instead of sharing raw data and validated scientific methods, S2024 assert their correctness and rely on a single book chapter presenting only summary data through 2007 without scientific descriptions of methods or validation [7], which was published in a chapter of a book edited by two of Stauffer's co-authors. The problems with the latter book have been mentioned for years [10, 11, 13]. Therefore, claims in S 2024 about population growth and reproduction are based upon weak evidence and uncorrected errors in past work by their co-authors.

Adult mortality

The debate over Wisconsin wolf mortality has also persisted because the state does not require its authors to share data transparently [17]. We modeled how such data on wolf deaths can be presented line by line [18]. Instead, S 2024 co-authors published yet another rebuttal without sharing data [19] and we had to rebut them again [17]. Without more, clearer data and scientific presentation of methods, the debate will never rise above its current, arid level.

S 2024 cite [20], which in my view perpetuated an error in modeling vital rates that we described twice [18, 21]. Although [22] corrected their estimates of hazard, that correction was incomplete as my colleagues demonstrated by treating collared wolf disappearances as an independent endpoint deserving more careful analysis of competing risks over time [23, 24]. Those findings have been replicated three more times for different populations and policy periods [25-27]. S 2024 does not fairly summarize our findings. Selective citation is a breach of research integrity according to the National Academies of Science (2017, p.36 https://doi.org/10.17226/21896). Instead, they repeat an unsupported claim that cryptic poaching is rare, "…only minor adjustment was needed (i.e., annual mortality was 25% instead of 24%)." That claim is untenable as I explain next.

Disappearance of radio-collared wolves in four US populations range from approximately 25-50% of all collared wolves. Variation seems to depend on the intensity of monitoring where the Mexican gray wolves and red wolves had lower rates of disappearance and more frequent monitoring whereas the less-monitored Wisconsin and Michigan populations had higher rates of disappearance [23-28]. S 2024 and related work have not addressed the association with policy periods, monitoring frequency, nor why wolves experience rates of disappearance two to four times higher than other marked wildlife`, which experience rates of disappearance of 6-13% [29-31]. Studies of collar failure do not reach the rates of disappearance seen among Wisconsin wolves [32]. [32] provided a possible maximum estimate of 13-14% for collar failures leading to disappearance. For further detail, see [17]. Instead of fair citation and addressing the substance of the debate, S 2024 embraces models that fail to include inter-year variation in rates of legal wolf-killing, do not handle competing risks with state of the art techniques from biomedical research on survival, and withhold data from readers and peer researchers [23].

In sum, the sweeping, vitriolic claims of S 2024 about parametrization bias and errors are shown above to be mere disputes about differing estimates. Their claims that we double-counted are unsubstantiated and seem to reflect misunderstandings. Their arguments that we should use better model specifications stumble on issues of non-independence of data, data that are not shared, and disputes over how to model. I am not persuaded, especially given the non-disclosures in S 2024 that I describe next.

Non-disclosure and non-transparency

S 2024 did not fully disclose competing interests, when they stated, "The Wisconsin Department of Natural Resources is partially responsible for wolf management in the state of Wisconsin. The authors declare that “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.” S 2024 p.1-2. However, I present below evidence to contradict that claim.

Three co-authors of S 2024 wrote in [19], “… 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.” That disclosure belonged in S 2024 also. Even that disclosure is incomplete in both financial and non-financial interests ((https://faculty.nelson.wisc.edu/treves/data_archives/SD1.pdf). The public information in the latter link belies S 2024 disclosures. Disclosures are important for reviewers and readers to be aware of the potential for financial and non-financial interests to have influenced approach, tone, and interpretation. Nor was their non-disclosure unintentional because I reviewed the second revision of S 2024 in Fall of 2023 and pointed out the omissions to the editors and authors. Interestingly, the editor asked me not to share information that is not public. However, the information above is public and moreover authors are required to disclose private information that could be a competing interest.

Non-disclosure is an example of misleading reviewers, editors, and readers. In an ironic an unwittingly correct assertion, S 2024 claim “we believe that our work exposes a serious failure in the peer-review process.“ p.11. It appears they misled their reviewers (and possibly an editor), so their claim seems hypocritical.

Finally, S 2024 does not meet the criteria for publication in this journal (http://journals.plos.org/plosone/s/criteria-for-publication), by failing the following criteria “4. Conclusions are presented in an appropriate fashion and are supported by the data… [what data?]… 6. The research meets all applicable standards for the ethics of experimentation and research integrity. …[see non-disclosures above] …7. The article adheres to appropriate reporting guidelines and community standards for data availability [undisclosed competing

interests, unshared data].” (emphasis added). Therefore, I urge retraction. S 2024 also assert correctness and others’ wrongness rather than engaging in debate over data and methods of collecting those data. They would prefer to debate models for which the input data can be portrayed inaccurately.

Funding and competing interests

I am one of the authors of the work being discussed TL 2022. For readers to judge potentially competing interests for themselves, I offer 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 10 August 2024.

5. REFERENCES

1. Stauffer GE, Olson ER, Belant JL, Stenglein JL, Price Tack JL, Van Deelen TR, et al. Uncertainty and precaution in hunting wolves twice in a year: Reanalysis of treves and louchouarn. PLoS One. 2024;19(6):e030148.
2. Treves A, Louchouarn NX. Uncertainty and precaution in hunting wolves twice in a year. PLoS One. 2022; 17(3):e0259604.
3. Treves A, Santiago-Ávila FJ, Putrevu K. Quantifying the effects of delisting wolves after the first state began lethal management. PeerJ. 2021; 9: e11666.
4. Treves A, Louchouarn NX. Considering a possible error in the proportion of packs breeding. PLoS One. 2024;comment on https://doi.org/10.1371/journal.pone.0259604.
5. Treves A, Santiago-Ávila FJ. Estimating wolf abundance with unverified methods. Academia Biology. 2023;1:10.
6. Wiedenhoeft JE, Boles SR, Wydeven AP, editors. A volunteer carnivore tracking program and its potential use in monitoring the timber wolf (canis lupus) population in northern and central wisconsin. World Wolf Congress 2003: Bridging Science and Community; 2003; Banff, Alberta, Canada.
7. Wydeven AP, Wiedenhoeft J, Schultz RN, Thiel RP, Jurewicz RR, Kohn B, et al. History, population growth and management of wolves in wisconsin. In: Wydeven AP, Van Deelen TR, Heske EJ, editors. Recovery of gray wolves in the great lakes region of the united states: An endangered species success story. New York: Springer; 2009. p. 87-106.
8. Wydeven AP, Wiedenhoeft JE, Schultz RL, Thiel RP, Boles SH, Heilhecker E, et al. Progress report of wolf population monitoring in wisconsin for the period october - march 2004. In: Resources WDoN, editor. Park Falls, Wisconsin PUB-ER- 2004: Wisconsin Department of Natural Resources; 2004.
9. Wydeven AP, Treves A, Brost B, Wiedenhoeft JE. Characteristics of wolf packs in wisconsin: Identification of traits influencing depredation. In: Fascione N, Delach A, Smith ME, editors. People and predators: From conflict to coexistence. Washington, D. C.: Island Press; 2004. p. 28-50.
10. Treves A, Darimont CT, Santiago-Ávila FJ. Comment on correcting stenglein & van deelen 2016 & comment on 2022 correction to stenglein & van deelen 2016. PLoS One Comments. 2022:https://journals.plos.org/plosone/article/comment?id=10.1371/annotation/4d92a9da-dc73-41bb-ad83-837ed707c948&

https://journals.plos.org/plosone/article/comment?id=10./annotation/cb45650a-9340-409e-a753-ef47579427ab.
11. Chapron G, Treves A. Reply to comments by olson et al. 2017 and stien 2017. Proceedings of the Royal Society B. 2017; 284(1867):20171743.
12. Stauffer GE, Roberts NM, MacFarland DM, Van Deelen TR. Scaling occupancy estimates up to abundance for wolves. The Journal of Wildlife Management. 2021;85(7):1410-22.
13. Treves A, Paquet PC, Artelle KA, Cornman AM, Krofel M, Darimont CT. Transparency about values and assertions of fact in natural resource management. Frontiers in Conservation Science: Human-Wildlife Dynamics. 2021;2:e631998.
14. Darimont CT, Paquet PC, Treves A, Artelle KA, Chapron G. Political populations of large carnivores. Conserv Biol. 2018;32(3):747–9.
15. Thiel RP, Hall W, Heilhecker E, Wydeven AP. A disjunct gray wolf population in central wisconsin. In: Wydeven AP, Van Deelen TR, Heske EJ, editors. Recovery of gray wolves in the great lakes region of the united states: An endangered species success story. New York: Springer; 2009. p. 107-18.
16. Treves A, Louchouarn NX. Considering a possible error in infant survival estimates in thiel et al. 2009 and whether wydeven et al. 2009 might be more precise and accurate. PLoS One Comments. 2022;
17(3):https://journals.plos.org/plosone/article/comment?id=10.1371/annotation/ad36aab3-f88e-4fb6-b01b-7405bdab7ad0.
17. Treves A, Agan SW, Langenberg JA, Lopez-Bao JV, Louchouarn NX, Parsons DR, et al. Response to roberts, stenglein, wydeven, and others. J Mammal. 2024;in press.
18. Treves A, Langenberg JA, López-Bao JV, Rabenhorst MF. Gray wolf mortality patterns in wisconsin from 1979 to 2012. J Mammal. 2017;98(1):17-32.
19. Roberts NM, Stenglein JL, Wydeven AP, Stauffer GE, Macfarland DM, Van Deelen TR, et al. Commentary: Gray wolf mortality patterns in wisconsin from 1979 to 2012. J Mammal. 2024.
20. Stenglein JL, Van Deelen TR, Wydeven AP, Mladenoff DJ, Wiedenhoeft J, Langenberg JA, et al. Mortality patterns and detection bias from carcass data: An example from wolf recovery in wisconsin. J Wildl Manage. 2015;79(7):1173-84.
21. Treves A, Artelle KA, Darimont CT, Parsons DR. Mismeasured mortality: Correcting estimates of wolf poaching in the united states. J Mammal. 2017;98(5):1256–64.
22. Stenglein JL, Wydeven AP, Van Deelen TR. Compensatory mortality in a recovering top carnivore: Wolves in wisconsin, USA (1979–2013). Oecologia. 2018;187(1):99–111.
23. Santiago-Ávila FJ, Chappell RJ, Treves A. Liberalizing the killing of endangered wolves was associated with more disappearances of collared individuals in wisconsin, USA. Scientific Reports. 2020;10:13881.
24. Santiago-Ávila FJ, Treves A. Poaching of protected wolves fluctuated seasonally and with non-wolf hunting. Scientific Reports. 2022;12:e1738
25. Santiago-Ávila FJ, Agan S, W.,, Hinton JW, Treves A. Evaluating how management policies affect red wolf mortality and disappearance (registered report). Royal Society Open Science. 2022; 9:210400.
26. Louchouarn NX, Santiago-Ávila FJ, Parsons DR, Treves A. Evaluating how lethal management affects poaching of mexican wolves (registered report). Royal Society Open Science. 2021;8:200330.
27. Louchouarn NX. Don’t judge the roar by its echo: Tests of assumptions, tools and policies for human-carnivore coexistence in north america: University of Wisconsin-Madison; 2023.
28. Agan SW, Treves A, Willey EL. Estimating poaching risk for the critically endangered wild red wolf (canis rufus). PLoS One. 2021; 16(5):e0244261.

29. Martin H, Hebblewhite M, Hubbs A, Corrigan A, Corrigan R, Merrill EH. Male elk survival, vulnerability and antler size in a transboundary and partially migratory population. The J Wildl Manage. 2023;87(4):e22386.
30. Sergeyev M. Annual survival and harvest vulnerability of elk (cervus canadensis): Brigham Young University; 2019.
31. Lamb C, Smit L, Mowat G, Mclellan B, M. P. Unsecured attractants, collisions and high mortality strain coexistence between grizzly bears and people in the elk valley, southeast british columbia. Conservation Science and Practice. 2023;5(10):e13012.
32. Habib B, Shrotriya S, Sivakumar K, Sinha PR, Mathur VB. Three decades of wildlife radio telemetry in india: A review. Animal Biotelemetry. 2014;2:4:4-13.