From: **Adrian Treves** atreves@wisc.edu
Subject: Please add to the rule-making file and the information being considered in recommending a 2021 quota for both the NRB and the DNR wolf team.
Date: July 13, 2021 at 9:44 AM
To: Johnson, Randy D - Dnr Randy.Johnson@Wisconsin.gov, jennifer.pricetack@wisconsin.gov, Ross, Laurie J - DNR Laurie.Ross@wisconsin.gov
Cc: Francisco J. Santiago-Avila santiagoavil@wisc.edu, KARANN PUTREVU kputrevu@wisc.edu

Laurie J. Ross, Board Liaison
Office of the Secretary
Laurie.Ross@wisconsin.gov
608-267-7420
PO Box 7921
Madison WI 53707-7921

and

Randy Johnson
Wildlife Biologist

Dear Laurie, Randy, and Jennifer

Please find attached our most recent paper on wolf management and our response to criticisms of it from sources at the DNR. Also, we are specifically requesting that both be added to the rule-making file AND the information being considered in recommending a 2021 quota for both the NRB and the DNR wolf team.

To make your job easier, here is a list of prior public comments that we submitted which should already be on the record for both NRB and DNR relating to wolf management:

2 July 2021 Letter to Wisconsin DNR re (mis)interpretation of the Adams et al. 2008 model and misuse for design of wolf hunting. Click here for full text.
18 June 2021 and 23 June 2021, Prof. Adrian Treves, PhD, offered a comment on Wisconsin Wolf management. For the full text of the comment, Click here
Prof. Adrian Treves, PhD, offered a comment on Wisconsin Wolf management. . For the full text of the comment, Click here
4 June 2021: The WDNR has often claimed that hunting wolves and other predators will generate net benefits for society. The common benefits claimed are protection of livestock, human safety, and improved tolerance for the survivors in the same population. The scientific evidence does not support these claims. For the full text of the comment, Click here
15 May 2021: regarding all aspects of wolf science. For the full text of the comment, click here. For the Appendices click here and for references cited click here.
15 May 2021: Comment to WDNR by Dr. Francisco J. Santiago-Ávila Full text here.
Related public comments sent to tribal and Wisconsin state officials can be found here.
Letter to the U.S. Fish & Wildlife Service from scientists concerned about Wisconsin wolf policy and management. (updated October 15th, 2014). Note this was sent to Dr. D. Macfarland at the time also.

Thanks in advance and have a good week

Adrian Treves, PhD

apologies for typos

Adrian Treves, PhD
Professor, Nelson Institute for Environmental Studies
University of Wisconsin-Madison
30A Science Hall, 550 North Park St., Madison, WI 53706, USA
atreves@wisc.edu • http://nelson.wisc.edu/people/treves/ • Tel: +1-608-890-1450

The University of Wisconsin–Madison occupies ancestral Ho-Chunk land, a place their nation has called Teejop (day-JOPE) since time immemorial.

In an 1832 treaty, the Ho-Chunk were forced to cede this territory.

Decades of ethnic cleansing followed when both the federal and state government repeatedly, but unsuccessfully, sought to forcibly remove the Ho-Chunk from Wisconsin.

This history of colonization informs our shared future of collaboration and innovation.

Today, UW–Madison respects the inherent sovereignty of the Ho-Chunk Nation, along with the eleven other First Nations of Wisconsin.

Response_WDNR_critic...AL.pdf
Treves_FSA_Putrevu_2021.pdf

Treves, Santiago-Ávila, Putrevu rebuttal of DNR critique of 1. Treves, A., F.J. Santiago-Ávila, and K. Putrevu, *Quantifying the effects of delisting wolves after the first state began lethal management.* PeerJ, 2021. **9**: p. e11666. 10.7717/peerj.11666.

Text in italics are spurious criticisms raised by the DNR or its allies which we rebut in boldface below.

1. Treves et al.'s comparison of the pre-hunt minimum population count and the predicted post-hunt (April) population size is inappropriate; and "apples to oranges" comparison. Wolf populations naturally fluctuate over a year, and the appropriate comparison of population sizes to assess impact of harvest to the population requires a 1-year time step comparing similar points in the population cycle. In other words, the authors would need to compare pre-hunt 2021 to pre-hunt 2022 or April 2021 to April 2022 (post-hunt), for which data are not yet available.

First, in both cases we are talking about wolves killed. We also compared April 2020 (pre-hunt) to April 2021 (post-hunt), a "1-year time step comparing similar points in the population cycle".

Second, we used WDNR data to calculate population growth during periods of strict ESA protections (average and minimums for 2017-2020), and simply added additional mortality that those models are unable to account for and that the models currently used by the DNR would be unable to capture given they do not consider increments in human-caused mortality from reducing protections, in contrast to Santiago-Ávila et al. (2020) **and Chapron & Treves** (2016a, 2016b)**.**

We agree that estimates of longer-term impacts would need to wait and compare April 2021 to April 2022, and yet the WDNR is going ahead with a Fall wolf hunt that may further imperil the wolf population *without being able to provide better estimates of impact for the past hunt*. Indeed, the implementation of a wolf hunt this Fall may very well preclude any estimation of specific effects from the February hunt.

The WDNR's suggested time steps imply that assessing the effects of the February hunt is somehow scientifically invalid, an implication that we reject wholesale.

Furthermore, the WDNR seems to be confusing the issue. In our peer-reviewed paper, we explained why a simple model was not only adequate, but in fact superior to more complex and uncertain models (Appendix 1). Now the DNR is trying to claim more complexity is needed (e.g., reproduction in 2021) before evaluating the impact of the February 20-21 wolf-hunt. We consider this to be a misleading claim and invite the DNR to engage in more transparent dialogue.

2. Treves et al.'s findings rely on methods that have been repeatedly criticized in the scientific literature. For example, the assumptions made about unknown collar failures being illegal killing are speculative, lacking data to support the illegal killing rate used in the paper. Further, the wolf population response during previous Wisconsin harvests provides no support for Treves et

Treves, Santiago-Ávila, Putrevu rebuttal of DNR critique of 1. Treves, A., F.J. Santiago-Ávila, and K. Putrevu, *Quantifying the effects of delisting wolves after the first state began lethal management.* PeerJ, 2021. **9**: p. e11666. 10.7717/peerj.11666.

al.'s claims that our methods are leading to significant overharvest of wolves. In fact, when we compared the observed change in the WI wolf population during harvest years to predictions using the Adams et al. 2008 published relationship, the greatest deviation from the mean expected change was an increase in the wolf population of >15%, not a population decline. Further, the observed change in the population size was still captured in the 80% prediction interval, lending more support for the predictive value of the Adams et al. 2008's relationship for informing Wisconsin quota decisions.

We consider the claim that our methods have been 'repeatedly criticized' to be weak in the absence of cited evidence. Nevertheless, this gives us an opportunity to provide the evidence from the past DNR work by Stenglein et al. 2015 to refute the current DNR's claim and set the record straight. Here we present a fully referenced history of the field.

There were four published short comments that criticized Chapron & Treves (2016a,b), in which we first proposed that reducing ESA protections led to population growth slow-downs or declines. None of the comments on our work presented new data, just repackaged a claim that population growth slow-downs reflected the Wisconsin wolf population nearing carrying capacity in 2012 (when the wolf population was estimated at 815 wolves or less than three-quarters of the 2020 population level).

Two comments from non-Wisconsin folks argued for negative density-dependence on mortality or population growth, instead of an additional source of mortality surfacing when ESA protections were loosened (Pepin et al. 2017; Stien 2017). Contrary to this claim, there is consensus among Wisconsin scientists that there was no negative density-dependence on mortality and an additional source of mortality of 4-5% annually is needed to explain declining growth rates during those 6 periods of reduced protection under the ESA (Stenglein et al. 2015; Chapron & Treves 2016a,b). We rebutted the Pepin and Stien comments easily because they presented no new data and seemed not to be familiar with Wisconsin wolf science (Chapron & Treves 2017a,b). Subsequent events have further confirmed our findings (See below and Treves et al. 2021b) and we have never heard from Pepin, Stien or their co-authors since. Therefore, we cannot see why the DNR would be referring to that discussion.

The DNR is likely to be referring to a third comment by Wisconsin scientists Olson, van Deelen, Wydeven and others (Olson et al. 2017). We pointed out these mistakes they made, the deliberate omission of information on wolf census, and again challenged the lack of new data behind their claims (Chapron & Treves 2017b). This year we showed how incomplete and irreproducible that team's work has been (Treves et al. 2021b). Our conclusions have not been weakened by new evidence – science must be judged on the quality of its methods or on new data not on how personal value judgments line up with the findings or not.

Treves, Santiago-Ávila, Putrevu rebuttal of DNR critique of 1. Treves, A., F.J. Santiago-Ávila, and K. Putrevu, *Quantifying the effects of delisting wolves after the first state began lethal management.* PeerJ, 2021. **9**: p. e11666. 10.7717/peerj.11666.

Science progresses by rejecting old hypotheses not by repeated skepticism without new evidence. So the DNR's claim that our work has faced scientific criticism is correct but that criticism fell short of the mark.

Furthermore, the social science data by Hogberg et al. 2015 and Browne-Nuñez et al. 2015, corroborate the claim that legalizing or liberalizing wolf-killing lowers tolerance and leads to more intentions to poach and more calls for more wolf-killing. We summarized the social scientific evidence in Science in 2014 (Treves & Bruskotter 2014).

Finally, the above findings have been independently verified by Santiago-Ávila et al. 2020 and Louchouarn et al. 2021 using independent datasets relating to radio-collared wolves in Wisconsin and New Mexico/Arizona respectively. Tests of our hypotheses are underway for red wolves and other populations as we write.

The DNR is aware of this history because we have shared each new paper and put it in context for them in person and in written communications. They cannot claim ignorance or misinterpretation. None of what we present here should be news to the DNR given our record of public comment and letters to the DNR since 2014 and even before. All are available freely online here for the public: http://faculty.nelson.wisc.edu/treves/CCC.php

It's time for the DNR to share its data openly and present evidence in a scientific debate judged by nationally recognized experts who have not been involved in the debate heretofore. The editors of the best journals in the world would be acceptable to us as arbiters of the debate and as selectors of the independent experts. Contrary to the DNR's claims and criticisms, we provide data and references for all our claims.

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We are not aware of any peer-reviewed criticisms of the model for estimating cryptic poaching so the DNR claim that we have been "repeatedly critiqued" is misleading when it comes to estimating cryptic poaching. Our results since 2017 stand unchallenged in the scientific literature and continue to be reaffirmed, as we explain below.

Nor is there a peer-reviewed counterhypothesis of which we are aware, let alone contrary evidence, to our findings that additional radio-collared wolves disappear when policy changes. Because radio-collars cannot respond to policy, and there is no known or even plausible biological mechanism for wolves to migrate out of state following policy changes (and the evidence is exactly the opposite: more immigration during periods of reduced ESA protections), we inferred that disappearances of radio-collared wolves were attributable mostly to cryptic poaching. The inference that most disappearances of radio-collared carnivores are accidence of human interference is corroborated by an independent analysis of Mexican gray wolves in Arizona and New Mexico (Louchouarn et al. 2021), a study of red wolves in North Carolina (Again et al. 2021), a study of tigers in Russia (Goodrich et al. 2008), and several other wolf populations in the USA (Treves et al. 2017a,b).

The models by Santiago-Ávila et al. and Louchouarn et al. use all available monitoring records of adult collared wolves in their respective jurisdictions. Moreover, in our current study, we did not use an illegal killing rate, but rather calculated the proportion of all mortalities attributable to unexplained disappearances of radio-collared wolves from the DNR's own data from 1980-2012 and used that to model additional deaths and disappearances from 3 November 2020-14 April 2021.

The WDNR's is a denialist argument that doesn't provide alternative hypotheses or evidence, it just dismisses the data.

3. During the quota discussions, the DNR used a non-harvest, human-caused mortality of 14% to determine a quota. The known mortality rate was <5%, resulting in a conservative mortality rate.

Where's the evidence of this <5%? Stenglein et al. (2018) estimates a rate of illegal killing of 9.4% and of other human-caused mortality of 5.1% (~14% when added) but Stenglein et al. 2015b estimated annual mortality at 23.5% so where is the peer-reviewed evidence to support the lower figures? However, the issue is precisely that they are substantially underestimating mortality, and even more so during periods of reduced protections (which Stenglein et al does not consider). Treves et al. (2017a) analyzed data of wolf deaths and estimated >80% of poached wolves not monitored went unreported, and also found the mortality rate was much higher for unmonitored wolves; both findings make our study assumptions conservative in estimating mortality. Subsequent research found the relative risk of other (non-legal) human-caused mortality may be underestimated by 17-36%.

Treves, Santiago-Ávila, Putrevu rebuttal of DNR critique of 1. Treves, A., F.J. Santiago-Ávila, and K. Putrevu, *Quantifying the effects of delisting wolves after the first state began lethal management.* PeerJ, 2021. **9**: p. e11666. 10.7717/peerj.11666.

Moreover, and more importantly perhaps: are we supposed to take the DNR's word on these estimates of 14% and 5%, when we are unable to independently corroborate those rates because they withhold data from public scientists, NGOs and even co-sovereign Tribes? For instance, wolf data were stored as recently as 2020 in the Natural Heritage Inventory, but we are told by tribal government staff that those data have been moved by the DNR without revealing where or providing the data.

Stenglein, J.L., T.R. Van Deelen, A.P. Wydeven, D.J. Mladenoff, J. Wiedenhoeft, J.A. Langenberg, and N.J. Thomas, *Mortality patterns and detection bias from carcass data: An example from wolf recovery in Wisconsin.* Journal of Wildlife Management, 2015. 7: p. 1173-1184.

3. Treves et al. use the minimum count in their study, rather than the best available science, which is captured in the occupancy-based estimate reported by the DNR and used to inform the February 2021 quota discussions. The occupancy-based estimate is based on a method that has been peer-reviewed and accepted, by Journal of Wildlife Management.".

This is a red herring. First, our use of the minimum-count is more cautious than the DNR's, and relies on time-tested population estimates that are consistent with pack counts and average pack size for 40+ years. Second, for reasons stated in our PeerJ paper, the minimum population estimate was both still used for quota decisions, and it still lies within the bounds of the occupancy model, so there is no qualitative scientific discrepancy with their new occupancy model(s). Third, results remain to be published by the JWM. Upon the publishing of these estimates, we readily invite the DNR to use our model to recalculate additional mortality with their preferred population estimate. But they should be explicit about the discrepancy between the new occupancy model and the traditional wolf monitoring methods that are consistent with the number of packs detected.

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Appendix 1
Why a population model without nonlinear effects is conservative

Non-linear responses such as density-dependence and compensatory mechanisms acting on population dynamics have sometimes been identified for some wolf populations in some periods. However, using a more complex model is not justified for several reasons.

The general population ecology literature predicts when density-dependence will not be observed and Wisconsin's wolves met three of the criteria: Fowler [1] long ago reviewed many mammal populations, wherein a majority -- but not all – of wildlife populations show density-dependence. Similarly, Brook and Bradshaw [2] added to our understanding with modeling of 1198 populations and explained why many conditions would prevent or obscure a density-dependent population dynamic for long periods or forever. We now cite our work published this month that documents three of those conditions to explain why Wisconsin wolves did not show negative density-dependence from 1980-2012 [3]. Indeed, it seems unsurprising that the Wisconsin wolves did not show negative density-dependence given pack territoriality, minimal changes in density over time, and the change in census methods 2 times between 1980-2012 and another 2 times between 2013 and the present.

Secondly, Stenglein et al. [4] and Chapron & Treves [5] agree there was no detectable negative density-dependence on mortality (which is our main focus). Although there is no consensus on density-dependence on reproduction, the only study finding such [4] found negative density-dependence which would tend to decrease the survival of juveniles to November 2020, increasing the percent reduction in the population reported. Our omission of such is therefore conservative. Furthermore, we find the evidence for negative density-dependence on recruitment is unsubstantiated [6, 7]. Therefore, inclusion of negative density-dependence is not justified and would only add to our estimate of percent reduction.

Some readers might wonder if there were compensatory effects on vital rates that might have arisen during our study period and might have led to an increase in the population or its rate of growth beyond the 3.8% average rate we began to model with. During our study period April 2020-April 2021, compensatory effects might surface as fewer deaths from other causes because hunters killed wolves, more immigration because of vacancies created by

hunting, or more births or higher recruitment rates because of space or resources freed up by the death of adults during the hunt. However the hunt occurred at the end of February 2021, so compensatory effects of the hunt, if any, would emerge afterwards. Our modeling concerns April 2020-April 2021. Claims about compensatory effects that might recover lost population have to show evidence for such effects AFTER the hunt.

Therefore, our simple population model, based on average growth rates of previous years, already incorporates potential structural non-linear effects to predict the population by November 2020 and then we begin deducting the additional human-caused deaths that began with federal delisting. The cumulative incidence functions we describe below already incorporate non-linear effects on mortality but research has proven those effects are depensatory not compensatory [8, 9], which is consistent with population-level analyses showing super-additive mortality [10] and depensatory population decline [5, 6, 11-13].

Regarding the inferences we made about depensatory effects, below we explain how the inferences about cryptic poaching were made with confidence.

We used time-to-event analyses on radio-collared wolves in two populations (and two independent datasets) to evaluate the changes in survival of wolves over time as policies changed. The policies in question were reductions in ESA protections interspersed with periods of stricter protection. We showed that disappearances of radio-collared wolves increased substantially during periods with reduced ESA protections, estimated at 19% in Wisconsin's less intensively monitored population [8] and 121% in the Mexican gray wolf population monitored five-ten times more intensively by several measures [9]. But it is not only the association with 6 changes in policy that allow us to estimate cryptic poaching.

We also examined mechanical failures of collars and migration. There are only three known outcomes for wolves with radio-collars that are lost to monitoring by radio-telemetry. First the transmitter may undergo mechanical or battery failure. Second, the collared animal may migrate out of range of telemetry. Third, the animal may die and its transmitter be destroyed by people.

There is no known mechanism by which policy change can cause mechanical or battery failure but moreover, disappearances of radio-collared wolves occur several hundred to 1000s of days earlier than expected for the life of radio-collars judging from the average life of collars in wolves that die from natural deaths, as we have shown for a number of populations of wolves [14-16].

As for migration, a radio-collared wolf must leave the state not simply shift range to be lost to monitoring. For Mexican gray wolves, the intensive monitoring once per week or twice per week and assiduous search for missing wolves did not reveal more than perhaps one case in >400 of a wolf with radio-collar migrating so far and being lost to contact, i.e., even migrants

are often recovered [9]. A red wolf study with >500 radio-collared wolves reported the same pattern [14]. Although Wisconsin wolves were not monitored as intensively then or now, our prior work showed migration was seven times more frequent into Wisconsin from Michigan than the converse [15], and moreover we used the more conservative cumulative incidence curves from Wisconsin rather than the much more dramatic 121% increase in LTF from Mexican gray wolves. Note emigrants are nonetheless lost to Wisconsin's wolf population making emigration an unsatisfactory rebuttal of our estimate of population decline.

We consider it vanishingly rare that a radio-collared wolf died of a non—poaching cause in a medium or substrate (e.g., salt water or underground) that destroyed the transmitter soon after death, although a case of poaching and dumping in saltwater is known [14].

The only remaining possible cause of disappearance of a monitored wolf is human manipulation. A great deal of social scientific data also support the willingness and intention to poach wolves in Wisconsin and beyond [17-21]. That evidence was summarized in Science [22].

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