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SUBJECT: Review of the FWS Species Status Assessment (SSA) for gray wolves *Canis lupus* in the eastern USA under the U.S. Endangered Species Act (ESA)

As a former official peer reviewer for FWS in 2019, I remain interested in how science is used to inform policy on endangered species, particularly wolf populations. Accordingly, I am sharing my unofficial peer review of the2025 SSA for gray wolves in the eastern USA: https://ecos.fws.gov/docs/recovery_plan/20251103_SSA%20for%20the%20Gray%20Wolf%20in%20the%20Eastern%20United%20States_final_508.pdf. My review is freely available at https://faculty.nelson.wisc.edu/treves/CCC.php.

Executive summary of the review by Adrian Treves, PhD

The U.S. Fish & Wildlife Service (FWS) 2025 SSA for eastern gray wolves presents a confident picture of secure gray wolves in Minnesota and perhaps other Western Great Lakes (WGL) states, along with little need or hope for wolf subpopulations establishing elsewhere east of the Mississippi for the foreseeable future. FWS confidence is misplaced because of unsupported suppositions, invalid assumptions, unscientific value judgments, and some flat out errors:

1. **Errors:** FWS errs in its definition of suitable habitat for wolves. FWS seems to believe suitable refers to whether wolf use suits humans. FWS errs in its estimates of maximum levels of lethal

control and maximum levels of human-caused mortality. FWS erroneously assumes density-dependent population dynamics despite scientific disagreements or even scientific consensus to the contrary. FWS errs in understanding patterns and causes of human tolerance, illegal killing of individual wolves, and sustainable human-caused mortality for wolf populations. For example, FWS equates owning livestock with intolerance for wolves. FWS ignores or dismisses reams of evidence indicating illegal wolf-killing is perpetrated by a small minority with extreme views; relatedly, the FWS ignores six studies showing FWS policy has increased rates of illegal killing. FWS erroneously treats the upper limit of sustainable human-caused mortality for wolf populations as 48% despite an FWS peer reviewer who showed arithmetically that value was too high and scientific consensus settling on 29% or lower. FWS erroneously treats human-caused mortality as consisting only of lethal control and 'harvest'. FWS assumes states count wolves accurately, precisely, reproducibly (hallmarks of good science), with sensitivity to changing conditions. FWS assumes this by ignoring several critiques of how WGL states count wolves and despite Minnesota estimating crudely every 5 years or more.

2. **Flawed assumptions and unscientific value judgments:** FWS assumes states will follow their management plans strictly, which are not even guidance documents and therefore unenforceable, in the opinion of the Wisconsin Court of Appeals in 2026. Even if state management plans are enforceable, they are subject to swift regulatory changes or statutory over-rule. Therefore, FWS assumptions are unrealistic that lethal management policies will not change in the next five years let alone the 100 years they model. This optimism fails to recognize the reasonable worst case that sudden dramatic events will recur, such as that seen in Wisconsin when a second hunt was planned for Fall 2021 with a quota of 50-100% of the remaining population, after the unprecedented overkill of Feb 2021. That case exposed the inadequacy of Wisconsin's regulatory mechanisms. Nevertheless, FWS optimistically anticipates states will follow their management plans. Beyond Wisconsin, FWS

assumes states will voluntarily comply with FWS desires, rather than putting in place ESA-mandated post-delisting monitoring plans founded in science and effective interventions for emergency relisting. For regions beyond the current range of wolves in the WGL, the FWS appears to deny recolonization by wolves by dismissing most evidence, claiming without evidence that confirmed wolves are loners, and circumscribing the historical range of gray wolves based off flimsy evidence or a single, discredited study by FWS insiders. FWS erroneously claims it has not made normative or value-based judgments. It does so dozens of times in association with modeling wolf futures. FWS generally uses terminology with negative valence about wolves, such as "pack hunters" rather than cooperative hunting or teamwork, "livestock depredation" rather than predation on domestic animals. These are value judgments not scientific choices; yet they slant FWS science subtly. The slant surfaces in encouraging rather than discouraging high rates of wolf-killing (citing a study found to make arithmetic errors, failing to cite studies that correct arithmetic errors) and encouraging business as usual by WGL states even though the geographic expansion of their wolf populations stalled out approximately 20 years ago.

3. **Apparently unlawful neglect of the ESA:** Many of the missing peer-reviewed articles are published in better journals using stronger methods than the ones cited by FWS, Also many were provided to the FWS by colleagues or by me in public comments on this and prior regulatory proposals going back years including my official peer review in 2019, which makes it part of the administrative record by definition. Therefore, the FWS is not unaware of the work but rather dismissing it or ignoring it. Such conduct violates the "available" part of best available scientific and commercial data" (BAS). When FWS is confronted by a scientific debate or disagreement between scientists, it selects the result it prefers rather than the result supported by better methods or stronger inference. Similarly, it selects scientists whom it agrees with even when their work is rebutted for non-transparency,

irreproducible results, or errors. Such conduct violates the "best" part of best available scientific and commercial data" (BAS).

None of the above assumptions, decisions, or errors are cautious or conservative about the security or protection of wolves. Instead, the FWS consistently assumes (or errs) in favor of killing more wolves or in favor of delisting wolves more quickly. Such a policy stance is contrary to the spirit and language of the US Endangered Species Act, the country's most popular environmental law.

I provide 136 citations to peer-reviewed scientific evidence and other scientific writings in support of remedying the above flaws.

I conclude the FWS has once again ignored or dismissed the best available science in favor of a predetermined policy outcome (delisting wolves nationwide). Until an unless FWS and other wildlife agencies recognize that BAS demands all science be sifted and winnowed for the best, regulatory changes and policies will remain unsound and flawed.

See Appendix 1 for Specific commentary on SSA by chapter and section along with citations to scientific literature.

Appendix 1. Specific commentary on SSA by Chapter and Section

(Text in plain Font are direct quotes from the FWS SSA, whereas boldface text are statements I made.)

Biology, Life History, and Ecology
"Oftentimes, breeding members can be quickly replaced from either within or outside the wolf pack, and pups can be reared by another pack member should their parents die" **Paints an overly optimistic view of wolf resilience. "Quickly" is a value judgment and vague (see below in full section). I elaborate on this problem subsequently. This leads to an incomplete statement of resilience** "Consequently, gray wolf population sustainability is a function of the productivity of the population and its proximity to other populations (Fuller et al. 2003, pp. 185–186)." **Omission of mortality rates obscures the important role of human-caused mortality HCM. A more traditional and scientific way of saying this refers to birth rates, mortality rates, and migration rates. I elaborate on this problem subsequently. This leads them to the following slanted interpretation:** "Where productivity is average to high and source populations are near, gray wolf populations can sustain higher rates of mortality than those with lower productivity." **A scientist writing with a different slant would say instead that removal of breeding wolves can lead to substantial delays in reproduction by packs and high rates of mortality will lead to more such failures in reproduction. If an isolated population is subject to high mortality, then immigration is unlikely to rescue it and the population will decline (1, 2). High rates of mortality can overwhelm reproductive success and immigration under circumstances that have been recently observed in Idaho and Montana probably and certainly in Wisconsin (3-5) withstanding an unsupported rebuttal by (6).**

Suitable wolf habitat
This paragraph perpetuates a misleading idea. The FWS treats illegal take as an immutable aspect of habitat suitability." **First that distorts the conservation biologist's understanding of suitable habitat which generally does not consider human-caused mortality as a feature of, habitats that makes them unsuitable. Rather, conservation biology treats human threats as just that: threats to be mitigated.**

The second misleading aspect of FWS' treatment of suitable habitat is the notion that illegal take cannot be reduced or prevented and therefore should be considered an attribute of habitat. Livestock are not always present and not all livestock owners kill wolves. Illegal take should be intervened and enforced against not treated as a default landscape condition. Research by our lab recommends how FWS should prevent wolf-poaching in several wolf populations (7-15).

Citation to non-peer-reviewed documents produced by interests groups are unsuitable for a government agency bound by the best available science (BAS) standard of the Endangered Species Act. For example, "We relied on the historical range map for the red wolf (Wildlife Management Institute 2016, p. 23) .." T**his is not a scholarly peer-reviewed document subject to the highest standards of peer review and scientific publishing, but instead a white paper produced by an interest group with a known mission and slant.**

Analytical Framework, p. 2

"In this document, we use the conservation biology principles of resiliency, redundancy, and representation to evaluate the current and future condition of gray wolves in the Eastern United States."

FWS' focus on the three Rs is a value judgment, not a scientific necessity. Conservation biology is a mission-driven or values-driven discipline(Soulé 1985), therefore the approaches it takes are just one value judgment among many. The 3Rs is one of many ways to assess viability of wolves (or the risk of endangerment under the ESA), not the only one. Therefore, FWS' assertion that theirs is science-based not value-based is inaccurate. They admit this themselves repeatedly by choosing whom to cite, whom to rebut and by statements such as this one: "However, understanding a species ' (inclusive of subspecies and DPSs) biological risk of extinction is necessary to determine if the species should be listed as a threatened species or endangered species under the Act, and, therefore, our analysis is focused on assessing viability.", **The decision to focus on viability is a value judgment. They are making an error exposed by the debate between Carroll and Wilhere (16-20). Note that even scientists make the error FWS is making by claiming it is using science then choosing one approach among many without justifying it in purely scientific terms (see also (21-29).**

Indeed, the choice of assessing viability has been criticized recently by (a) scientists concerned less with viability which often devolves to a minimum viable population) who are more concerned with averting relisting after delisting, and (b) by scientists who argue that the three Rs are insufficient and one must consider individual wolves, wolf packs, ecotypes, and other sub-population-level units of organization, e.g., (18, 30-33).

"Our definitions are somewhat different than those presented in Shaffer and Stein (2000, pp. 308–311) because our focus is on assessing the viability of a particular species rather than their broader focus on ecosystem function and biodiversity….Representation was originally conceived as the conservation of species within an array of different environments or ecological settings as part of conserving functioning ecosystems (Shaffer and Stein 2000, pp. 307–308). However, in the context of assessing species viability, representation in different ecological settings is a proxy for adaptive capacity (Smith et al. 2018, p. 306), which is the ability of a species to adapt to both near-term and long-term changes in its physical (climate conditions, habitat conditions, habitat structure, etc.) and biological (pathogens, competitors, predators, etc.) environments. Therefore, we define representation as the ability to adapt to new environments." **I am skeptical of this modification of the definition of representation. IT seems to allow FWS to side-step the notion that some individual wolves may carry the genetic material allowing the wolves to adapt to habitats the FWS does not want them to occupy. Take for example, agricultural areas with livestock. Many individual wolves use such areas without causing property damage or other conflicts with people (34-38). Perhaps those individual wolves should be protected precisely because they hold the adaptability the FWS claims to value, which would make the habitat suitable? If FWS only looks at where wolves are now and assumes no adaptation to future habitats is possible, then the argument is circular because the FWS is not preserving the evolutionary potential to adapt to new habitats. Also, if the FWS ignores wolves that have adapted to people, the FWS can maintain its absurd definition of habitat suitability I criticized above. Here I find another pair of value judgments by FWS, namely focusing on wolves where the FWS wants them to be and focusing on conflict not coexistence.**

Also note the they do not cite any support for the following incomplete description of resilience:
"We can best gauge resiliency by evaluating population-level characteristics such as: demography (abundance and the components of population growth rate—survival, reproduction, and migration); genetic health (effective population size and heterozygosity); connectivity (gene flow and population rescue); and habitat quantity, quality, configuration, and heterogeneity. For species prone to spatial synchrony (regionally correlated fluctuations among populations), distance between populations and

degree of spatial heterogeneity (diversity of habitat types or microclimates) are also important considerations."

Later they define genetic change (one of the factors that can lead a species to adapt to changing condition) but omit explanation of plasticity (another such factor) here "…new environmental conditions through either plasticity or genetic change (Nicotra et al. 2015, p. 1270; Beever et al. 2016, p. 132). The latter (evolution) occurs via the evolutionary processes of natural selection, gene flow, mutations, and genetic drift (Crandall et al. 2000, pp. 290–291; Zackay 2007, p. 1; Sgrò et al. 2011, p. 327)." (End of paragraph). **This omission of plasticity of behavior needs scrutiny because the FWS is not allowing for the possibility that unsuitable habitat by any definition becomes suitable in the life of an individual wolf through behavioral plasticity, making wide swathes of landscape the FWS deems unsuitable to become suitable. Allowing individual wolves with diverse behavioral traits to thrive and reproduce would be one way of advancing recovery of wolves. But they seem to turn a blind eye to individual behavior and to individuals as a whole.**

Chapter 1 Biology, Life History, and Ecology

This chapter is incomplete. It is missing use of odor cues in territorial marking and use of vocalizations for long-distance social communication; missing intelligence and cooperative breeding, cooperative territoriality, cooperative hunting.

Discussion of occurrence of wolves in the northeastern USA (NE USA) is incomplete. I highlighted this as a set of inferences that will allow more canid-killing. Namely, if one accepts the FWS implicit inference that there are no gray wolves in the NE USA (I do not accept it) then it would be OK to kill all canids encountered because they are not legally protected. if instead one admits that genetically unambiguous gray wolves are found in the NE USA or genetically ambiguous wolves are common in NE USA then states and tribes must be more careful in management to avoid killing a listed species. It would suit a predetermined goal of delisting gray wolves if there are none to protect in the NE USA and no subpopulations exist to represent greater diversity.

Here citation only to Chambers without citing NCEAS 2014 suggests that Chambers is good science. BCEAS 2013 refutes that citation to Chambers et al (39).

Definition of range of gray wolves: I believe the error here is that morphological data can be used to infer genetic composition that allows us to separate individuals into nominal species.

Here the FWS confuse hybrid zones with gradients of admixture. Basically they want to draw a dark hard line but reality does not permit that.

"We relied on the historical range map for the red wolf (Wildlife Management Institute 2016, p. 23) to determine which areas to exclude from our analysis area for the gray wolf in the Eastern United States." **Non-peer-reviewed source by an interest group with a mission. Scientifically the handling of this question is important because it affects how geographically extensive their treatment should be.**

"We assumed that gray wolves and red wolves did not co-occur within the core of red wolf's historical range (see *Interpreting Historical Range*)." **This assumption is inconsistent with the FWS' own position in this SSA regarding zones of overlap, introgression, or 'hybrid zones'.**

"Therefore, states entirely within red wolf historical range (Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Mississippi, Maryland, North Carolina, South Carolina, Tennessee, West Virginia, and Virginia) are not included in our analysis area. If only a portion of a state is included in the red wolf historical range (Illinois, Indiana, Kansas, Missouri, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, and Texas), we include the entire state in the analysis area for the gray wolf in the Eastern United States because it is likely that gray wolves and red wolves co-occurred in these areas (see *Interpreting Historical Range*)." **On its face this seems a reasonable and conservative step by including potentially more range than existed. However, recall there is little or no genetic evidence to support the exclusion of the former states.**

Second, best available science says these interbreeding canids can and do 'introgress'. Another problem is their approach demands a specified time point. I urge a more honest appraisal that they don't know which states form the boundary of gray wolf range (if any) and they are relying on the original listing because the historical range is scientifically contentious and no particular time point is specified by the word historical. O understand a federal court made this more difficult for FWS but scientifically we CANNOT know the range to study if we do not know the date to specify and we did not have genetic data from the period in question.

". These 23 states encompass the potential historical range for the gray wolf in the Eastern United States,…" **Here they mislead us. Really they are saying 'this is the range we will limit ourselves to although we have little or no evidence to support removing or adding several states. Choosing a range is a value judgment not a scientific dictate of some clear set of data. Claiming it is the best available science is misleading.**

Species Life History

Right after the first paragraph, I would have mentioned they are cooperative breeders, a rare social behavior in mammals (40), and they cooperatively defend territories from transgressions by competitors. It is important because we should understand that killing even one member of a team that cooperates in reproduction, territoriality, and hunting, can affect the function of the entire team (41-44) and for wolves see a hypothesis about team failures after lethal management leading to more livestock predation (45).

Once paired with a mate, gray wolves may produce young annually until they are over 10 years old (Fuller et al. 2003, p. 175). Litters are born from early April into May and can range from 1 to 11 pups, but generally include 5 to 6 pups (Mech 1970, pp. 118–119; Fuller et al. 2003, p. 164). Normally a gray wolf pack has a single litter annually, but two litters from different females in a single pack have been reported and, in one instance, three litters in a single pack were documented (Fuller et al. 2003, pp. 175–176; Stahler et al. 2020, p. 49; California Department of Fish and Wildlife (CDFW) 2021, p. 1). Offspring usually remain with their parents for 10 to 54 months before dispersing (Mech and Boitani 2003, pp. 11–12; Jimenez et al. 2017, p. 585. **Apparently missing two life history events: (1) young often stay with their parents in places like YNP and form multigenerational large packs where conditions allow. Dispersal is more common where there are many open territories and smaller pack sizes. (2) Also, packs often fail to reproduce in a year (80-90% succeed each year) but then many litters fail completely (no surviving pups) as pup mortality is high. Omission seems to exaggerate the productivity of wolf packs.**

"Generally, by the age of 3 years, most gray wolves will have dispersed from their natal pack to locate social openings in existing packs or to find a mate and form a new pack (Mech and Boitani 2003, pp. 11–17; Jimenez et al. 2017, p. 590)." **Where is citation to undisturbed packs in prey-rich habitats such as Smith's study in Yellowstone?**

"The innate ability of gray wolves to disperse long distances (Smith et al. 2020, p. 88) allows populations to quickly expand and recolonize vacant suitable habitats, but dispersers are subject to varied levels of human-caused mortality (e.g., Mech 1995, entire; Boyd and Pletscher 1999, entire; Treves et al. 2009, entire; Mech 2017, entire; Hendricks et al. 2019, entire) (see *Human-Caused Mortality* in Chapter 3 below for more detail)." **Of course they are subject to variable human-caused mortality. More accurately, they are subject to higher human-caused mortality than long-term residents of successfully breeding packs (Fritts and many others).**

Note here and elsewhere, the FWS parochially cites USA studies even though Canadian and European biologists have elucidated many of the same patterns of life history and after all it is the same species in those regions. Almost by definition, the best available science uses all available science and makes a scientific judgment of its utility or validity before discarding it, not an a priori judgment about whom to cite or which regions to focus on.

"Allee effects" **They cite Stenglein and van Deelen but fail to cite the correction to that paper (46) and our challenge to the completeness of the correction (47)** https://journals.plos.org/plosone/article/comment?id=10.1371/annotation/4d92a9da-dc73-41bb-ad83-837ed707c948 & https://journals.plos.org/plosone/article/comment?id=10.1371/annotation/cb45650a-9340-409e-a753-ef47579427ab**. By definition an article that was corrected is not best available science, so they would have to cite the correction and furthermore challenged articles need to be addressed as controversial, citing the rebuttals, or it is selective citation and an incomplete literature review, which is a no-no by the National Academies of science engineering, and Medicine. (2017) (48).**

" territories tending to be smaller at lower latitudes" **I was not aware of this but it makes sense based on primary productivity (vegetation) leading to higher ungulate biomass in lower latitudes, which can support denser populations of wolves even if not larger packs. More importantly, this evidence should have been incorporated into their forecast model of future population expansion. I did not see that it was.**

"In gray wolf populations, pack social structure is very adaptable. In many instances, breeding members can be quickly replaced from either within or outside the pack, and pups can be reared by another pack member should their parents die (Packard 2003, pp. 58–60; Brainerd et al. 2008, entire; Borg et al. 2015, pp. 184–185; Stahler et al. 2020, p. 49)." **Yes but they fail to mention the long delays reported by some of those same authors, which can occur when a breeding wolf is killed. Their handling is superficial (as befits their predetermined goal of asserting that wolves Are resilient to high levels of human-caused mortality) as they quickly move on to the claim without explaining that common conditions in the lower 48 will slow down the resumption of reproduction after a breeder dies. The common conditions include high levels of human-caused mortality, isolation, small packs sizes, sparse wolf populations, etc. each of which reduce reproductive rates or immigration rates**

"Consequently, gray wolf populations can rapidly overcome severe disruptions, such as intensive human-caused mortality or disease. The likelihood a pack will maintain its territory declines if both breeders are

killed; however, if one member of the breeding pair is killed, the pack may hold its territory until a new, unrelated individual arrives to replace the lost breeder" **Note lack of citation to "rapidly overcome" — what does that mean? One actually has to show that the time to next breeding is the same or close to the same AND that pups survive at similar or higher rates, otherwise high human-caused mortality was not "overcome".**

Here the FWS chooses to minimize harms, simply asserting that wolves can take it in most cases. They rely too heavily on data from Alaska when the data from Brainerd et al. that is NOT from Alaska paints a less rosy picture. Alaska differs here in the key role of compensatory immigration (unhinged packs and vast wild range of wolves allows for frequent immigration into a pack that has lost a breeder). The more appropriate comparison are packs in the lower 48. Cassidy's data and some of Brainerd's contributors — the FWS should have studied those packs and summarized those Datta particularly carefully and then noted they do not know anything about pack dissolution and breeding success in the southern gray wolf packs of Colorado California, maybe even southern Wisconsin.

Footnote 4 is a very incomplete literature review of density-dependence because evidence is mixed and controversies are still strong. And they perpetuate the incomplete lit review by dismissing or not summarizing studies of density-dependence in the following paragraph (hence not best available science):

"… gray wolf populations, the density of gray wolves on the landscape can impact specific vital rates such as adult survival, natality rates5 and recruitment, and dispersal. These vital rates can directly influence population growth and the ability for a population to recolonize vacant habitats or respond to population declines. In general, when suitable habitat is available (see *Suitable Habitat*), these vital rates are positively influenced by low gray wolf densities, which ultimately results in relatively rapid population growth and expansion. As gray wolf abundance and distribution increases and they begin to occupy most of the available suitable habitat in an area, the population growth rate declines. Examples of this density dependent relationship with
population growth can be seen in the Northern Rocky Mountains (NRM) and the Western Great Lakes gray wolf populations (Service et al. 2016, Figure 7a; Service 2020, pp. 15–24). High gray wolf densities have negative effects on adult survival (Murray et al. 2010, entire; Gude et al. 2012, pp. 112–115; Cubaynes et al. 2014, pp. 5–11; O'Neil et al. 2017, pp. 9524–9528); natality rates and recruitment (Gude et al. 2012, pp. 112–115; Stahler et al. 2013, pp. 222, 232; Schmidt et al. 2017, pp. 18, 25); and dispersal distance, rate of dispersal, and age of dispersal (Jimenez et al. 2017, pp. 5–12; Sells et al. 2022a, pp. 7–12). Conversely, when gray wolf densities decline and suitable habitat remains available, any or all of the above vital rates may be positively affected (Stahler et al. 2013, pp. 226–231; Cubaynes et al. 2014, pp. 5–11; Jimenez et al. 2017, pp. 5–12; Schmidt et al. 2017, p. 25; Smith et al. 2020, pp. 77–92), thus, providing opportunity for increased population growth." **But they do not cite the debate over density-dependence in Wisconsin (49-53) withstanding the data-free rebuttals by (3-6, 13, 14, 22, 28, 46, 47, 54-65).**

Again these omissions are misleading and violate NAS 2017 guidelines on research integrity. Selective citation is an example of a breach of scientific integrity because it paints a slanted picture of what we know and don't know, what scientists are debating, and presents false confidence in the studies cited.

I reached the end of the section that might discuss cooperative territoriality and breeding. Although they alluded to both they did not address the most important features if one wishes to evaluate the effect of human-caused mortality as follows:

First, a team functions as a coordinated unit, wolf packs are families with trusting, long-term social relationships based off familiarity, experience together, and a close working relationship based on trust, experience, familiarity, and practice. Therefore, death of one or more members of the team is known to affect its success in breeding, territoriality, and hunting (citations act end of this paragraph). (2) individuals are not necessarily substitutable for each other. There is role specialization and some outstanding individuals will be responsible for most pup care, territorial patrolling and defense, or hunting success. FWS largely ignores these individual differences in wolf capabilities. In addition to some evidence for individual differences in behavior and cognitive capabilities wolves and dogs (66), consider scientific literature on individual differences from other species including carnivores Loss of Earth's old, wise, and large animals(41-44, 67-72).

A consequence of ignoring individual wolf capabilities and role specialization is to minimize the harms that can occur when mortality increases due to management interventions and policies. FWS paints a picture that the fate of wolf populations is restricted to the success of breeding and the growth of the population. While those are important, so too are the survival and success of experienced, older wolves, the success and persistence of large packs that bred well in the past versus small packs struggling to make it in marginal habitat or struggling because of inexperienced adults (citations in prior paragraph). By devaluing individual wolves and focusing on packs somewhat and populations almost exclusively, the FWS repeats an error that scores of articles have pointed out in the last decades. For example, not all individual wolves cause problems with domestic animals or threaten people. Packs that dissolve or are shot out often lead to increase sin domestic animal losses. Understanding why requires understanding individual differences and intra- and inter-pack dynamics, which the FWS has largely left out or ignored (see citations above to Wydeven et al. 2004l; Chavez and Gese 20025, Santiago-Ávila et al. 2018).

Suitable Habitat

"These models have shown the presence of gray wolves is positively correlated with prey density and natural and forest cover; and is negatively correlated with high road density, high human density, presence of agricultural land or ranching activities, and other human disturbances on the landscape, such as land conversion and percent impervious surface" **Naturally correlations need not imply causation. Rather than focusing on biological mechanisms, the FWS relies on expert opinion and correlation which can only be based on where wolves were seen in recent years. By contrast, the mechanistic causal explanations are that gray wolves are generalists and can therefore thrive wherever birth rate exceeds mortality rate on average. Humans cause most mortality (14, 73), so wolves will be excluded from breeding in places where humans kill too many of them. Human-caused killing is illegal take for endangered gray wolves so it is a threat that should be abated not accepted. It is extrinsic whereas intrinsic habitat suitability should not include human-caused mortality that FWS should be stopping. Or put another way, the ESA envisions recovery of wolves wherever they can survive. They can survive around people if people don't kill them (illegally moreover). Data from Italy, Spain, Poland, and most of Europe prove this point definitively, but FWS does not cite much if any of these data (74-77).**

Moreover, the correlates of human-caused mortality above are characteristic of intolerant rural regions of wolf range NOT characteristic of all high-density road areas (witness the road study by

Kohn et al. 2009 or military bases with little traffic), or even all rural human populations, witness Ojibwe and the many livestock owners who do not hate wolves (13, 78-81). They acknowledge some of these points but not all but their literature review is incomplete and outdated.

"Thus, in this SSA Report, we consider suitable habitat to be areas containing adequate wild ungulate populations (e.g., elk and deer) and a low risk of conflict with humans (e.g., low road density, low human density, adequate natural cover without agricultural land or domestic animal ranching)" **Here it is explicit that their entire definition of suitable habitat surrenders to human-caused mortality rather than abating it.**

Also note any citation to Gude et al. 2012 should include the correction to its math errors (Vucetich 2012 cited above) and generally subsequent papers indicating they over-estimated the sustainable rate of human-caused mortality by a large margin.

Chap 2
Demographic Needs: "remarkably resilient"
Again "remarkably resilient" is a value judgment not a scientific assessment. The extirpation of wolves reminds us of the limits to resilience (see Chapter 6 also)

"\…gray wolves may be regulated by density-dependent, intrinsic mechanisms when ungulate densities are high and human-caused mortality is low (Van Deelen 2009" **here they cite a chapter (not strictly speaking anonymously peer-reviewed by independent reviewers either) which advocates for "maximum sustained yield" of wolves, which has been challenged on both scientific grounds and on muddling facts and values and for assuming density-dependence despite a lack of evidence (22).**

See Wisconsin in 2021 and the possible failure of a majority of packs to breed after the February 2021 wolf-hunt (3). FWS should acknowledge that they do not know what happens to a wolf population where all but a few wolf packs fail to breed.

"In wolf populations, each pack occupies and secures a discrete area with access to a finite amount of food resources, which influences population size " **Strictly speaking this is not correct or well referenced. It is not correct because discrete means exclusive and reams of articles they cite note how transients cross territories and neighboring packs share some areas of overlap (82-84)—logically that is why intraspecific killing happens at all.**

Above is also an incomplete literature review because (a) theory and evidence of territorial species shows that territoriality breaks down under two conditions (very high density and very low density). At very high density it is too energetically expensive and risky to defend against all the neighbors and interlopers. At very low densities there is little point to intense patrolling and defense of the territory because interlopers are so few. This is an example of FWS just citing Mech and not going any further even though his view is just one view. FWS should have cited others studying wolves and gone beyond wolf literature to discuss the implications of high mortality for territorial behavior (85, 86).

" low-density populations or in areas where gray wolves are recolonizing, new breeding pairs are more easily able to establish territories (Packard and Mech 1980) " **Although I agree with this statement, the source is 45 years old so not surprisingly it is incomplete, i.e., we know more about this now. Also and perhaps more importantly, the statement above contradicts their definition of suitable habitat.**

If suitable habitat has enough prey and low human-caused mortality then the statement about lower density is a missing variable in their definition of suitable habitat. They are being inconsistent and arbitrary in choosing their variables.

“ multiple breeding females within a pack are more common, leading to a higher potential reproductive rate than packs with a single breeding female (see *Species Life History*).” **Although I agree, they erroneously link this to population regulation. Only if secondary litters survive can they affect population regulation. Another way of saying this is that almost no one measures wolf birth rate (I.e., pups born) because it is so hard to measure and pup survival is so low. At most researchers measure pups at the den when they emerge an very rarely pups alive in the den before emergence. In short, the presence of a second female giving birth has a negligible effect on the population unless the pups survive to autumn.**

This statement is inaccurate “Adult gray wolf survival rates typically decrease as densities increase (density-dependent intrinsic population regulation), whereas recruitment appears to be 26more dependent on food availability (extrinsic regulation) (Stenglein et al. 2018, p. 13; Smith et al. 2020, p. 91).” **First Stenglein did not find density-dependent survival and we challenged her claims of finding density-dependent reproduction because it is not base don reproduction but a poor correlate of it (see citations above to the copious scientific debate in Wisconsin). Even worse they side-step the debate over density-dependent survival, failing to cite the Wisconsin debate accurately or completely. While that debate is unsettled, there is bad, good, and better evidence and some sort of consensus emerging.**

“Pack size and composition can also play a role in population regulation because smaller packs have fewer individuals to assist with food provisioning for pups, to compete with adjacent packs for food, and to support the minimum pack size necessary for recruitment (Stahler et al. 2013, pp. 226-231)” **I agree with this but they fail to see the link to human-caused mortality. The spatial distribution of human-caused mortality (across many packs or concentrated on a few packs) will tend to have different effects on population growth and stability. Distributing HCM across many packs will reduce the size and success of many packs. They fail to cite Santiago-Ávila et al. (2018) for pointing out that lethal control of wolves suspected of killing domestic animals leads to some increase in risk for neighboring livestock, possibly because of this failure of smaller packs. That in turn can lead to more lethal control in an accelerating death spiral.**

“At larger pack sizes, intra-pack competition for food and socially-induced stress from competitors during the breeding season can impact maternal condition, resulting in smaller litter sizes;…” **This claim is not adequately substantiated.**

Note that ending this sub-section without mentioning how HCM affects pack size and reproductive success is a major oversight. Specifically, FWS assumes HCM is the same as any cause of mortality. We do not know this.

Also FWS selectively cited some experts and does not acknowledge ongoing controversies among scientists. Furthermore when choosing who to cite they preferentially cite Mech and others in non-peer-reviewed book chapters not in the strongest peer-reviewed journals, thereby breaching research integrity and showing a preference for certain results regardless of the methods or quality and age of the research.

In the section "Connectivity and Genetic Diversity" they cite a Fennoscandinavian study rather than the more US-centric study by vonHoldt et al. that came out last year. Why? Do they like the number 500 better than the high numbers vonHoldt and colleagues estimated?

"Gray wolves in the Western Great Lakes metapopulation are well connected7 to each other…" **value judgment muddled with scientific evidence. Then** "…**and also linked to populations in Canada". Here's an example of citing the inferior sources. (12) presented all data and even led to the correction of the study FWS does cite (Stenglein et al. 2015). Yet FWS ignores (12) and make errors as a result. For example, (12) reported WIDNR data found one wolf (or maybe two wolves) went to Canada and none were known to come from Canada in a 30+ year period. Minnesota surely has more but I am not aware of data to that effect. MIDNR may have 1 or 2 known wolves from Canada or going to Canada? Also the wolves that migrate to and from Canada have to reproduce successfully if this connectivity is meaningful. Then they assert it is true without evidence** "This connectivity, as noted in a number of PVAs we discuss below, allows for adequate retention of genetic diversity at lower population sizes than theoretical estimates or general guidelines would recommend (e.g., than the 50/500 rule discussed above)." **Note how they cite the rule not the evidence that is sufficient to offset small isolated populations' losses of diversity.**

I'm curious about how they cite some assertions of fact. Witness "Generally speaking, connectivity or effective dispersal between populations or subpopulations is a critical component in the maintenance of genetic diversity in gray wolf populations (Wayne and Hedrick 2011, entire; Räikkönen et al. 2013, entire; Carroll et al. 2014, pp. 81–82). " **Here they claim the entire article for two teams is relevant (I doubt it) but only a few pages of another author (which I also doubt). Dr. Carlos Carroll might take exception.**

Also witness "A study of the Scandinavian gray wolf population noted that connectivity was, in fact, more important to the retention of genetic diversity within a population than the population's size (Liberg and Sand 2012, p. 12). Such connectivity facilitates the retention of genetic diversity within subpopulations and in the larger metapopulation." **Neither of those authors is a geneticist.**

Overall this section is marred by a shortage of data on actual migration between isolated sub-pops. of meta populations. Where are the data? By ignoring specifics, FWS can say something sweeping such as even one wolf from Canada per generation can rescue a small isolated population from genetic harms.

Representation

I don't believe their interpretation of representation is correct, see my intro. In general, the tone and slant of their writing is to emphasize how wolves can survive high mortality rather than impartially assessing the current representation of the range of adaptability.

"In our analysis area, gray wolves only occur in the Western Great Lakes area, which represents a fraction of the historical range of the species. However, given that this is the only extant population, our discussion of adaptive capacity will only focus on attributes of this population, as only an existing population can contribute to the species 'ability to adapt. Many attributes of gray wolves in the Western Great Lakes area, including a wide distribution, high capacity for dispersal and colonization, high genetic diversity, and a generalist life history, are all positively correlated with adaptive capacity" **Seems like a logical flaw: OK first there has been reported evidence of gray wolves in the NE USA so ignoring it doesn't make it go away. Logically if gray wolves made it into NY and other NE states they have**

some data to contribute to this issue. Second, the only extant gray wolves in the NE USA does not mean the only relevant data. Now where are the Fennoscandinavian studies and the rest of Europe? All wolves can inform us — because it is not a question of geographic uniqueness but rather habitat characteristics.

"… characterize the likelihood that gray wolves in the Western Great Lakes area will be able to adapt to a range of environmental changes" **So they aren't habitat generalists? inconsistent and capricious.**

"Additionally, greater numbers of packs and breeding pairs spread across the range in the Eastern United States further enhances redundancy. For example, our recovery criteria for the gray wolf in the Eastern United States includes ensuring the survival of the existing population in Minnesota and reestablishing at least one viable population outside of Minnesota" **Since Mladenoff et al. 1997 and perhaps earlier the WGL is considered one population so why are they switching terminology here? This is inconsistent. MN is not a population anymore than the UP of MI is by their definition At best it is one bit of a meta population. Seems arbitrary to focus on MN and relegate all other parts of its meta population to whatever fate, which we know includes risky levels of HCM in Wisconsin. Predetermined conclusion that the NE USA will be dismissed forever after.** "In general, gray wolves in the Eastern United States need multiple, resilient subpopulations with multiple packs distributed across a broad enough area of the Eastern United States to reduce the potential impact of catastrophic events on the species 'extinction risk." **I do agree with that but I wonder if they are going to foster recolonization of areas outside the WGL or just rely on their unscientific and probably unlawful definition of habitat suitability and taxonomy to rule out additional recolonization?**

"For example, a PVA for gray wolves in Wisconsin found a completely isolated population of 300 to 500 individuals would have a high probability of persisting for 100 years under all scenarios evaluated (Rolley et al. 1999, p. 43; WI DNR 2006a, pp. 7–8). " **I realize it's just an example but to be even-handed they should have noted a lot of new data have accumulated since these were run so they should be done again. Citing the old numbers is simply misleading.**

Managing at a hypothetical "cultural carrying capacity"8 of 300 gray wolves instead of allowing the population to reach the "biological carrying capacity"9 of 500 gray wolves had little effect on the estimated risk of extinction in Wisconsin" **out of date and wrong-headed because the definition of cultural carrying capacity is not clear and highly contentious without empirical support. Also FWS is not citing the best studies of this question anyway. "**here "8 Cultural carrying capacity is the maximum population size tolerated by a given community's social and cultural norms." **Note lack of citation because this is nonsense and rejected by serious social scientists such as Bruskotter and Naughton. It is crap because many communities exist not just one and they all differ in their aggregate of individual tolerances.**

Moreover human tolerance is not a landscape feature but a potential threat for the FWS to work on. FWS cites "(Rolley et al. 1999, pp. 42–43; WI DNR 2006a, pp. 7–8)." **But these are not peer-reviewed, not social scientific experts and not up to date.**

The following sentences about red wolves and Mexican gray wolves seem to come out of nowhere as if the low numbers for viability can be applied to gray wolves simply because FWS reports those data? This is inconsistent with their own claims that eastern gray wolves should be considered based on current WGL gray wolves.

The next paragraph reports on Petracca's analyses for Washington claiming they successfully refuted (Santiago-Avila et al. 2024) which they did not; also not mentioning Treves 2023 pre-print (30)challenging Petracca et al. 2023 in pre-print also. This is another example of where their preferred science is challenged and they summarily dismiss criticism without accurately summarizing the scientific debate because they like the results of one study.

Citing the Swedish Environmental Agency is not a citation to a peer-reviewed study but instead a citation to a source that is subject to intense pressure from interest groups that wish to hunt wolves.

Chapter 3: Conservation Efforts and Stressors

Opening paragraph about stressors: missing small pack size resulting in lower reproductive rate per pack. Also note the entire section is unreferenced, meaning they have not apparently consulted the latest conservation science for how to measure success or how to define and study threats? (87-91)

They do not mention adequacy of regulatory mechanisms explicitly so mapping onto the 5-factor analysis is unclear.

Figure 7
(I offer the below critique because a conceptual model like theirs reveals how they think about cause and effect. My analysis suggests their thinking about cause and effect is muddled, unjustified by evidence, and includes some no-nos as far as conceptual models go.)

Figure 7 deviates from all the literature I know in defining human-caused mortality (HCM) as legal methods. Although they write e.g., before 3 legal methods, by not mentioning illegal HCM they are minimizing the role of the most frequent HCM (14).

Also they do not connect tribal management to HCM for no apparent reason (arbitrary).

Although they acknowledge the effect of state and federal management on HCM can go either way they do not explain the relationship.

Furthermore Figure 7 includes "Other sources of habitat modifications**" with no connecting arrows back to government management. Would they like to sweep illegal take in with other private actions here? I surmise this is where they would also place private hounding or trapping of other species (non-permitted or permitted for hunting another species) that harms wolves. By not connecting ANY arrows back to government management this category is distinct from their narrow definition of HCM. The justification for treating private illegal kill as separate from HCM is not explained.**

Nor do they note or cite our work showing that cryptic poaching is different from reported poaching in pattern and process (Santiago-Avila, Louchouarn, Agan cited previously).

I want to re-emphasize the point about illegal HCM. By apparently omitting it, they dismiss and ignore the positive correlations we have found between legal HCM and illegal HCM.

They also fail to identify human tolerance as a cause of HCM (both legal and illegal), so the left side of Figure 7 is an incomplete picture of the causes of wolf resilience.

Figure 7 is also inconsistent with their other writings in two ways. First, they ignore the component of suitable habitat that relates to density of other wolves (Chap 2). Second, they mention "suitable habitat' in a pink box which they had previously defined as abundant prey and low HCM; but then they add another box of "abundance of prey species"**. Because their definition of suitable habitat already includes prey and HCM, Figure 7 duplicates those two variables inflating their importance without justification .**

abundance of prey species" **(which by the way should have been prey abundance to avoid confusing the issue that maybe wolves need diverse species of prey).**

Finally, Figure 7 "Demographic needs" ignores density of wolves around packs, and blurs the difference between number of wolves in a pack (a correlate of pack success) and number of wolves in the population. Their box labeling number of wolves is imprecise and unclear.

In sum, this schematic omits information, duplicates variables such as HCM and prey availability which is considered poor practice for conceptual models. This sort of muddy thinking does not clarify their view of resilience but instead muddies it.

**** end of critique of Figure 7****

I skipped MI and MN state management to proceed to WI state management below.

"…however, Wisconsin Statute Section 29.185 requires the Wisconsin DNR to allow hunting and trapping when gray wolves are not listed on Federal or state threatened and endangered species lists (WI DNR 2023a, p. 75)." **FWS does not mention a state court decision stating that setting a quota of zero is allowed under statute.**

Respondents "would prefer "about the same number" of gray wolves or more in the state; the population size was approximately 1,000 gray wolves at the time of this public attitudes survey (Bradshaw et al. 2022, pp. 41–42; WI DNR 2023a, pp. 127–129)." **Here they do not cite our work showing that at every populations size when such a question was asked, the majority prefer the middle of the range of values which is usually near the current population size estimate (80). In short, surveys of current adult preferences do not reveal much as they rise with the growing wolf population and simply reflect a central tendency in the survey sample. If the sample is representative then maybe it represents current adult residents but if not then it is not even informative for the central tendency.**

"Wildlife Services for timely and effective gray wolf conflict assistance; ensuring funding for the gray wolf conflict program; continuing research on conflict mitigation, prevention and new techniques for addressing conflicts; and increasing public awareness of the gray wolf conflict program and abatement techniques" **Just a note for the future that the goal includes "effective". If that is defined as we have done so in the scientific literature then it should reduce statewide conflicts with wolves.**

While FWS might be faithful to state management it does not address the scientific validity of state policies for management interventions such as lethal control or harvest to manage conflicts.

Therefore, the FWS seems to be on thin ice if challenged on the adequacy of regulatory mechanisms under the ESA.

Also, regarding best available science, the FWS blandly notes that a wolf advisory committee will evaluate data to advise policy without noting that scientists are not included on that Wisconsin WAC unless they are part of a favored interest group too.

The paragraph on enforcement against illegal HCM hardly includes arrest and prosecution, if at all but does mention voluntary compliance.

In the section on federal lands, FWS abdicates responsibility for addressing HCM to states. See (92) on whether this is a lawful delegation to states when discussing federal parcels.

Also what about ceded lands under treaty between feds and tribes? Why are tribes not mentioned in this section when federal law requires USDOJ to act as trustee for tribes? Tribes come last. I see little or no mention of co-sovereign management of ceded territories after delisting.

Forest Service, BLM, FWS, and NPS manage parcels and defer to states on hunting and trapping. What about hounding? What about incidental take during ESA protection? Has federal management been adequate thus far? Will it continue to be adequate after delisting?

Also there is the elephant in the room about whether protective regulations are enforced or just on paper.

NPS is different FROM the above three federal agencies. FWS offers no estimate of wolf numbers on Voyageurs NP. Why not? Isn't that a cause for concern about fundamental uncertainty? Other NP parcels are apparently open to state-managed hunting of wolves.

"Stressors
Human-Caused Mortality
Causes of gray wolf mortality can be separated into two broad categories that include natural causes (e.g., intraspecific strife, disease, starvation, and accidents) and anthropogenic causes, or "human-caused mortality" (e.g., harvest, lethal control, illegal take, vehicle strikes, and human- caused accidental mortalities)." **Note inconsistency with Figure 7.**

"Unregulated hunting and trapping, the excessive use of poison, and the activities of government trappers eradicated gray wolves across much of their historical range in the lower-48 United States in the early 1900s" **Here an impartial summary would ask which scientific studies suggest unregulated killing continues today (Treves et al. 2017a,b,2021). In the above they downplay that illegal killing is the major component of HCM during ESA protection.(Treves et al. 2017). All these were cited above.**

"Effects of Human-Caused Mortality
Effects on Population Growth" **This is a leap without justification. Mortality affects individual wolves, which in turn, can influence pack success, which in turn can influence metapopulation unit persistence and viability, which only then affects entire population dynamics. By skipping the intermediate steps they are dismissing and ignoring a large set of literature that addresses pack failure to reproduce or dissolution and its negative consequences, metapopulation persistence, etc,**

under different rates of mortality. Jumping to the population-level is a sleight of hand that protects the FWS from (a) citing literature they do not prefer and admitting to uncertainty where we have little data, or (b) addressing controversies over HCM's long-term effects.

"The effects of increased mortality on a population can be described as compensatory or additive" **incomplete as three independent teams have discussed super-additive or depensatory mortality (93, 94) and cited previously: ; Vucetich 2012; Chapron & Treves 2016a,b,2017a,b0**

Many wildlife populations can compensate for changing levels and types of mortality up to a certain point; after this point, mortality becomes additive and survival begins to decline. Gray wolves are no exception. As described in *Species Life History* in Chapter 1, density dependence and its effect on certain life history characteristics plays a large role in the ability of gray wolves to compensate for increased human-caused mortality. Although debate continues about which is most important, the three primary mechanisms with which gray wolf populations may compensate for increased human-caused mortality include a reduction in natural mortality…" **While correct, the emphasis on compensatory life history traits contrasts with the bulk of the literature that they do not cite which emphasizes super-additive or depensatory responses to increased HCM.**

"Some researchers have even indicated that increased levels of human-caused mortality may be super-additive through the loss of dependent offspring or future reproductive output (Creel and Rotella 2010, pp. 3–6); however, other researchers have challenged this finding (Gude et al. 2012, pp. 113–116) r noted that evidence for super-additive effects was weak (Horne et al. 2019, pp. 40–41)." **misleading summary. Again I find the bulk of the literature favors super-additive effects (see above). Also Gude et al. 2012 has been debunked for errors in calculations and never corrected (Vucetich 2012).**

"In Wisconsin, human-caused mortality was found to be additive during recolonization then transitioned to compensatory as the gray wolf population grew and expanded (Stenglein et al. 2018, entire)." **Failure to cite our challenges to Stenglein et al. 2018. They also omit the correlations we have repeatedly reported between legal HCM and illegal HCM (refs above to Santiago-Avila et al and Treves et al. for Wisconsin wolf mortality). They also cite Chakrabarti et al. 2022 without mentioning it was challenged and found faulty on statistical grounds by (95).**

"There is considerable research and continued debate surrounding the level of human-caused mortality for which gray wolf populations can compensate and maintain population stability. Dependent on the analysis, researchers estimate that human-caused mortality rates between 17 and 48 percent result in gray wolf population stability (Fuller 1989, pp. 24–25, 34; Fuller et al. 2003, pp. 182–186; Adams et al. 2008, pp. 18–21; Creel and Rotella 2010, pp. 3–6; Gude et al. 2012, pp. 112–113; Vucetich and Carroll 2012, entire; ODFW 2015, p. 31; Hebblewhite and Whittington 2020, pp. 7–8)." **Again they ignore the error in Gude et al. 2012 reported by Vucetich 2012 in an FWS report on Wyoming. Gude et al. provides the high (too high) estimate of 48%.**

"An analysis of mortality rates and population growth, reported from studies conducted across North America over an approximate 35-year period, indicates that gray wolves are able to compensate for annual rates of human-caused mortality up to approximately 29 percent of the known or estimated population (Adams et al. 2008, pp. 18–21)." **I believe the consensus is that Adams et al.'s estimate of 29% is the accepted upper limit when the wolves suffering that rate are supplemented by compensatory immigration from unexploited adjacent subpopulations. I'm struck that whenever there is a low estimate of sustainable HCM or a high estimate of HCM, the FWS throws in a**

'however' and casts shade on the results they do not like. But when a study says something they do like (high HCM is sustainable or HCM is low) they do not look further to all the challenges to that preferred result. Another case in point is a caveat that 29% may be too high, which FWS ends with Gude et al.'s flawed study debunked by Vucetich 2012.

"However, many of the studies reviewed to estimate this rate were based on autumn/winter minimum gray wolf population counts (Adams et al. 2008, pp. 18–21). Therefore, given that minimum counts likely underestimate true population size, the actual rate of mortality that allows for population stability may be lower than 29 percent. Some have posited that because growth rates used to estimate this gray wolf population stability threshold were obtained from a relatively small sample of the larger studied population, extrapolation to the larger population is questionable (Morales-González et al. 2022, pp. 471–472). These researchers cite an earlier study that suggested a reduction in wolf population growth rates across all levels of human-caused mortality (Creel and Rotella 2010, pp. 4-6; Morales-González et al. 2022, p. 472) to support their position, but fail to consider another study that challenged those findings and demonstrated that the inclusion of recruitment and human-caused mortality data better explained variation in wolf population growth (Gude et al. 2012, pp. 112–116)." **Again Gude et al. 2012 an error-filled analysis rides to the FWS rescue.**

"Effects on wolf dispersal"
Note misuse of harvest when they mean HCM, indicating where their heads are and should not be as the trustee of the ESA.

Overall omission of summary of evidence on illegal HCM. Also, overall, a positive slant on harvest without scrutiny of whether it is effectively regulated.

"Effects on Gray Wolf Social Structure
Although gray wolf populations typically have a high rate of natural turnover (Mech 2006, p. 1482)," T**his should be scrutinized as not recent and how he defines natural turnover. If he is referring to lower 48 wolves in non-protected areas then natural turnover could refer to wolves being heavily exploited by legal and illegal HCM. What about Yellowstone? Doug Smith's data suggest not much natural turnover.** "increased human-caused mortality, primarily through regulated harvest, may negatively 65affect the dynamics and social structure of packs" **why focus on regulated harvest here and not illegal HCM? Again they follow the structure of "Some are concerned but nothing to worry about because others do not" Also by breaking this section into two parts (pack dissolution, persistence) and a second part (reproductive effects) which makes it seem as if these are unrelated. That is misleading.**

"In some gray wolf populations that are at or near carrying capacity, where breeder replacement and subsequent reproduction occurs relatively quickly, population growth rate and pack distribution and occupancy are largely unaffected by the loss of one or both breeders" **Yes but the small recolonizing populations likely in the future for the eastern USA is exactly the problem to address, rather than emphasizing huge populations with large packs sizes as in Canada and Alaska.**

"Breeder loss can and will occur in the future to some degree, regardless of the presence of human-caused mortality, and that the loss of any individual will have some effect on pack dynamics. However, the effects of this breeder loss on the metapopulation of gray wolves in the Eastern United States is likely to be minimal, as long as a sufficiently large population is maintained that is well-connected to other populations via dispersal." **Do they think Canada will provide that for the WGL? If so where are the**

data showing one Canadian wolf per generation is entering MN and MI? If they mean Canada will provide compensatory immigration into the NE USA, where is the evidence? If they mean MN will provide all of the eastern USA with migrants, where is the evidence? We found only one MN collared wolf in the Wisconsin death sample over 30 years! (12).

"Boonstra (2012, entire) concluded that chronic stress in wildlife was rare, but that it could benefit the affected species by allowing it to adapt to changing conditions to maintain, or improve, long-term fitness. Indeed, Bryan et al. (2015, p. 351) argued that the physiological changes observed in the stressed wolf population could be considered adaptive and beneficial to the gray wolf when dealing with the specific stressors." **Seemingly FWS wants us to believe stressed wolves are better than unstressed ones!**

"This indicates that the overall genetic diversity of a wolf population may be resilient to human- caused mortality, assuming they do not exceed thresholds that result in precipitous declines" **I would dispute this claim because it is overall HCM — not the reported HCM that they measure — which is needed to draw this inference. And because they are systematically dismissing and ignoring illegal HCM, they do not estimate total HCM as we have done (3, 4, 63). Hence the inference is dubious.**

"Sources of Human-Caused Mortality
Human-caused mortality includes both controllable and uncontrollable sources of mortality. Controllable sources of mortality are discretionary (i.e., they can be regulated by the managing agency) and include permitted take, legal harvest, and direct agency control. Sources of mortality that are difficult to regulate and occur regardless of population size include natural mortalities, illegal take (which we define as illegal killing of wolves, i.e., poaching), and accidental deaths (e.g., vehicle collisions, capture-related mortalities). Below, we provide a brief discussion of the forms of human-caused mortality that have been documented in the Eastern United States" **Why is it OK for them to throw up their hands and claim illegal HCM cannot be regulated? Complete absence of summary of our work on poaching moreover.**

"Lethal Control of Depredating Wolves
There are certain circumstances in which preventative and nonlethal techniques have been shown to be effective for reducing wolf depredation of livestock. These include proactive methods to prevent wolves from acquiring food rewards to curb learned behaviors (Much et al. 2018, p. 76); the inferred effectiveness of human presence at reducing recurrent depredations (Harper et al. 2008, pp. 782–783); the use of predator-proof fencing where resident wolf packs occur (Mayer et al. 2022, pp. 8–11); and the adaptive use of multiple preventative and nonlethal methods to minimize sheep (Ovis aries) depredations (Stone et al. 2017, entire). There are also circumstances in which lethal control has been shown to be effective at preventing future depredation events. Lethal control of depredating wolves is used reactively rather than proactively, often after other, nonlethal techniques to prevent depredations were unsuccessful (Bangs et al. 2009, p. 110), but it may improve the overall effectiveness of nonlethal methods because wolves may then associate humans with an increased risk of injury or death (Meuret et al. 2020, pp. 1, 408–411)." **The latter article mistakenly in my opinion included studies that do not address the question (e.g., perceived effectiveness and self-reported data by livestock owners; moreover the study is superseded by more recent work that they should cite here: (96-99) Here again they cote a study with a result they like even though it does not say exactly what they claim, while ignoring more recent, more complete studies that do not agree with FWS preferences (i.e., killing wolves does not protect domestic animals (28, 98).**

"Targeted lethal removals may be effective at resolving conflict because a relatively high proportion of depredations in any given year occur over a relatively small area and involve a relatively small number of wolves (Olson et al. 2015, entire; DeCesare et al. 2018, pp. 9–11). Although incremental removal of a few individual depredating wolves shortly after a depredation occurred (i.e., within seven days) reduced the potential for depredations to continue, full pack removal had a more immediate and longer-lasting effect on the frequency of recurrent depredations (Bradley et al. 2015 pp. 6–9). The targeted removal of at least one adult male wolf from depredating packs (Harper et al. 2008, pp. 781–783) and the targeted removal of a high number of individuals relative to pack size significantly reduced the probability of recurrent cattle (Bos taurus) depredations the following year (DeCesare et al. 2018, pp. 8, 10–11) in studies completed in Minnesota and Montana, respectively" **Decesare is very crude (county level) and does not verify any of the data on alleged wolf predation on domestic animals or address prior critiques including my own (Treves et al. 2016 critique of correlations and of Weilgus and Peebles, Poudyal, oKompanyets or Bradley et al. In short by ignorign prior work Decesare seems to present a new better analysis but instead just presents another variant rehash of the same incomplete literature.**

Missing so many articles on non-lethal methods. Also citation to studies that have been challenged as irreproducible and unscientific e.g., Harper et al. 2008 and Bradley et al. 2015. The lit review for this section is out of date, stopping at about 2020. They do not cite 6 studies of killing wolves to protect domestic animals that found mixed effects, most often no effect, and recurrent counter-productive increase sin livestock losses (K(100-106) and of course the best US study of all by Santiago-Avila et al 2018 which they cite but do not summarize fairly.

FWS is missing about 30 studies of non-lethal methods that suggest effectiveness to prevent wolf attack on domestic animals. Also those studies use stronger inference (better methods) than the studies of lethal intervention. Here's a partial list (104, 107-120)

"There is some evidence that the combination of targeted lethal control of depredating wolves and regulated harvest of wolves has the potential to reduce wolf-livestock conflicts without having a significant impact on wolf abundance." **This followed by a cherry-picked Wisconsin example is pure myth. See if you can find methods, See if they address illegal killing, methods for measuring wolf-killing, verifying losses were caused by wolves, etc.**

"Illegal Take (i.e., Poaching) of Wolves" **ok finally…Here they repeat the challenged findings of studies that did not analyze disappearance of collared animals an endpoint alongside known causes of death. FWS does not adequately summarize the scientific debate, siding with those who claim it is lower than we do but never present the data. Our critics claim all their collars failed but do not present evidence. When confronted with the high rate of disappearances of collared wolves they claim something is different about wolves or their collars, without proof. When we asked for evidence they go silent. See refs to Santiago-Ávila, Louchouarn, Agan and replies by Roberts and Breck. Same problem with Liberg's 2020 paper for Scandinavia which we rebutted (121, 122).**

"However, it is not reasonable to assume that all, or even most, wolves with unknown fates have died, particularly through illegal means, because radio-collared wolves may go missing for a variety of reasons (e.g., collar failures, end of useful battery life, wolves moving out of monitoring range)" **It is reasonable enough to pass peer review in better journals than the studies they prefer. Indeed we addressed each of those innocuous reasons in great detail so the FWS is simply cherry picking arguments by our detractors which have NO DATA behind them. We have data to back up each assertion of fact that poaching is much higher than official estimates allow.**

“ One study estimated that a maximum of 4 percent of missing wolves in Wisconsin may have actually died from any cause (Stenglein et al. 2015a, pp. 372–374). Another demonstrated that the rate of wolves that go missing was positively related to wolf abundance (Liberg et al. 2020, pp. 4–6).” **Stenglein et al. 2015 made an error we pointed out and they subsequently corrected it but not enough (Stenglein et al. 2018. We explained why it was not enough. Liberg et al. 2020 made errors we pointed out in Treves et al., 2020 but FWS does not cite either of our challenges. For example, Olson’s work is error-filled and incomplete, we challenged two of his papers and pointed out the errors in Chapron & Treves 2017b and Santiago-Ávila etc al. 202. In fact the latter explained why Olson et al. 2015 got the wrong answer mathematically. Louchouarn et al. 2021 was challenged by Breck et al. 2023, which we challenged and pointed out their errors in louchouarn et al. 2025. Also, all of the papers we challenge fail to share data that they rely on. All citations are presented above.**

”. wolf population management through legal harvest, they may be more inclined to act illegally to address their concerns (Olson et al. 2014, entire; Suutarinen and Kojola 2018, pp. 418–420)”” **FWS is mistaken about Suutarinen, cites only 2 pages when the entire article is relevant. and miss another paper by the same author which says something very different (123). FWS misrepresents the Finnish authors as social scientists and Olson et al. 2014 was shown to misrepresent the evidence and ignore disappearances of collared animals.**

Also, FWS failed to cite (81, 124-126)<— an FWS employee for many years, and (127) thereby missing the best research on their topic.

“Consistent with this theory, a growing body of evidence indicates that illegal take increases when legal take regulations become more restrictive and limit management options (Olson et al. 2014, pp. 4–8; Olson et al. 2017, entire; Pepin et al. 2017, entire; Stein 2017, entire; Suutarinen and Kojola 2018, pp. 418–420; Liberg et al. 2020, pp. 4–6)” **This is full of errors and selective citations. See above.**

“In general, compared to the early twentieth century when take was unregulated, the regulation of human-caused mortality has reduced the number of wolves indiscriminately killed by humans, which has allowed wolves to recolonize areas of suitable habitat within their former range. Despite rules and regulations to discourage such activity, the illegal killing of wolves will continue in the future and although it may affect recolonization potential outside of core areas and overall distribution of gray wolves in the Eastern United States, at current levels, these mortalities have minimal impact on wolf abundance in the Western Great Lakes region.” **The argument made here should be looked at skeptically. Given that recolonization has largely stopped since wolves reached the Wisconsin Central Forest and throughout the U.P. of Michigan which dates back to about 2000s, what evidence is there that unregulated and regulated HCM is low enough to allow further recolonization? It would actually appear as if HCM has stopped recolonization in its tracks and the FWS has failed to curb illegal HCM so that is the limiting factor now (at least in the WGL states).**

Their handling of the effect of wolf-hunting on tolerance for wolves is out of date and does not distinguish between peer-reviewed research and non-peer-reviewed research such as Lewis et al. 2018. More recent peer-reviewed work is now available from Metcalf and colleagues.

:… Human-Caused Mortality in the Western Great Lakes Region

"All other forms of human-caused mortality, including illegal take, make up a small proportion of known human-caused wolf mortalities when compared to lethal depredation control and harvest, although much illegal take of wolves is likely undocumented" **All of our studies contradict the latter claim (Treves et al. 2017a,b, Santiago-Avila et al. 2020, Santiago-Avila & Treves 2022) using the most state of the art methods and published in better journals than Stenglein. There was only one year in Wisconsin (2021) that so-called legal take rose to such a level. And lethal control during livestock incidents rarely exceeded other causes. The FWS claim is especially inaccurate when one remembers that they mismeasured total HCM when counting only legal HCM. We pointed out the arithmetic error that state and federal agencies make when, failing to account for undetected illegal killing, the agency claims a certain cause of death is the highest risk (14). FWS is simply flatly wrong in a asserting that lethal control was the major cause of death. In the long run, poaching has been the majro cause of death even in the worst year for legal killing (Wisconsin 2021 when the harvest overshot by 82%), even then there is some evidence for illegal killing keeping pace (3).**

"and distribution across the Western Great Lakes region, illegal take alone or in combination with all other forms of mortality has not prevented the expansion of the gray wolf population in the Western Great Lakes region." **I disagree. Until about 2003, recolonization was quickly expanding range but after liberalizing wolf-killing and the correlated rise in poaching, recolonization stopped in WI and perhaps in different years but for related reasons, also MI and MN (49-52) rebuttals by Olson, Stien, and Pepin were cited above and did not succeed in rebutting our principal finding because they lacked substantiating data, made errors, and failed various tests of data quality and transparency..**

"This is an appropriate use of the data because all three of the states 'counts/estimates are conducted prior to the breeding season" **unorthodox. What they mean is that the counts were conducted prior to counting pups. But worse, almost no scientist does it this way, instead defining their wolf year not by calendar year but by the census period and birth season (usually ending there wolf year in April or May). One has to be careful when using the calendar year because wolf mortality rises in late winter.**

"thus, we can be certain that a wolf counted in early February was also a member of the population in December of the prior year. Thus, all mortality rates discussed below were calculated by dividing the number of wolves that died from each type of mortality for a given year by the population count/estimate for the end of that calendar year plus the known number of wolves that died from 77all causes that same year (i.e., this sum in the denominator represents the minimum number of wolves known to be alive at some point during the calendar year)." **OK that is the shortcut many have used including me, rather than counting the actual survival time of each marked wolf, which is often impossible. However, the annual mortality rate that excludes disappearances or includes unmarked wolves is a wildly inaccurate (under)estimate of actual annual mortality. Footnote 14 includes a serious under-estimate of total mortality that all wildlife biologists have criticized for decades. Known fates are a subset of all fates especially when collared wolves disappear and are censored, and especially when collared and uncollared wolves experience different mortality rates (128).**

"We report the levels of regulated harvest that occurred during past periods when the species was delisted in the Western Great Lakes region. We also report the level of lethal control of depredating wolves that has occurred while gray wolves were listed in each state, and during the periods in which wolves were delisted. Our future condition projections in Chapter 6 provide quantitative illustration of the effects of potential future harvest and lethal depredation control on the wolf population in the Western Great Lakes

region, should delisting occur." **Future projections fail to incorporate our estimate of actual mortality during the Feb 2021 Wisconsin public, regulated Wolf hunt in Treves et al. 2021 in PeerJ. Also in Treves & Santiago-´®Avila (60) we showed how the WI DNR method for estimating the statewide wolf population counts dead wolves, exaggerates habitat saturation, and uses methods that are imprecise, inaccurate, insensitive to changing conditions, and irreproducible.**

"Regulated Harvest in Wisconsin
Tribes declined in subsequent years to 24 gray wolves in 2013 and 6 in 2014 due to a lack of demonstrated harvest (WI DNR 2013, pp. 1, 2; WI DNR 2014, p. 4; McFarland and Wiedenhoeft 2015, pp. 2, 4). In 2021, the Wisconsin DNR set a gray wolf harvest quota of 200 wolves. In accordance with their treaty rights within the ceded territories, the Ojibwe Tribes declared the full 50 percent of the allowable harvest in the ceded territories and the Wisconsin DNR honored that declaration. As a result, 119 gray wolves were allocated to the state and 81 gray wolves were allocated to the Ojibwe Tribes. The Ojibwe tribes have not harvested any of the allocated gray wolves during any of the regulated harvest seasons and it has since been clarified that the Ojibwe Tribes may use their quota share for protective purposes, rather than be required to harvest those gray wolves (Newland and Estenoz 2022, in litt.)." **Incomplete and State-biased summary. The tribes complained and denounced state policies towards their ceded territorial rights. The tribes won in federal court and state plaintiffs won in state court to challenge state policy in 2021 and 2022. If FWS intends to address adequacy of regulatory mechanisms it might behoove FWS to mention that federal and state courts have precedents with which to stop state wolf-hunting. This also casts a different light on the apparent deference FWS gives to states if the states had to be sued to respect other legal rights?**

Table 4 ignores legal and illegal mortality estimates.

"Footnote to table 4. b Each state in the Western Great Lakes conducts its population counts/estimates in mid- to late winter, when gray wolf populations are closest to their minimum because it precedes the spring breeding season." **This ignores that the 2021 WI wolf-hunt occurred during the mating and pregnancy season which might adversely and significantly harm future reproduction. Tet the state did not measure reproductive success statewide or in a random sample of packs the following summer (Treves & Louchouarn 2022). Therefore the state has an inadequate regulatory mechanism for hunts that run past December. Such hunts are likely to recur because Wi statute allows it and because interest groups have shown the willingness to sue the state to extend the wolf-hunt dangerously.**

"a tribal offer of 85 gray wolves)" **This misrepresents the state and federal relationship to tribes. By federal treaty tribes reserve half of all quotas of harvestable species except timber. The state does not "offer" anything. When the tribes chose not to hunt, the state did not have a legal right to reduce the tribal quota. The federal government (GWS) as trustee of Indian treaty rights enforced by USDOJ should not make this mistake.**

"After that season, the minimum count of gray wolves in the state remained relatively constant, falling slightly from 815 gray wolves at the end of 2011 to 809 gray wolves at the end of 2012 (Service 2020, p. 22)." **We challenged these estimates based on under-reporting of poaching and under-estimates of actual mortality. We also challenged their 2013 and 2014 population estimates on similar grounds. An additional ground to challenge official numbers in 2013 and 2014 was the addition of hounds to the methods allowed for wolf-hunting. This adds unreported killing because (a) free-ranging hounds can kill wolves out of sight of permitted hunters (129), and (b) hound hunters are the least**

tolerant members of the surveyed populations in several peer-reviewed studies (78); Treves et al. 2009, 2013, Browne-Nunez et al. 2015), many of which studies were cited by FWS but not mentioned in relation to the risks of poaching in this section.

FWS refers to 2013 and 2014 "hunting and trapping season" **omitting mention of hounding a wholly different hunting technique covered by different regulations and laws. Note there is virtually no evidence about hound-hunting in the scientific literature but they should cite our pre-print (129) which Is hared with them. FWS should also mention that hounding has not been studied so it is poorly understood and of recent origins requiring more study to understand if it is actually a legal and sustainable method of harvest.**

The first court case they mention is the one that allowed the unregulated 2021 Feb hunt. Other court cases not summarized and only one even mentioned. "due to litigation by Hunter Nation, a Jefferson County Circuit Court ruled the Wisconsin DNR must implement " By contrast what follows does not **mention the plaintiffs or their basis for challenge** "(due to legal challenges, hound hunting was not permitted during the 2012 season)." **And in 2023. FWS does mention the 2021 litigation below. But the omission of the plaintiffs here gives free advertisement to Hunter Nation while concealing the plaintiff's who oppose the state wolf policy in three other cases (2012, 2021, 2023).**

The "17% harvest rate" **has no methods and is not supported by peer-reviewed literature (3)**

This statement is false "The Wisconsin DNR's approved quotas considered 2020 wolf population data, population response to previous harvest seasons, scientific literature" **because they only considered their cherry-picked scientific literature and our work does not support the state claims (3).** "on model projections." W**hich were later shown by (60)la to be inaccurate and unscientific by numerous criteria.**

"As a result, 86 percent of the gray wolves harvested during February 2021 were harvested with the use of hounds. Moreover, due to environmental conditions that provided excellent tracking conditions, the speed of gray wolf harvest exceeded past years and prevented the Wisconsin DNR from accurately evaluating harvest in real time because its 24-hour reporting requirements, while effective at lower speeds of harvest, did not allow the state to close the season in time to limit exceeding the quotas. As described in the *State Management: Wisconsin* section above, the Wisconsin Plan incorporates several measures to minimize the potential to exceed regulated harvest quotas in the future including zone-specific license issuance, and the intent to engage in rulemaking to require harvest registration time be reduced to 8 hours, instead of 24 hours, as currently outlined in state law (State Statute 29.185). Both of these measures would allow the Wisconsin DNR to better monitor harvest data and ensure timely zone closures.?**" In addition to the issue of not understanding hound-hunting (see above), doesn't this paragraph indicate that regulatory mechanisms were inadequate in the past ? Ensure? And really, what evidence do they present above? The above text comes straight from the state without any FWS skepticism. I see three problems. First the inadequacy of the past evidence from the Feb 20s1 hound hunting should make the FWS and any federal court skeptical about the adequacy of current regs. Second the proposed current regs are not proven adequate just proposed to help. Will they?**

Third, an impartial and unbiased FWS would have written something like "However, scientists have questioned the ability of the WDNR to prevent illegal killing, enforce zone closures, require timely reporting, reduce permit applications, and manage the size of hunting parties that use hounds.

“Depredation Control in Wisconsin From 1979 through 1989, there were only five cases (an average of 0.4 per year) of verified gray wolf depredations in Wisconsin , but the number of incidents has steadily increased over the subsequent decades as gray wolf populations have expanded.” **omitted first peer-reviewed report (Treves et al. 2002). But if they cited me they might also have to cite me for explaining why the correlation does not imply causation and subsequent work predicting where such events occur and therefore making it easier to prevent proactively with non-lethal methods (Treves et al. 2004, 2011, 2016). There is no conclusion to draw other than FWS cherry-picked evidence and discriminated against viewpoints they do not like.**

“During the 1990s there were an average of approximately 4 incidents per year, increasing to an average of approximately 54 per year (ranging from 14 to 92) during the 2000s and to an average of approximately 91 per year (ranging from 61 to 125) from 2010 to 2022 (WI DNR 2023a, p. 89). “ **Omitted alternative explanation that interventions were increasing the losses (Santiago-Ávila et al. 2018) and the geographic spread not the population size increase may have been responsible.**

“While wolf-livestock depredations occur across gray wolf range” **exaggerated claim flies in t he face of the evidence (Treves et al. 2004, 2011, 2016). “..** in Wisconsin, most occur in the northwestern part of the state where livestock production is in close proximity to gray wolf habitat (WI DNR 2023a, pp. 90–91)”. **No mention of central forest area and points north with more livestock which do not have more losses. No mention of our risk maps quantifying where most occur.**

“Although the number of incidents and impacted farms is relatively low**” relative to what? They don’t mention that more livestock die from other wild causes, disease, weather, and accidents than from wolves, perpetuating a biased look at the issue.**

““Between 2012 and 2014, when gray wolves were delisted and lethal depredation control and regulated harvests were allowed, verified gray wolf depredations on cattle and the number of farms with verified depredations 93declined significantly (i.e., decreased by 26 percent from 2012 to 2013 when 43 farms were affected, and by 11 percent from 2013 to 2014 when 36 farms were affected) (Wiedenhoeft et al. 2015, pp. 4–5, 12),” **use of the word ‘significantly’ implies a statistical test for this sort of claim but none was performed. More concerning, the Walker administration clamped down on open records, hid data from researchers, and stopped a lot of there analysis of previous years. In short, these data are suspect on many fronts (22, 130).**

“ indicating that active management with regulated harvests and targeted lethal depredation controls can reduce conflicts.” **Inaccurate and contrary to the bulk of peer-reviewed evidence some of which I have cited above.**

“These structural changes in agriculture, along with the increased use of conflict mitigation techniques, may reduce the frequency or severity of wolf-livestock interactions.” **An impartial observer would point out that wolf-hunting stopped from January 2015 until February 2021, so the explanation for a decline in livestock losses can be found there if you like correlations as they apparently do (see above).**

“Although wolf depredation on hunting dogs is often perceived to have increased with gray wolf population growth in Wisconsin, data from 2006 to 2024 suggest a more stable pattern. Excluding two outliers in 2012 and 2016, the annual number of reported hunting dog depredations has remained

relatively consistent, exhibiting natural interannual variability but no clear upward trend (Ruid 2024, USDA-Wildlife Services, unpublished data)." **Several problems here: (A) unpublished data, (B) removal of outliers (first mention of such manipulations, (C) no citation to multiple peer-reviewed studies (not just by me and my team but by Olson and Bump).**

" The low incidence in 2012 coincided with ongoing legal challenges to the use of dogs in wolf hunting, likely leading to reduced hunting activity and underreporting of wolf-dog conflicts.**" Why not mention the lawsuit and plaintiffs like they did for Hunter Nation above?**

" In contrast, 2016 saw a marked increase in depredations following the removal of individual fees for bear dog training permits, potentially increasing dog exposure in wolf-occupied areas (Wydeven 2025, in litt.).**" unpublished work without citation to Treves et al. 2002, Wydeven et al. 2004 cited previously here and (131, 132). I think FWS just adopted whole cloth the report by Wisconsin green Fire, an interest group.**

"While the Wisconsin DNR compensates dog owners for mortalities and injuries to their dogs, the DNR takes no action against the depredating pack unless the attack was on a dog that was leashed, confined, or under the owner's control on the owner's land (this is the case while the species is listed and would remain the case if the species were to be delisted). Instead, they send e-mail notifications to inform hunters and bear-dog trainers of the areas where depredations have occurred (Wydeven 2025, in litt.)" **Critiques of the interventions mentioned in this paragraph have been published and peer-reviewed in Wydeven et al. 2004 and Markosyan, Menefee, Treves since 2022, now available in pre-print at https://www.biorxiv.org/content/10.1101/2022.08.16.504031v3.**

Also, Treves & Menefee pre-print examined the unreported injuries to wolves caused by hounds and the harassment and unlawful behavior of hounders affecting human bystanders.

No citation to evidence for the effectiveness of non-lethal methods and the higher quality of those studies than to lethal interventions (dozens of references above).

"(1) as authorized under section 4(d) when the population was reclassified to threatened (from April 13, 2003, to January 31, 2005), (2) by special permits (from April 1, 2005, to September 13, 2005, and from April 24, 2006, to August 10, 2006), and (3) when delisted (from March 12, 2007, to September 29, 2008; May 4, 2009, to July 1, 2009; January 27, 2012, to December 19, 2014; and January 4, 2021, to February 10, 2022). During those times, a total of 425 gray wolves were lethally removed for depredation control under the various authorizations. The number of gray wolves removed per year ranged from a minimum of 10 (in 2009) to a maximum of 76 (in 2012) (not including 2022, because it does not include the peak depredation time period for that year) (Table 5)." **These periods were specifically and particularly examined for the relationship between legal HCM and illegal HCM. No mention by FWS. Above I cited all the studies led by Chapron, Santiago-Ávila, Treves.**

"Additionally, Wisconsin Administrative Code NR 10.02(1)(b) would allow a landowner, lessee, or occupant (or any person with their permission) to shoot a gray wolf in the act of killing, wounding, or biting a domestic animal on private property. Those shootings must be reported within 24 hours and the gray wolf carcass must be turned over to the Wisconsin DNR." **Do either of these regulations actually prevent illegal take or claims of threats to property or human life that are false? Why wouldn't a landowner drag a dead wolf onto their land and claim it was killed there? Given the poor record of**

effectiveness of lethal control (six studies see reference to Kutal, Grente, Krofel, Nadler Preiss-Bloom, Suba, Santiago-Ávbila cited above) and the mixed record of success of another several, why would WI allow practically-unregulated killing like this?

Moreover, WI pays compensation for property damage very generously and then also kills wolves compounding the incentives for fraud and the incentive to complain to remove wolves causing no harm (Naughton-Treves et al. 2003; Treves et al. 2009; Treves & Bruskotter 2014, Treves et al. 2013 cited above) .

Other regions

" human-caused gray wolf mortality is overwhelmingly the result of dispersing wolves being mistaken for coyotes and killed;" '**Overwhelmingly' implies data. Where are the data? Are they peer-reviewed?**

How do you distinguish a dispersing wolf from one of a pair attempting to breed, or of a pack member for a pack not yet detected? The FWS certainty beggars belief. They did not cite Treves et al. 2009 here but did elsewhere when they liked the finding — on extra territorial movements (ETM) by wolves that belong to a pack. Such ETMs can last long and range very widely (1 month or more and hundreds of km within a state and beyond its boundaries) only to return.

"However, such cases of misidentification also occur in parts of our analysis area where wolves are considerably larger than coyotes (e.g., wolves misidentified as coyotes and killed by hunters in Missouri range in size from 80–104 lbs.; MDC 2013, entire)." **Also Treves et al. 2017 for numerous cases of this in Wisconsin, which begs the question 'how do FWS and states propose to regulate illegal killing if this happens as commonly as everyone seems to admit?**

Chapter 4

"This method relied heavily on information from radio-collared wolves; thus, the Wisconsin DNR had an intensive radio-collaring program with a goal of having
at least one radio-collared wolf in approximately half of the wolf packs in Wisconsin" **They almost never got close. The multi-year average through 2007 was 13% of packs (Wydeven et al. 2004, 2009).**

"In 2018, due to the amount of effort required to conduct minimum counts as wolf abundance and distribution increased in Wisconsin, the Wisconsin DNR began work to develop an alternative method to estimate wolf abundance in the state based on a scaled occupancy model (Stauffer et al. 2021, entire;" **FWS does not fairly represent our many critiques of Stauffer et al. 2021 which we deem inaccurate, imprecise, irreproducible and insensitive to changing conditions. In particular it was calibrated for years without legal killing and counts dead wolves from prior years in current population estimates perforce (60). They do mention our critique after but not completely or fairly.**

"were consistently within the estimated range of values produced by the scaled occupancy model indicating that model results were a reliable alternative to minimum counts " **This conceals that the confidence intervals were very large around the scaled occupancy estimate and the average was consistently higher than the minimum count method used since 1979 (60).**

Table 7.

I have criticized this table or a slight modification of it in toy official 2019 peer review of the delisting proposal. Did they ignore my peer review?

Specifically, accepting state estimates of wolf abundance: FWS are not using BAS simply because they accept non-peer-reviewed and non-disclosed census methods that have been criticized by peer-reviewed analyses (Crabtree et al. (133) for Montana, Treves & Santiago-Ávila 2023 for Wisconsin) and Creel et al. 2021 (134) not peer reviewed but worrisome for MT and ID.

Figure 13.
We specifically challenged the Wi annual estimate of wolf abundance in Treves et al. 2021 (22) because of changing methods creating a false impression of a single line when actually there are 4-5 different trajectories with different confidence intervals. Thus Figure 13 gives a false impression of confidence for WI. Also MN is unlikely to be accurate given the way they survey so it too gives a false sense of certainty about how many wolves are present.

"However, based on additional discussions with the lab that conducted the 2019 analysis and the physical characteristics of the individual canid in the recent video, MDIFW considers these animals to more closely resemble eastern coyotes rather than gray wolves (Webb 2023, pers. comm.)." **visual assessment of morphology is not an accepted method for species designations even if one has the carcass. Assessment from a photo is laughable. Pers. comms. no matter how expert the claimant is unscientific, irreproducible, imprecise, and almost worthless.**

"There are confirmed records of a few lone gray wolves elsewhere within the Eastern United States since the early 2000s." **How do they know they were lone? Moreover this whole section indicates that human-caused mortality is preventing recolonization of a significant portion of historical range.**

Chapter 5
"We developed this model to create transparency in our conclusions regarding gray wolf resiliency and redundancy, two key components of viability, and to quantify our uncertainty in these future projections." **They set a high bar for themselves because of the high standard set in current scientific literature and consensus on what constitutes transparency. The second part of that sentence regarding quantifying uncertainty also deserves separate and equal scrutiny.**

"We chose the type of model, scale of the model, and assumptions of the model based on the best available scientific information." **This is a problematic claim because these are value judgments, i.e., not scientific dictates. I mean the choice of assumptions is usually based on opinion or preference; the scale likewise is a preference. Maybe the type of model is more scientific but given there are alternatives, the BAS requires they explain why the type of model they chose is the best. Treves et al. 2021 (22) explains how one embeds value judgments into models and their specifications (including scale and type) based not on science but on assumptions and preferences. We used the WI 1999 mgmt plan as an example.**

"Chapter 6, we qualitatively discuss expectations regarding expansion of gray wolf populations outside of the three states in which gray wolves currently occur in the Eastern United States, potential changes in suitable habitat and prey availability, and potential changes in genetic diversity. We did not develop a quantitative model to project recolonization of these areas because these areas are not currently occupied and gray wolf demographic data specific to these areas do not exist. Undertaking such a modeling effort would have required us to make subjective assumptions about how states that are currently unoccupied would manage gray wolves in the future." **This is unconvincing. Predictive models (projections, future scenarios, etc.) often do what the FWS claims it cannot do. Moreover their model for the WGL**

already contains subjective assumptions (see below) and speculations about future management regimes. The ESA demands such projections when it addresses adequacy of regulatory mechanisms in the 5-factor analysis. Note also that they have implicitly embraced forecasting in their definition of habitat suitability (prey and HCM), which presumes past trends predict future conditions. So their decision to go qualitative is unpersuasive.

"Analysis Units"
By treating each state (MI, MN, WI) as an independent analysis unit, the FWS is obligated to consider migration between states. We published the data on such historical, known migration of radio-collared wolves (12).

If they do not address migration then the model will under-estimate viability (conservative). If they handle it otherwise they may over-estimate viability (risky). A great deal of Uncertainty lies in this issue as we explained for Wisconsin wolves (4, 62, 63) withstanding a critique by Roberts et al. which we debunked.

"To construct a population model for each state, we first determined whether density-dependent or density-independent growth better characterized the population dynamics in each state." **As stated this would require a complete and thorough review of the literature on the topic. There is currently consensus that WI wolves did not show density-dependent mortality (Stenglein and Treves et al. agree, see above citations) but no agreement on whether reproduction was density-dependent (Stenglein et al. 2015 versus Chapron & Treves 2016a,b,2017a,b). Within the latter lack of consensus note that Stenglein et al. refuse to share data and Treves et al. has shown with a great deal of evidence that their claims of density-dependent reproduction are based on unclear figures, unshared data, and known non-independence of samples. We even quoted Stenglein's authors back in 2004 about the latter problem. (Wydeven et al. 2004). FWS should be aware that failing to summarize the debate fairly is a breach of scientific integrity and failing to note which studies used better methods or worse methods such as data sharing is a failure to use BAS.**

Regarding Michigan, the study by O'Neill et al. 2017 shows pack-scale density-dependence which means the FWS should model at a pack level not a population level to capture ANY density-dependence.

For Minnesota I am not aware of good evidence for population-scale density-dependence so I will read with interest. Coming back to this later, Id did not find that evidence.

"Density- dependent growth describes populations in which growth rates are related to population size." **Strictly speaking that is not correct. Only if the geographic area is kept constant can you make the latter claim. Because wolf packs settled in new areas over time, the geographic area is not constant. It is more accurate to say that life history rates correlate with density as the name suggests. The distinction is very important because we showed that density of WI wolves changed by 1% approx. over a ten-year period while the population grew several-fold (Chapron & Treves 2016a and Treves et al. 2021).**

"where small population sizes are limited by mate finding or when increasing numbers of conspecifics provide a benefit to fitness such as for herd or flocking species)." **This is an odd statement because it is particularly relevant for cooperatively breeding species like wolves. Their omission seems an intentional omission. They seem happy to mention "pack hunting" but reluctant to mention**

"cooperative breeding" as if any positively valued word such as cooperative should not be applied to wolves but pack hunting (Vaguely negative) is OK. That trips them up here because positive effects of large pack sizes on reproduction are precisely what we observe in wolves. Therefore the following statement is downright misleading "Positive density dependence is generally only observed at very small population sizes and is related to other small population effects. " **it is misleading because (a) O'Neill et al. showed pack-level density-dependence so the FWS needs to look at this issue REGARDLESS of population size. It is also misleading because wolves specifically show positive density-dependence as pack sizes grow.**

They also blunder in defining density-independent growth as "where growth is not related to population size, in which populations grow at a steady continuous rate indefinitely". **There is no such requirement of steady growth but rather no correlation between density and growth over a sufficient period to evaluate the correlation. BTW in Treves et al. 2021 we cited third party researchers studying many mammalian populations; those authors (135) and others found several reasons why populations would not show density-dependence. We identified three of those reasons applied to WI wolves (and probably also MI and MN wolves). FWS is ignorant of our work or dismissed it so they can argue for density-dependent population growth. Why? Because it makes two inaccurate claims easier (a) wolf population growth will naturally diminish as wolves have already filled suitable habitat, so we should not expect further growth beyond the WGL, and (b) they can claim sustainable levels of HCM or harvest will not harm population growth. Both (a and b) are inaccurate or false.**

"We compared model results to determine which model best fit the gray wolf population data for each state. We then used the parameter estimates from the best fitting model to project future gray wolf population size under several scenarios…" **OK here they walked straight into a BAS pitfall. Transparency issue: they are ignoring the scientific debate over d-d growth summarized above. Uncertainty: they are treating the state population estimates as if they have low uncertainty. States would like to claim that but peer-reviewed literature specifically attacks the reliability an uncertainty of the WI population estimate (Treves & Santiago-Ávila 2023) and FWS claim our work was rebutted falls far short of a scientific rebuttal. Specifically we questioned the precision (i.e., uncertainty) of Wisconsin's method. It is also not transparent as we argue on multiple points, therefore FWS uncritical use of the numbers — without discussing and considering wider confidence limits on those numbers — cannot be considered transparent.**

Model parameters
They state "Positive density-dependent growth is described by the following equation:"**"***rmax* is the per capita intrinsic rate of growth (which captures reproduction – natural mortality + immigration – emigration)" **I see problems with there definition and also the FWS minimized uncertainty erroneously.**

Also keep an eye on how they define natural mortality. I don't think "natural" is used in the theoretical work on this equation but I could be wrong. In any case they are not transparent about what they mean by "natural" here.

Also note immigration and emigration are two parameters so treating states as independent units and then parameterizing migration in this way is challengeable (double- or even triple-counting migrants). Said in another way, the FWS assumes the states have independent growth trajectories. Their model parametrization above seems to assume immigration and emigration are independent

phenomena in each analysis unit, yet the states neighbor each other so one state's emigrant is another state's immigrant, etc. This should be transparently discussed or they are hiding a source of uncertainty and double- or even triple-counting wolves (inflating population viability).

Indeed, they capriciously abandon the meta-population concept they spent a previous section writing so much about.

"*K* is the estimated maximum population size for a particular state" **If one assumes that the states are saturated or that all suitable habitat is known and will not change, we can estimate K. However neither condition is met historically in any of the states. Therefore, K will be a systematic under-estimate in WIs not sure about MI and MN. As the history of WI wolf population growth shows, each time someone tried to estimate K, the wolves proved them wrong (22), so why not again?**

"and *h* is an estimate of the additive effect of harvested animals (*m*) + animals removed due to lethal depredation control of wolves (*c*) on gray wolf population dynamics (i.e., the per wolf effect of removal on the overall population growth)." **Whoa…see above for the problem with natural mortality. Now, "Natural mortality" appears to include vehicle collisions and poaching because they pulled H(c+m) out of the term h for additive legal HCM. I see several potential problems.**

In their model, all legal HCM is removed from population size in December of year t (by their definition). That assumes no legal HCM affects reproduction in year t adding to year t+1. That assumes that all dead breeders are replaced after h is imposed. Brainerd et al. 2008 and Wisconsin's Feb 2021 wolf-hunt undermine these assumptions, which would lead to higher growth estimates than would really occur because the equation counts successful breeding by packs that are subsequently hit by additive HCM occurring before the end of the wolf census period. That is fine if all HCM occurs before Dec. 31 of year t but it does not (Santiago-Avila & Treves 2022 (10) showed how poaching is higher in winter months when snow cover is most likely and TChapron & Treves 2016a,g,2017a,b (withstanding error-filled and data-free rebuttals) showed how poaching is additive to legal HCM affecting Nt+1.).

Also, FWS would have done well to consider the number of breeding packs (not the number of wolves) as the variable upon which to grow the population (Treves & Louchouarn 2022 demonstrated the effects that can have on population estimates (inflating uncertainty). Also Treves et al. 2021 showed how how using the numebr of packs rather than the number of wolves altered claims about density-dependent growth.

And many authors like Vucetich 2012 in an WFS peer review (did they forget it is in their own files?) have discussed how the timing of events can shape outputs of such models greatly and that there is no consensus on when to add and subtract life history events from population dynamic models.

Ultimately it is a value judgment about how you model legal HCM but the FWS does not mention that transparently.

"here λ is the ratio of the population size (*N*) at time (*t*) over the population size at the previous time step (*t –1*), and all other variables are as defined for Equation 1 above." **Wait…ask an expert but this explanation seems wrong. Lambda should be the historical average, I.e., predict Nt+1 from past**

lambda. The importance of this issue is that FWS has to choose the time period in the past to estimate lambda and justify this is useful for future projections. Not transparent.

Also uncertainty is hidden here because of Course year to year variation in growth would appear as confidence intervals around lambda and they should be wide confidence intervals to include bad and good years. Instead they seem to use a single value without addressing its variability.

"Based on the analyses described below, negative density-dependent models were a better fit for the empirical data for all states (see Supplementary Material A for details of model fitting). Therefore, we used negative density-dependent models when estimating future population size in our model projections." **In short, it was a predetermined conclusion because they used the state population estimates all of which show a decline or flattening out in the latter part of the time series as expected if the populations were reaching K carrying capacity. However, all three populations were hunted in the second halves of the time series (even setting aside my qualms about how they count wolves in WI and MN), so the imposition of limits was by legal HCM. That is not accepted as evidence of d-d population dynamics. The theory of density-dependence does not accommodate purposeful human decisions to increase or decrease mortality BASED ON DENSITY. When that happens the population shows human-imposed reductions in density not intrinsic density-dependent growth. That is why FWS reference natural mortality without defining it, so they can dodge the possibility that a component of natural mortality actually correlates with legal HCM (i.e., poaching). This is another example of non-transparency.**

Uncertainty would change the shape of population growth curves so it is harder to detect density dependence. So they sweep uncertainty about census counts and different methods for census-taking under the rug (Treves et al. 2021).

Finally, by claiming negative density-dependent growth in wolves, the FWS can claim a K exists. I explained above why d-d growth is not well supported at a population level. Also K is dubious if wolves were allowed to fill suitable habitat. So the entire premise of their favored model is dubious. D-d population dynamics just makes their job easier not better (Treves et al. 2021)..

"Mortality scenarios"… we also varied the levels of human-caused mortality in the populations (i.e., regulated harvest and lethal depredation control) to examine a range of potential future effects of this stressor in the future." **Here, by separating so-called natural causes' from HCM that is legal (harvest and lethal control), the FWS side-steps non-independence of mortality causes. For example, poaching is correlated with legal HCM in five studies by four different lead authors (no citations to that finding is like they can cover their eyes to dismiss the science). As a result when they vary legal HCM for scenarios they fail to vary poaching even though we have proven beyond reasonable doubt that the two covary. FWS is acting in an arbitrary fashion here.**

"describe the two future mortality scenarios we constructed." **Only two? Off the top of my head I can think of half a dozen other realistic scenarios so their reliance on a few is arbitrary (harvest with and without trapping, with and without hounding, no lethal control, no harvest, respecting the treaty that allocates half of quotas to tribes with and without tribal participation, allocating less as WI did in the past, etc.). I'm sure I could think of more. Also what if a state adds lethal control after hound losses to the causes that allow lethal control? Note that by examining only two scenarios, they have implicitly assumed lethal depredation control will always be present, hence non-lethal methods will never replace them and rates of lethal control will look like past ones,. i.e.,**

no reduction in livestock losses, no effectiveness of lethal control. By implication, wolves with threatened status in MN are being killed to no effect. Shouldn't FWS transparently address this issue? And after delisting, shouldn't the adequacy of state regulatory mechanisms be interrogated if the states continuously kill wolves to no effect?

"g the number of gray wolves harvested by the population count/estimate for the end of the calendar year21 plus the known number of gray wolves that died from all causes that same year, i.e., this sum in the denominator represents the minimum number of wolves known to be alive at some point…" **very strange. This procedure deflates the apparent legal HCM by summing two values in the denominator. Because harvest happens late in the year typically but censuses are concentrated afterwards and all other mortality occurs before the harvest, one might think the their algorithm might be sensible (always bearing in mind it is not the best available science and represents a value judgment about how to calculate annual HCM rate). But uncertainty has not been considered. The uncertainty arises from the timing of HCM relative to census.**

Also they are wrong that it is a minimum number. The actual minimum number is H/Nt, an estimate that assumes harvest occurs before all other mortality (unrealistic but that's the theoretical minimum estimate).

Now let's look at how their estimate of harvest rate behaves if the timing of harvest is not at the end of the year. if lots of mortality occurs in Jan-March as we showed it does in Santiago-Avila & Treves 2022, or harvest occurs after 31 December as with Wi Feb 2021 season, then the procedure does not make sense because dead wolves are counted as if they were alive by the time fall harvest rolls around and last year's harvest is not counted at all in this year's harvest — unless I misunderstand. Given they did not transparently explain what they do if harvest occurs in Jan or Feb it is impossible to confirm or deny that they did it right. So transparency is lacking and uncertainty has not been handled scientifically.

Footnote 21 seems wrong.

Mentioning ancient data without clear methods (rather than more recent data from Idaho and Montana), FWS states "…we chose to model harvest levels as a consistent proportion of the population (i.e., a harvest rate) versus a fixed number of gray wolves removed annually; the best available science discussed above does not indicate that harvesting a fixed number of wolves consistently (especially as population sizes change) is likely." **Absolutely no mention of hunter effort. Catch per unit effort is a standard approach to estimating sustainability of harvest and estimating harvest rates, yet FWS does not mention this hallmark of harvest models.**

While I agree that harvest rate should rise and fall with population estimates, they should address uncertainty in population estimates and the BAS about how this type of harvest is also prone to cause population crashes (Fryxell on fisheries (136)). Namely as manager see declining bags they reduce quotas and vice versa, responses that produce extra variability in population size oscillations.

Failing to mention hunter effort (non-transparent) and failing to mention uncertainty in population abundance estimates makes their approach NOT BAS By classical scientific principles.

"Based on the best available scientific information in state management plans," **If they are going to modify BAS they should explain what makes some of the information less good in state mgmt. plans and did they apply that lens throughout?**

"we assumed that legal harvest would cease in each state when gray wolf population sizes were reduced to a specific population size (Minnesota) or state relisting criteria (Michigan and Wisconsin), as described below." **Well that's not what stopped harvest in the past. Court decisions did. They fail to model courts stepping in when the states failed to regulate adequately.**

Also if population size estimates change as they did in WI, and harvest continues despite not knowing if the population estimate is truly above the stated level or simply appears to be above the stated Lebel, then what? To wit,
"Finally, in Wisconsin, gray wolves would be listed as a state-threatened species if the population size is fewer than 250 gray wolves in the state (WI DNR 2023a, pp. 126–127). Therefore, we assumed in all scenarios that if the gray wolf population size was at or below 250 in the state of Wisconsin, legal harvest would cease in that state." **This assumption was flawed in the past so it is reasonable to doubt it now. Namely,. The DNR recommended a quota of 130 but the NRB over-ruled and set a quota of 300 (FWS reiterated this history themselves) and our models showed both quotas had a finite probability of dropping the wolves below the state relisting threshold (4, 63) withstanding the lame rebuttal by Stauffer et al..**

In short, harvests do not end when there is a risk of dropping below state thresholds for stronger protection UNLESS pro-wolf interests sue. The behavior of the WI NRB (and even the WIDNR) suggests a risky wolf harvest would proceed. Also see the quota over-shot in Feb 2021 for additional risky policies.

Lethal control.
They chose to include only past lethal control rates. This assumes no increases will occur. That is a value judgment not a scientific one.

A more realistic upper level estimate would entail estimating the maximum number of independent livestock incidents multiplied by the maximum number of wolves killed per incident. It's unclear why they took the maximum killed historically only during May-September and in Minnesota why they truncated the years (no scientific justification for that). The result of that assumption is they will under-estimate the future lethal control during a bad year that extends beyond May-September (increasingly likely with climate change).

Also, they ignore changes in rules, which have occurred (e.g., WI allowing private landowners shooting permits without verified losses), or changes in rules that might allow lethal control under more liberal conditions. BTW they also don't cite studies of fluctuating levels of livestock loss or studies that examined good and bad years for wolves vis a vis lethal control. For example, one year (2015?) WI did catch-up killing, meaning USDA-WS killed wolves implicated in livestock losses from past years as if they carried their culprit status with them like a brand which would predict future problems from those wolves even if no problems had occurred in the interim. Also USDA is

killing wolves that were probably not involved in livestock losses, further accelerating problems by surviving wolves (see Santiago-Ávila et al. 2018).

Policies like those above undermine FWS estimates of max lethal control. Therefore WFS is not being transparent about how their estimates minimize rates nor handling uncertainty properly.

"Future Mortality Scenarios": **although they seem reasonable, I have shown above that they are consistently minimizing the annual conceivable rate of HCM based on past observations of state management.**

Basically, conservation biology (which they purport to follow with the 3Rs) calls for the precautionary principle. In practical terms for an exercise like FWS is performing precautionary means using the higher estimates of past HCM and total mortality into the future, and using the lower birth rates into the future.

Also, philosophically if not legally the FWS is encouraged by the ESA to cooperate with states not to roll over. They are also legally the secondary trustee after delisting and the primary trustee during ESA protections. A fiduciary trustee has to be transparent and use the best available science to account to all beneficiaries for use of the asset using the prudent man standard. Doesn't this mean the worst case scenario should guide all actions?

I could easily do another modeling effort using these precautionary principles and taking the worst case scenarios to forecast future conditions of the WGL. They could easily do it too but chose not to. That choice is a value judgment.

"in addition to the scenario described above that is consistent with the updated Minnesota and Wisconsin plans, we consider a second scenario that is consistent with past practices and relies on past data from the Western Great Lakes states to inform mortality rates. This scenario reflects the observed harvest and lethal depredation control rates. We do not consider a scenario where Wisconsin, Minnesota, or Michigan increase harvest rates or lethal depredation control rates above those observed in the past (but see Appendix 4) as these states have not expressed an intention to reduce wolf populations from current population sizes. Overall, the updated plans in each state outline a conservative approach to gray wolf management with a focus on maintaining viable gray wolf populations and managing gray wolf-human conflict at a local level." **This sounds reasonable if intentions and commitments are legally binding but we also have a history of failing to live up to those commitments (e.g., WI Feb 2021 wolf-hunt and then proposing a second one in Fall 2021). Or Michigan overturning the public ballot initiative, which implies a realistic predictable risk that their legislature reverses the management plan.**

"…for Michigan, harvest occurred in 1 year only; therefore, the maximum and minimum rates of harvest are the same value. " **this statement is false and betrays their misunderstanding of uncertainty. Indeed it betrays how they under-estimate uncertainty systematically in a way that modern wildlife science has long understood to be misleading. Their estimate of harvest rate is h / (Nt + other mortality) hence the denominator contains a range of values depending on uncertainty of those estimates. Therefore annual harvest rate also has ranges of values even if only one year saw harvest. By failing to note that transparently and misunderstanding uncertainty their scenario is not BAS.**

Regarding WI "if the population falls below 800, we assume the state will manage populations to promote growth as stated in its management plan. We further assume this growth occurs at the rate we would expect based on our density dependent model." **These assumptions are dubious given the 2021 state policy issues already discussed and given Treves & Santiago-Ávila's 2023 doubts about the precision, accuracy, sensitivity to changing conditions, and reproducibility of the census method.**

Note that the "Mortality scenario 2" **seems to use maximum legal HCM. it does not. First the lethal control rate is not the max (see above), the annual harvest rates and lethal control rates are not the maxima (see above), and the FWS did not correctly calculate the uncertainty about the maximum rates (see above) including systematic under-estimates of all rates. Also footnote 25 make sit sound like fluke exceedances of quota but those so-called flukes might recur. It is a value judgment by FWS to call them flukes and exclude those as maxima.**

They also omitted health and human safety HHS mortality which was quite high for several years in MI, for one.

WI 2023 mgmt plan "could potentially**" reduce overkills. Maybe, but where is the evidence? And its just promises not actual enforcement.**

Because Minnesota is such an important part of FWS' plans for viability, let's revisit the imprecise way MN estimates wolf abundance. Historically MNDNR sent out a questionnaire survey to a large number of government agents who spend time in the field and might perceive wolves. There are reams of evidence that (a) people misperceive coyotes, big dogs, and other stuff as wolf tracks in the snow, (b) people misidentify Canids under field conditions even when they see them, (c) people double-count the same wolves when those wolves use adjacent census units. Is MNDNR using BAS to take into account these sources of error and possible systematic over-estimates of wolf numbers?If not, the FWS is adopting there less-than-BAS uncritically and non-transparently without a scientific accounting of uncertainty.

Also wildlife scientists know that people often miss loners and packs living in remote hard to reach areas. MNDNR corrects for the former by including an old unverified correction factor from Fuller's study. For the latter I think they do some correction but is it verified and uncertainty correctly parametrized? Who knows?

In short, MN DNR count of wolves has lots of uncorrected possibilities of over-estimate and some corrected and uncorrected sources of under-estimate.

When identifying occupied and unoccupied habitat where the MNDNR have no data, they use studies of radio-collared wolves mainly from protected areas. Those estimates may not apply to agricultural areas of the state. Who knows what effect this has because the data have not been published in a way that allows replication.

Also MNDNR only did this survey every 5-10 years.

Yet FWS treats the MN state estimate of wolves as truth. Also the risks posed to MN wolves from a sudden increase in HCM or a catastrophic mortality cause might not be quickly detected because of the intervals between successive census. Therefore, FWS should be even more precautionary than I have indicated above, at least with regard to MN wolves.

"As harvest and lethal depredation control rates increase, our models assume that, at some point, human-caused mortality from these two sources becomes fully additive as gray wolf populations can no longer partially compensate for these higher levels of mortality. We model the transition from partially compensatory human-caused mortality to fully additive human-caused mortality as occurring at a random value between 20 and 40 percent combined harvest and lethal depredation control each yea" Oof. **Besides failing to account for super-additive or depensatory mortality (Creel & Rotella, Vucetich 2012, Chapron) & Treves 2016a,b), they chose very high rates of HCM to make the transition. Recall Adams said levels about 29% were unsustainable when compensatory immigration is low or absent as would be the case for the WGL (see my above comments on migration from Canada and migration between states). Therefore a shift to additive should happen before 29% (see Vucetich 2012 for an even lower, precautionary, conservative estimate of the level of HCM at which that might happen).**

Also, total HCM is measured by all studies cited. FWS is in error when using just legal HCM. Therefore FWS is actually encouraging unsustainable high HCM by states after delisting. This is again non-transparent.

Furthermore, they do not address uncertainty about compensatory and additive mortality because that is affected by which wolves are killed and when. For example, consider if another WI wolf hunt occurs in Jan and Feb? What if hounding selectively targets breeders? Breeders are killed, so such mortality is super-additive.

"i.e., if there is a low probability of having fewer than 192 to 417 gray wolves), there would be an even lower probability of Allee effects occurring (i.e., of having fewer than 20 wolves)." **There seems to me to be a problem here. The estimate they cite Stenglein & van Deelen referred only to WI and MI (in addition to having made errors (see Treveset al. 2022 and the correction by Stenglein and van D that followed.). So even if Stenglein and van D are correct, FWS has to remove the >20 wolves from the calculation and consider MN separately when calculating the threshold. And is the threshold a proportion of the population or an absolute number given that wolves breed in packs not as individuals?**

"… MVPs require normative (value-based) decisions around acceptable levels of risk (Flather et al. 2011, p. 314, Frankham et al. 2014, p. 61, Wolf et al. 2015, p. 1-2). Specifically, the level of acceptable risk over a specific timeframe must be defined (i.e. a 5 percent risk of extinction over 20 years or a 10 percent risk of extinction over 30 years) in order to determine an MVP." **This is ironic given (a) they riddled their work with normative value judgments see above and (b) the ESA actually does set a normative level of acceptable risk based on SPR not numbers.**

"Key Uncertainties and Assumptions…
Models can benefit decision-making by: explicitly and transparently defining assumptions; … providing an adaptable framework to incorporate new data as it becomes available …" **how does their model do that? "**In our future projections, we captured the effects of three major stressors on gray wolf populations in our models (harvest, lethal depredation control of depredating gray wolves, and disease)." a**And along the way they missed, ignored, and down-played the major cause of HCM which is poaching (Treves et al. 2017a,b).**

"Additionally, our model assumptions were designed to avoid making quantitative predictions for situations where uncertainty was unacceptably high" **value judgment**

and to increase transparency by explicitly stating our uncertainties (and the strategies we used to address them)." **Uncertainty is a scientific concept that is quantified by appropriate parametrization of imprecision (known errors in measurement, inaccuracies (partly known bias in measurement), and missing data (unknown unknowns). They did not do any of these things properly (see above). Instead they treat uncertainty (real) as if it was Ok to throw up their hands and dismiss the missing information.**

All in all, their apparent candor about their uncertainty hides more than it reveals. Remember they hide value judgments (assumptions without scientific validity) within their choice of parameters and model structures. They apply the theory of density-dependence to make their job easier and encourage human-induced caps on population size by modeling only within current geographic limits (See above). They dismiss a huge set of literature by simply not citing it. They define variables vaguely or idiosyncratically without reference to best practices, and they systematically and consistently underestimate risk and over-estimate state compliance with plans on paper.

Appendix 4, **the so-called Mortality scenario x**

FWS writes that Mortality scenario x "…is inconsistent with both the goals of all three states ' management plans and past practice in the states." **But note all four of their numbered regular scenarios are also inconsistent with past practices (see above).**

"In Mortality Scenario X, we calculate the harvest rate necessary to reduce the population to 200 gray wolves in Michigan, 1,600 gray wolves in Minnesota, and 250 gray wolves in Wisconsin in 5 years, assuming maximum past observed rates of lethal depredation control (as derived for Mortality Scenario 2 in Chapter 5 above) (Table A 9)." **I noted above that the claim that those rates are maxima is inaccurate. I can easily come up with higher rates of lethal control if I (a) expand the season to a full year, consider catch-up killing like Wisconsin did, add HHS killing, etc. and (b) imagine a slight legislative change to allow lethal control by private landowners, for hound losses, for farmed deer, etc.**

Recall the belief in state promises is a value judgment by FWS.

Also the following assumptions are value judgments: (a) state thresholds remain the same, (b) lethal control does not spur yet more farm animal losses spurring on yet more killing which 6 studies of wolf killing show can happen in a minority of cases (see above).

Footnote 32 **contains an error. MN is >200 miles from Michigan. THE FWS seems to be counting the distance from Nn to WI as <100 miles (correct) and the distance between WI and MI as <100 miles (correct) so treating MN and MI as within 100 miles (incorrect). Not sure if it matters but what if Wisconsin wipes out its wolves in between MN and MI then the latter two are isolated >200 miles apart?**

Apart from the above, the crux of Mortality scenario X is what harvest rate WOULD BE REQUIRED to lower the WGL states wolf populations to 1600 (MN) and 250 (WI) and 200 (MI) in

5 years. Several things should be noted about this model besides my issues with max rate of lethal control.

(A) The model assumes 5 years which is not conservative because they allow reproduction in between mortality events. In other words, a state can eliminate its wolf population in one year as Wisconsin attempted in 2021.
(B) the model assumes incorrectly that there is no super-additive mortality resulting from poaching, loss of litters, infanticide, or Allee effects harming small packs' or loners' abilities to reproduce when density is decreased by the harvest or catastrophes;
(C) poaching is undetected for years,
(D) wolf census methods are insensitive, inaccurate and imprecise in MN and WI at least.All of these assumptions of their mortality scenario X are NOT precautionary or conservative.

To suggest their Mortality scenario X is impossible, FWS states "(i.e., when wolves were almost extirpated from the lower-48 United States in the early 20th century), these eradication programs relied on unregulated and widespread use of poisons, along with unregulated harvest incentivized through bounty programs and the use of professional trappers. Currently, the regulatory landscape in the Western Great Lakes does n" **but there is no evidence to compare those historical mortality rates to current ones.**

The extirpation of past generations was coupled with endangerment of deer populations and clear-cutting forests. We have no data and on its face, it is simply not comparable to today (with telescopic sights on automatic weapons, snow machines, hounding, night-time hunting, and a higher human population). Therefore, we don't know if mortality rates during Wisconsins; worst wolf hunt exceed past rates but I suspect they did. It is a value judgment if one looks back to low-data historical anecdotes or recent Feb 2021 Wisconsin evidence with better data for the model of what can go wrong.

In short, for a pessimist like me, a state like Wisconsin can reduce its wolf population in one year by far more than the FWS anticipates in five years.

By simple precautionary modeling Treves & Louchouarn (2022, 2025) demonstrated that Wisconsin could easily have reduced its population below 250 in a single year with a second harvest. We also showed that even without a second harvest the disastrous Feb 2021 wolf-hunt could have lowered the population below 250 with a finite probability and undetected by the state monitoring system.

If one accepts my pessimistic outlook, the real question is what is the risk that all three states do this in the same 2 year period and if coincident with disease and black swan events of climate or prey shortage what is the result?

Had the FWS been truly precautionary as conservation biologists are supposed to be then it would have assumed one state like WI did the worst imaginable (killing almost all breeders and most of its population in a single harvest) and what is the probability that MI and MN do something similar in the year that follows (i.e., up to one year later because reproduction in WI would not resume normally for at least 12-24 months when breeders are lost (Brainerd et al. 2008))? If a disaster happens once it can happen again.

All I am trying to say is that a pessimist using science like me comes up with different estimates than an optimist like FWS also using science (albeit not BAS)— either approach is a value judgment not a scientific dictate.

Chapter 6

My last word and overall conclusion is that the FWS achieves their confidence primarily by (a) assuming state management plans will be followed without sudden catastrophic changes leading to wolf-hunts and policy spasms like inWisconsin in 2021 when a second hunt was planned for Fall 2021 with a quota of 300, after the unprecedented overkill of Feb 2021; (b) states count wolves accurately, precisely, reproducibly (a hallmark of good science), with sensitivity to changing conditions; (c) non-precautionary, non-conservative assumptions and erroneous claims about maximum levels of lethal control, maximum levels of human-caused mortality (HCM), (d) statistical independence between causes of death so they need not consider super-additive mortality or run-away poaching, (e) assumed density-dependent growth that accelerates at small sample sizes rather than Allee effects and depensatory mortality that slows reproduction at low densities, and (f) long periods of decline rather than sudden ones, followed by 95 years of return to their rosy model conditions. None of these (a-f) are cautious or conservative. Several do not match the best available science measured by replication of findings, quality of journals, or transparency of data and analyses.

And finally a comment on the way the establishment (state-affiliated researchers and FWS) respond to challenges to their claims. They publish some scurrilous response with errors and without transparent data-sharing then claim our challenges have been rebutted or refuted (their word). But when we publish a rebuttal to them they do not cite it or claim a rebuttal or refutation is present.

Moreover we the few challenging the establishment have limited time and resources to rebut and often have to litigate to Get access to their data. I'm pointing out the uneven playing field. It is easy to rebut with nonsense then ignore the challenge to orthodox, establishment views. Until the FWS and the courts actually examine the quality of evidence and methods, the transparent and reproducibility of science, BAS will never prevail.

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