Review of the United States Fish and Wildlife Service (USFWS) Proposal to Designate the Northern Rocky Mountains (NRM) Population of Gray Wolves as a Distinct Population Segment (DPS) and Remove this DPS from the Federal List of Endangered and Threatened Wildlife (71 Fr 6106)

By

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Review

I have read the proposed rule by the USFWS to designate the northern rocky mountain population of the gray wolf as a distinct population segment and removing this distinct population segment from the federal list of endangered and threatened wildlife (71 FR 6106). At the present time, I cannot scientifically support the proposed rule as written to both define the DPS and remove the population from the ESA. I summarize my reasons here, and then explain each reason in detail below.

First, I would like to state that I find the major conclusions of the USFWS with respect to the biological conditions of wolf recovery, including the numeric and distributional goals, to have been met and exceeded by all measures of population recovery. This has clearly been the result of decades of dedicated work, management and monitoring of dozens of agency and cooperating agencies personnel. Scientifically, the biological recovery of wolves is one of the most significant conservation achievements of the 20th and early 21st century, and will undoubtedly be recognized as such by environmental historians decades from now. I also comment below in general that I believe the proposed rule does an excellent job (with few exceptions) at presenting the biology, ecology, and management history of wolves in the NRM-DPS, and provides an adequate review of the factors affecting the species with the exception of point 4 below.

Despite scientific agreement about whether the population of wolves in the Northern Rocky Mountains has met recovery goals, I cannot scientifically accept the logic of the

USFWS's arguments to delist and re-define the NRM-DPS at the present time for these reasons. My reasons have to do not with whether wolves have been biologically recovered, but due to logical inconsistencies given the contingent nature of the proposal as it stands dependent on the faulty Wyoming plan. I also question some of the evidence used to argue for discrete population status given dispersal, and question the validity of the definition of suitable habitat.

However, my main stumbling block in my scientific review of whether delisting should occur would not be held up by these secondary points if it were not for my principle problem with the proposal, the logical biological inconsistency of the DPS and dependence on Wyomings position.

1. Inconsistent application of the logic underlying the definition of Distinct Population Segment and dependence on Wyomings' development of an acceptable wolf management plan

I believe there is no justifiable biological rationale for a redefinition of convenience of the DPS to redefine the status of wolves outside YNP in WY, while refusing to consider additional areas in bordering states as falling outside the DPS. First, I find the biological rationale and interpretations of suitable habitat (notwithstanding comments below) in adjacent states as reasonable scientific support for the definition of DPS for the NRM as adopted on pp 6111 of the rule. I accept the scientific rationale for the three criteria for the DPS; discreteness, significance of the population segment, and conservation status of the population segment. I think the USFWS has done a scientifically adequate and legally consistent job of defining these terms and coming up with a definition of DPS that meets biological criteria and is well justified. I do have one important scientific disagreement with the definition of discreteness of the population with respect to dispersal potential, which I discuss below in point #3 below. These might expand the definition of DPS to include areas further away, but these would not preclude acceptance of the DPS with some minor modifications. I therefore mostly support the definition of the DPS as recognized by Fig. 1 in the proposed rule, and agree with the USFWS that it is unlikely for wolves to become endangered again in the foreseeable future in this DPS area with responsible state management.

My critique starts because it was precisely this biological definition of the DPS that the proposed rule uses to argue against inclusion of areas in neighboring states including OR, WA, UT and CO. The proposed rule argues against including these additional areas based on the biologically meaningful definition of the DPS. The proposal concludes that "Expansion of the

DPS to include CO or larger areas of UT would have required significant expansion of the DPS". The biological rationale for coming up with the boundary of the DPS was, I believe, scientifically justifiable, and was based on the finding that suitable habitat in adjacent states contributed little to wolf recover. Thus, logically, the argument against including adjacent states depends on accepting this DPS as biologically and scientifically defensible, and I agree that the evidence presented does a good job of doing this.

However, in order to 'work around' the state of Wyoming's intransigence to adopt an approved (or approvable) wolf management plan, the proposed rule states on pp 6117:

> "we would reclassify the portion of Wyoming that is not a significant portion of the range and the portion that is in the National Parks in WY as "not listed". *The DPS would no longer exist*. The significant portion of the range that exists outside the National Parks within the state of WY would continue to be listed as "non-essential experimental" based on the biologically significant nature of that portion of the species' range….". [emphasis added]

To argue at length that the entire NRM-DPS should be considered when arguing against adding CO or UT to the example, yet to accept changing this DPS as a strategy to accommodate WY is inconsistent and is an illustration of the USFWS changing the interpretation of DPS to suit expediency without a firm biological or logical basis. Throughout the proposal, it is emphasized again and again that recovery must proceed with all three states linked through joint recovery targets; much effort is used to justify the DPS, and so forth. This option of arguing for biological definition of the DPS only to drop the definition just as a strategy to accommodate WY is illogical, scientifically indefensible, and opens the argument that CO and UT are also "biologically significant portions of the species' range". This re-definition of the DPS for reasons of management expediency would no doubt have important biological and legal precedence in other endangered species populations as well, and if this proceeds, will negatively affect endangered species management of other populations.

In conclusion, I disagree with the approach to 'reward those who have undertaken positive efforts to conserve the species and alleviate the threats posed by human-caused mortality" (p 6136). While I recognize the cooperative nature of endangered species recovery, the definition of DPS is contentious enough that to dismiss it to reward cooperative states when there is no strong biological rationale for considering WY separate from MT and ID is illogical and contradictory. In the spirit of the MOU struck between MT, WY, and ID, this problem must be resolved at the state level and then the USFWS must apply a consistent biologically sound definition of the distinct population to the entire NRM.

2. Scientific problems with the definition of suitable habitat

Habitat is one of the most misused and poorly defined terms in ecology (Hall *et al.* 1997), and can be meant to mean everything from the fundamental niche of a species, to its geographic range on earth, to specific vegetation and plant communities that a wildlife species is associated with. I found the de-factor definition of suitable habitat used for wolves in the proposal was tautological. Defining wolf habitat as only the areas in which we currently find wolves is circular logic, and will have the result of 'freezing' in time the current wolf distribution. Oakleaf *et al.* (2006) models follow this logic, and while they do a good job of capturing a snapshot of present wolf distribution, they do not address a critical factor; density dependence in habitat selection (Rosenzweig 1981).

Despite some references to stability in the distribution of wolf ranges within the NRM-DPS in the proposal, I know of know credible published scientific information that tests the hypothesis that wolf expansion within the NRM-DPS has stabilized to an equilibrium that would be required for the ad-hoc definition of wolf habitat used by Oakleaf *et al.* (2006) to be valid across a range of densities. Instead, one scientific publication at least, suggests the potential for continued population spread from YNP in the future in a density dependent fashion (Hurford *et al.* 2006). This suggests that wolf density itself is a habitat 'factor' following the theory of density-dependent habitat selection (Rosenzweig 1981). This theory predicts that, for a territorial species, the pattern of wolf habitat selection following recovery follows that of an ideal despotic distribution predicted by theories of density dependent habitat selection (Fretwell & Lucas 1970; Fretwell 1972) where 'good' territories fill up earlier, and poor territories fill up later. This is indeed what appears to have happened with wolf recovery.

The practical difficulty of this is that using the static definition of suitable habitat yielded by an analysis that does not consider effects of changing wolf density will result in overestimation of the area of suitable habitat if populations decline in the future. Alternately, the 'static' Oakleaf model will underestimate of the area of suitable habitat if populations continue to increase. There is no easy remedy to this problem of defining wolf habitat as a function of wolf density itself except first to address whether there has been spatial stability in the range occupied by wolves as suggested, without evidence, in the proposal. Then, models that explicitly link selection to density can test what true habitat potential at the highest densities could be as the true potential density.

My final comments with respect to the definition of suitable habitat are a statistical one. The rationale to use the cutpoint probability of 50% to designate occupied wolf habitat is not a rigorous or defensible method. Oakleaf used logistic regression to compare an unbalanced

number of used and unused putative wolf pack home ranges to model habitat selection by wolves in the NRM-DPS. Determination of the optimum cutpoint probability to discriminate used and unused locations in multiple logistic regression is best determined with the trade-off of specificity and sensitivity in sensitivity analyses following logistic regression (Hosmer and Lemeshow 2000; Fielding & Bell 1997; Liu *et al.* 2005). Practically, the optimum cutpoint probability for a logistic regression model depends on average prevalence across sites, and can vary from near 0 to near 1. For example, for a species that occupies many sites out of the range of possible sites, the optimum cutpoint probability may be closer to 1, and vice versa for rare species. For wolves, this could result in either over or under-estimation of the true amount of suitable habitat.

In the Carroll et al. (2003) models, I'm less clear that the methods used correspond strictly to the same constraints on predicting presence and absence based on cut-point probabilities. But because their habitat models yielded 'relative' measures of habitat ranking, these sorts of measures are extremely sensitive in their predictions to the underlying distribution of available habitat rankings (Johnson *et al.* 2006; Keating & Cherry 2004; Boyce *et al.* 2002). For example, if most habitats within a study area were at the higher end of habitat quality (a left-skewed habitat quality distribution), then using an optimal cutpoint probability of 0.5 would tend to overestimate the true median 50th percentile because use of 0.5 assumes a normally distributed habitat quality distribution about 0.5. This would result in under estimation of the amount of suitable habitat in right skewed habitat quality distributions, and could result in the same biases as for the logistic regression model in comparing predicted habitat potential for a species.

3. <u>Underestimation of the importance of dispersal in maintaining connectivity and the link to population size</u>

Both the proposal, and the landscape movement model of Carroll *et al.* (2003) most likely severely underestimates the role of long-distance dispersal in wolves. Thus, I scientifically challenge the scientific definition of discreteness used in the proposal and the argument to exclude CO, UT, OR, and WA. Mean wolf dispersal distance is reported in both the proposal and Caroll et al. (2003), and only a few long-distance dispersals were recorded. However, by the services own admission, only 30% of all wolves in the NRM-DPS have been collared, so long-distance dispersals are likely even more important. Moreover, detecting long-distance dispersal is very difficult, and definitely underestimated in most species (Shigesada & Kawasaki 1997; Kot *et al.* 1996). To examine connectivity between YNP and potential recovery areas in

Colorado, for example, Carroll et al. (2003) used mean dispersal distances. However, modeling work shows this vastly underestimates the rate of spread because the difficult to detect tails of the dispersal kernels are underestimated (Lewis *et al.* 1997; Turchin 1998; Shigesada & Kawasaki 1997). Therefore, including more realistic dispersal kernels instead of the de-facto assumed Gaussian kernel of Carroll et al. (2003) would increase habitat potential in areas adjacent to the DPS as recognized. This would at the very least enlarge the boundaries of the DPS, if not warrant further consideration of these areas to the DPS.

Moreover, I think not enough thought was given to the interaction between connectivity and population size in wolves and the potential for establishing unique DPS's in adjacent states that would certainly contribute to conservation of the species. For example, Hurford *et al.* (2006) showed that density dependent dispersal was critical in driving the rate of population spread from Yellowstone National Park. Maintaining wolves at higher than the minimum 10-15 wolf breeding pairs will help maintain connectivity within the DPS but also will provide opportunities to contribute to establishment of new wolf DPS's.

4. Failure of state plans to consider public wolf trapping as an important source or mortality

The present mortality rate for wolves is 26% per year, 75% of which is already human caused mortality resulting from control actions and illegal shooting. This does not leave much room above the standard and accepted 30% threshold for mortality to keep wolf population growth positive. While some human caused mortality may be compensatory, for example, as human harvest of wolves increases post-delisting through hunting, I believe that human-caused mortality resulting from trapping will may be compensatory, and instead, could cause wolf mortality to exceed the 30% threshold, especially given spatial variation in trapping harvest.

Wolf managers in the NRM-DPS do not have much experience with managing a recreational wolf trapping season. Where wolf trapping does occur, such as in Canada, trapping can often be the most important source of mortality (M. Hebblewhite, unpublished data, U of Montana). For example, in the foothills of Alberta, trapping is the leading cause of mortality, natural or otherwise, and comprises 60% of all human-caused mortality (M. Hebblewhite, unpublished data). In Canada, wolf and other trapping is managed using set trap lines where individual trappers can trap on 1 trapline. The trapline system was created to ensure sustainable fur harvests by spatially structuring trappers. With the current mechanisms to manage wolf trapping post-delisting in Idaho and Montana, I believe there will be high risk of high wolf trapping related mortality once the states open up trapping seasons for wolves.

Unfortunately, no information is given on the spatial structure of either wolf harvest or wolf trapping. Because wolf trapping could, under the US trapping system, occur anywhere, trappers could concentrate in key wolf population areas, leading to the local extirpation of wolves. This situation occurred within the last 5 years with wolverine trapping in Montana in the Little Belt mountains, which apparently lead to significant overharvest, and emphasizes the importance of approving state wolf management plans that have enough details to evaluate their efficacy a-priori.

5. <u>Failure to recognize the potential for new DPS's for wolves</u>

Under the definitions of distinct population segment and significant portion of the species range, it is conceivable that new DPS's in adjacent states could recover, requiring definitions of recovery targets for these new DPS's that have not been addressed. Its obvious that doing so in this review or proposal is outside the scope of this review or proposal, but at the very least, more specific details are required both here in the summary, the discussion of distinct population segments, and in the following statement on pp 6106' "Intention to use section 6 agreements to allow states outside the NRM DPS with service approved wolf management plans to assume management of listed wolves". More information is required to evaluate which states would be affected, and whether or not service approved wolf management plans would work towards establishing new distinct population segments, for example, as wolves recolonized and recovered in Colorado and Utah.

I would think that scientifically, the goal of the USFWS would be to recover this DPS while allowing for the potential within the former geographic range of the species, which includes CO and UT, to recover wolf populations into potential DPS's in the future. For example, the OR wolf recovery plan as reviewed in the proposal would consider wolves delisted within the eastern OR portion of the DPS when only 4 wolf packs were recovered. But insufficient information is presented for western OR. I don't think the goal here should be to do this, only that I think it scientifically important for the USFWS to leave options and language open for these conservation opportunities in the future.

6. <u>The ad-hoc measure of population viability for wolves.</u>

Given recent advances in Population Viability Analyses (PVA) for endangered and recovered species, I find it surprising and scientifically inadequate that a more formal PVA for the NRM-DPS has not been conducted. I think this is warranted to examine the viability of 15 packs in

each state as a function of the demonstrated stochasticity observed to date in the population as a result of overharvest, disease, and climatic variation interacting with livestock depredation. Throughout the proposal I was a bit worried just how many times since even 2000 that at least 1 of the states populations of wolves had dipped below this threshold number of 15 packs or 10 breeding pairs. Moreover, the planning horizon for evaluating population viability, 30 years or 10 wolf generations, appears to have no formalized rationale or corollary in the literature in which 100-year PVA windows appear to be the standard. I strongly encourage more detailed analyses to refine the recovery targets with the amazing data now in hand. This would be in keeping with the principles that have resulted in previous changes to recovery targets. These need not be addressed by the USFWS but perhaps the states when they take over management.

Specific Minor Scientific Criticisms

Pp 6114 – Section "Analysis for Significance", subsection Unusual or Unique Ecological Setting" – the term ecological cascade is not a commonly defined term in ecology and is confusing when used as a synonym, as I assume it is here, for trophic cascade.

Pp 6115 – Section "Analysis for Significance", subsection Unusual or Unique Ecological Setting" – I disagree with the statement "into a much more diverse, ecologically complex,….. than is found elsewhere within historical wolf habitat in the northern hemisphere, including Europe and Asia".

This is a particularly US-centric statement that has no scientific validity, as a quick review points out. The Muskwa-Kechicka management area, a 40,000km^2 area in Northern British Columbia, Canada, was the source of wolves for the second year of reintroductions to YNP and Idaho and was specifically chosen because of its high predator-prey diversity. Referred to as the "Serengeti of North America", the M-K is home to the following large carnivore species; wolves, grizzly bears, black bears, wolverine, lynx, cougars, bobcat, coyotes, fisher, marten and otter. The M-K is also home to very diverse ungulate guild including; elk, moose, white-tailed and mule deer, Stone sheep, woodland caribou, mountain goats, and bison in the Pink mountain herd on the eastern edge of the M-K (Gustine *et al.* 2006; Bergerud & Elliot 1998).

Moving to Asia, wolves exist in perhaps the most ecologically rich ecosystem in the world in Sikhote-Alin Zaopvednik reserve in the Russian Far East where they co-exist with the following

predators; Amur tigers, Amur leopards, Asiatic black bears, brown bear, lynx, sable, and wolverines at least. And the area is home to a rich ungulate community including red deer, roe deer, sika deer, moose, musk deer and wild boar (Miquelle *et al.* 1999). Finally, in eastern Europe in Bialoweiza National Park on the Belarus-Poland border, wolves co-exist with 4 other predators and 5 other large ungulates, similarly complex. Therefore, remove this false statement.

- Throughout the document, the recovery targets are defined many different ways either as 10, 15, or 10-15 breeding pairs per state. Its very confusing, and I think should be clarified.
- Spelling mistake on pp 6116 – 2nd column, 2nd paragraph – our, not out.
- Finally, why do the Idaho and Montana management plans differ with respect to stopping harvest at 10 for Idaho and 15 for Montana? In the proposal, its stated that Idaho will reduce their harvest once wolf breeding pairs declines below 15, whereas Montana will stop harvests altogether once the # of breeding pairs hits 15. Why do these differ? How can the USFWS accept the Idaho designation considering the excellent arguments on the relationship between probability of breeding and pack size.
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General Questions

1. **Is our description and analysis of the biology, habitat, population trends, historic and current distribution of the species accurate?**

   Yes.

2. **Does our document provide accurate and adequate review and analysis of the factors affecting the species?**

   Yes.

3. **Are our assumptions and definitions of suitable habitat logical and adequate?**

   I address this question above, and find some logical and statistical inadequacies in the definition of suitable habitat.

4. **Are there any significant oversights, omissions or inconsistencies in the**

proposed rule?

I think failure to include wolf trapping specifically, as well as the details of proposed harvest plans should be included for the state plans. Considerable detail is given for WY, but none for ID and MT. This should be required before approval to delist is given.

5. Are our conclusions logical and supported by the evidence we provide?

No, as outlined in point 1 above, your conclusions are self contradictory and appear to be designed to expedite a specific outcome regardless of the biological and logical contradictions.

6. Did we include all necessary and pertinent literature to support our assumptions and conclusions?

Yes.

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