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The following is my review of two 2 documents published in the Federal Register on 28 October 2013.

Document 1: Review of “Endangered and Threatened Wildlife and Plants; Threatened Status for the Bi-State Distinct Population Segment of Greater Sage-Grouse With Special Rule”

In general, I think this is a very thorough document. However, it is difficult to have to search for the literature to this document, and this process added to the time needed for review. The March 23, 2010 decision to consider the Bi-state population as a Distinct Population Segment (DPS) may be valid, but it is awkward to have that as a separate decision to this current rule. Because portions of the DPS decision (such as genetics) seem to have relevance to the current rule, the current format requires readers to go through the 2010 document in addition to the current document. In fact, the comments requested were often directly related to the earlier ruling which made it confusing as to which ruling was being considered.

In order to be clear, I also considered the information within the DPS ruling (March 23, 2010), since that ruling has ramifications on this ruling. There are multiple issues regarding a DPS decision. The first issue is ‘discreteness’ of the population segment. The justification for this appears to be largely genetic, likely as a result of long-term spatial isolation. I think this argument is supportable. The second issue is ‘significance’ of the population segment. Genetics also plays a role in this argument and once again I believe there is a strong case. The isolation of this population from other populations supports that argument as well. The third issue is whether the population segment’s status should be considered relative to the Endangered Species Act. Because that issue direction relates to the current ruling, I will consider that below.

There was a specific request for information on the following topics. I list them below with some comments after each topic.

Executive summary: On Page 64359 in the first paragraph there is a statement of “managed not by a formal SGI participant but are consistent with the SGI.” There may be more on this later, but this is very ambiguous as to what is consistent with the SGI.

Biology, habitat requirements: Little information was provided, but there is no indication that sage-grouse in this DPS differ from those found elsewhere.

Biology, genetics and taxonomy: I mentioned this above, but genetics is covered in the earlier ruling and is compelling. It should be noted that just because the DPS has isolated for a long time (perhaps thousands of years) doesn’t mean that the birds differ in any other meaningful way (i.e., behavior and/or morphology).

Biology, historical and current range: Historical range is based largely on distribution of potential habitat but I doubt the authors have much that can be proven without ambiguity. The historical range (Schroeder et al. 2004) suggests that connectivity existed with populations to the north, but the genetics

seem to argue against that. The information on the current distribution is also somewhat unsatisfying, especially regarding the Pine Nut and White Mountain areas. How can the White Mountain area be considered a Population Management Unit when nothing is known about the population? On page 64361 (last paragraph in middle) there is a reference to “four to eight demographically independent populations”. This might be true, but I am not aware of how this was demonstrated. For example, how would you know if the White Mountains are an independent population since you don’t know anything about the birds there? If there are 8 independent populations, then some population management units would contain more than one independent population. Table 1 (Page 64362) lists the estimated population sizes. It was very unsatisfying to rely on unpublished data for these estimates and the numbers seem to poorly reflect the lek counts, at least in some areas. It appears that some of these numbers were obtained by estimating the proportion of males counted on leks and considering the typical sex ratio. It would be nice to know what those proportions and ratios are. That still might not explain all of the estimates. Given that the authors mentioned 4-8 populations, wouldn’t it be possible to list these beneath each PMU?

Biology, historical and current population levels and trends: A cynic to this ruling would argue that the populations in the core portions of this range have remained somewhat stable over the last couple decades. The authors need to make clear that the issue of ‘risk’ is not only about the downward trends, but about the inherent risk associated with small populations. On page 64362 in the first paragraph (roughly in the middle) there is a statement about “Sage-grouse abundance declines...” It has become standard for statements like this to be repeated, but with little support. I went to the Garton et al. (2011) paper for clarification of what was stated for 2 of the relevant populations (perhaps metapopulations). For the Mono Lake population (perhaps lines up best with the Bodie, Mount Grant, and Desert Creek-Fales PMUs) there is a statement in the book (page 324) of “no consistent long-term trend over the 40-year period”. For the South Mono Lake population (page 325) there is a statement of “the population showed no obvious pattern through time”. These statements and the corresponding figures (15.4a and 15.4b, respectively) seem to argue against the blanket statements that these populations have declined or are declining. If the authors want to make the case that the populations have declined, the data argument should be stronger. In my opinion, the issue of whether or not the populations are declining is not the only issue, and in this case, maybe not the primary issue. The primary issue is one of risk. Are these birds at risk in this area? The answer is clearly yes, primarily because of small population size, lack of connectivity within the DPS (see demographically distinct population segments above) and lack of connectivity with sage-grouse elsewhere (genetic and demographic isolation). The authors risk their credibility by focusing on unsupported arguments of downward population trends. In the second paragraph on Page 64362 (first sentence) there is another reference to declining population trends. Aside from the trends that appear to not be downward, can there possibly be enough data to examine the Pine Nut area? By including that DPS in this sentence, the authors are implying that there is enough data to support this statement. By the way, in the supporting species assessment, there was an acknowledgement that there was not enough data for an assessment. There is also a mention of population redundancy here. I don’t think redundancy by itself is the key issue. In my opinion the key issues are area of occupancy and connectivity between the occupied areas. For example, if all the areas were connected and not demographically independent (as was stated earlier), there would be no redundancy and yet the overall DPS would be in better condition. On page 64363 (4th paragraph), the reference to the population size in the Pine Nut PMU was poorly supported. In the 5th paragraph on the same page there is a discussion about connectivity among PMUs. Connectivity among PMUs is an artificial construct (these are management units). I think what needs to be discussed here is connectivity among ‘populations’. This results in confusion in the following paragraphs. For example, near the end of the page there is a statement “This PMU is considered to be largely isolated from the

other PMUs". That statement is clearly incorrect since the PMU borders 2 other PMUs. On page 64364 (2nd paragraph) there is another reference to an isolated PMU. I think this confusion would be relatively easy to eliminate by addressing populations rather than PMUs. There are also references to populations of birds in this section. This would be easier for the reader to understand if the populations were identified in Table 1.

Listing determination: There are several issues here related to habitat, utilization, disease or predation, inadequacy of existing regulations, and other manmade factors: I believe it is important to focus on 'risk' in this discussion and not on past impacts on populations (Ok to focus on impacts to PMUs). My reason for this is the lack of long-term downward trends in the populations. In my opinion the high level of risk is enough to support listing this population, but there should also be acknowledgement that the populations in this DPS have not shown dramatic declines yet. The degree of risk could be addressed by consideration of the wildfires of 2012. For example, the largest wildfires within the range of sage-grouse were 223,000 ha (Long Draw in Oregon), 184,000 ha (Holloway in Oregon and Nevada), 126,000 ha (Rush in California and Nevada), 100,000 ha (Ash Creek in Montana), and 94,000 ha (Kinyon Road in Idaho). Any one of these fires in the range of the bi-state populations would have a severe impact on their viability. In general, the discussion of the issues (grazing, disease, predation, climate change, mining, urbanization, pesticides, etc.) is useful.

If one concludes that the risks are sufficient due to small population size, fragmentation, lack of connectivity, and other factors, then the five following factors should be considered before listing.

A. The present or threatened destruction, modification, or curtailment of its habitat or range

The authors have made their case for this issue.

B. Overutilization for commercial, recreational, scientific, or educational purposes

In the second paragraph on page 64373 the authors list disease and predation for this factor. I believe this is an error and that disease and predation should be listed under factor "C" (as they were listed on page 64370).

C. Disease or predation

Although these are not the most important issues, they are addressed by the authors. The largest issue associated with this may be raven predation associated with habitat fragmentation, landfills, etc.

D. The inadequacy of existing regulatory mechanisms

Because the species is not listed and most of the land is private, the authors are correct in stating that this is currently an issue.

E. Other natural or manmade factors affecting its continued existence

The authors have addressed this issue sufficiently.

On page 64373 (right side of the page under point 1) the authors make the point that "the current trend in abundance is unknown..." This contradicts the statement on page 64362 that "declining Bi-State DPS population trends continue..." Point 2 discusses the connectivity within and among PMUs which is not relevant. The topic should be connectivity among and between populations. In point 4 there is a reference to the trends of leks. I don't recall this being discussed earlier or data being presented. In point 5 there is a reference to the theoretical minimums for long-term population persistence. The real issue here should be the individual populations within the DPS since the authors have stated the most populations (4-8) are demographically isolated.

On page 64374 there is some discussion about the probability of population persistence (from Garton et al 2011). I believe that these risks are based on an interpretation of data collected in the past and that numbers are likely to be different if the rate of habitat loss and degradation increases. In the middle column and last paragraph there is a reference to 'resiliency' and 'redundancy'. I think the concept of resiliency makes sense, but redundancy does not make sense in this context. The way this concept is being used, 4-8 isolated populations in the Bi-state area would be better than 1 continuous population occupying the same footprint.

Overall: I believe there is a strong case for the population being considered a distinct population segment and I also believe there is a strong case for listing the species as 'threatened'. Even though I was not convinced of long-term population declines, I believe the case for substantial risks is clear.

Document 2: Review of "Endangered and Threatened Wildlife and Plants; Designation of Critical Habitat for the Bi-State Distinct Population Segment of Greater Sage-Grouse"

On page 64331 (left side under the section titled Critical Habitat Determinability) there is a reference to "critical habitat for the eight species is determinable". I'm not sure what the "eight species" refers to; probably an error.

On page 64335 (first full paragraph in center column) there is a discussion about connectivity habitat. I think this is OK, but it would be nice to have a map that shows what you are talking about. I was able to use the figure on page 64351 and figure 1 of the species assessment to get an idea of what was being discussed here. Presumably, the difference in the 2 maps includes the connectivity habitat (listed as critical habitat on page 64351 and not included as suitable habitat in Figure 1. It shouldn't be that difficult to figure this stuff out.

In Table 3 on page 64339, it is somewhat confusing to interchange unit numbers with unit names (the names are not in the figure on page 64351). I think it is easy to get confused between populations, critical habitat units, PMUs, suitable habitat, connectivity habitat, and different maps. For example, it is not clear to me why the critical habitat units (1-4) were not the same as the Population Management Units (PMUs). I believe this could all be simplified (and should be).

The quantity of protected habitat in Units 1 and 4 were quite large given the lack of solid population data from the Pine Nut and White Mountains PMUs). Does protection of potential habitat in the Pine Nut and White Mountain area (units 1 and 4) diminish the protection in the other areas?

In general I think use of a resource selection function (RSF) was in interesting way to classify critical habitat. It has the advantage of being less arbitrary than some other techniques. I do have a concern that is fundamental to the use of models. Development of an RSF is a good first step, but there was no indication in the document (unless I missed it) that there was any effort toward model validation or testing. What I mean by this is that development of the model is the first step. Testing of the model with a different set of data is the second step. The third step is revision of the model and/or application of the model to management. The second step helps to show that the model provides a good explanation for the presence (use) and/or absence (non-use) of birds. Although there was a reasonable effort to explain the RSF process, it is possible that the model development included this second step. If that is the case, then it is likely OK. Another positive sign is that it was clear, that modification of the critical habitat, based on new information is possible.