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May 3, 2019

Kat Powelson
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Dear Kat:

I have completed my review of the U. S. Fish and Wildlife Service Species Report titled "Bi-State Distinct Population Segment of Greater Sage-Grouse." In response to your email on 20 March 2019 in which you invited me to review the Report, I provide specific comments relative to the Biological Information Chapter in the attached "Species Report Review Comment Matrix" Excel file. However, I am not providing additional references after that section as it became clear that most of the literature reviewed in the Report only extends to 2011–2013 and I was updating content with newer literature. I am confident the U. S. Fish and Wildlife Service does not expect me to spend the days or weeks necessary as an invited reviewer to thoroughly review and then input information from current citations into the current draft of the Report. However, that is my main concern with the current Report. Otherwise, I found the Report to be well-written, well-reasoned, covers the most important information relative to the threats and status of the Bi-State Distinct Population Segment (DPS) of greater sage-grouse, and it includes the most relevant scientific information. In addition, there were only a few typos, but not enough for me to be consider them a problem. I cannot comment on commercial information as that did not seem to be a focus of the Report nor do I think it should be.

(1) Have we assembled and considered the best available scientific and commercial information relevant to this species?

The information that has been cited is solid and relevant. My only comment is the need to include more updated literature in the Report. As noted in my introductory paragraph, the literature that is cited in most cases is several years old or older, mainly extending to 2011–2013, and could be updated. Greater sage-grouse are one of the most well studied species, so there is plenty to cite. However, this could become obsessive and perhaps is unnecessary as the goal of the Report is to evaluate the threats and status to the Bi-State Greater Sage-Grouse Population.

Although I note the need to populate the Report with more updated literature, I am very impressed with the information that was covered and feel it is well organized and thorough. The scientific information that was presented was very relevant to Bi-State Greater Sage-Grouse. It was especially informative to see much of the research that has been conducted on the Bi-State Greater Sage-Grouse DPS presented as well as summary data related to each of the 6 Population Management Units. I would recommend one last step to finalize would be to validate that all recent papers on Bi-State Greater Sage-Grouse are included or were considered in the revised Report. Seems that missing those references would be a mistake given the need to better understand the Bi-State DPS of greater sage-grouse.

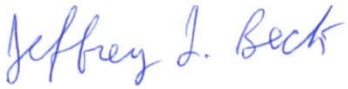
(2) Is our analysis of this information correct?

I have no issue with the analysis of the information. I did not find any instances where the authors of the Report incorrectly analyzed data. The information that was interpreted or summarized appears correct.

(3) Are our scientific conclusions reasonable in light of this information?

Yes, I believe that all conclusions that were drawn in each of the summary sections were reasonable in light of the information that was available. The main threats to the population including habitat fragmentation, connectivity, invasive species, exurban development, mining, wild horse grazing, and etc. were identified and discussed thoroughly.

Sincerely,



Jeffrey L. Beck, PhD
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Reviewer Name	Chapter	Page	Line #	Comment
Jeffrey Beck	Biological Information	10	31	Here is a great place where sagebrush community resilience and resistance as related to the indicators of soil temperature and soil moisture regimes could be cited to provide context . Please refer to Chambers et al. (2014), which appears in the References Cited section.
Jeffrey Beck	Biological Information	11	25	Please cite Pratt et al. (2017) in the same parentheses with Connelly et al. (2004) as the results reported in that paper are relevant and more provide an empirical analysis that Connelly et al. (2004) did not. Pratt, A. C., K. T. Smith, and J. L. Beck. 2017. Environmental cues used by greater sage-grouse to initiate altitudinal migration. The Auk: Ornithological Advances 134:628–643.
Jeffrey Beck	Biological Information	11	36	Could cite Pratt et al. (2017) who found greater sage-grouse interpreted direct indicators of resource quality, especially temperature, when timing movements between seasonal ranges. Timing of migration was also dependent on individual characteristics including location, reproductive status, and habitat use. Please see paper for more information on environmental cues, which were more pronounced for migratory birds, which responded more to changes in temperature which they responded to for summer and fall migrations. Pratt, A. C., K. T. Smith, and J. L. Beck. 2017. Environmental cues used by greater sage-grouse to initiate altitudinal migration. The Auk: Ornithological Advances 134:628–643.
Jeffrey Beck	Biological Information	12	35-41	Fedy et al. (2012) has perhaps the best movement information for males and females and for different life stages (e.g., nest to summer, summer to winter, and nest to winter locations). See Fedy, B. C., C. L. Aldridge, K. E. Doherty, M. O'Donnell, J. L. Beck, B.Bedrosian, M. J. Holloran, G. D. Johnson, N. W. Kaczor, C. P. Kirol, C. A.Mandich, D. Marshall, G. McKee, C. Olson, C. C. Swanson, and B. L.Walker. 2012. Interseasonal movements of greater sage-grouse,migratory behavior, and an assessment of the core regions concept in Wyoming. Journal of Wildlife Management 76:1062–1071
Jeffrey Beck	Biological Information	13	24-27	Dinkins et al. (2016) reports data on nest site selection and nest survival from 928 nests across several study areas in Wyoming. Please cite this with these other references. Dinkins J.B., K. T. Smith, J. L. Beck, C. P. Kirol, A. C. Pratt, and M. R. Conover. 2016. Microhabitat conditions in Wyoming's Sage-grouse Core Areas: Effects on nest site selection and success. PLoS ONE 11(3):e0150798.
Jeffrey Beck	Biological Information	13	34-36	I disagree with this statement. Please see Doherty et al. (2014) who did find an effect of grass height on nest success. Doherty, K. E., D. E. Naugle, J. D. Tack, B. L. Walker, J. M. Graham, and J. L. Beck. 2014. Linking conservation actions to demography: Grass height explains variation in greater sage-grouse nest survival. Wildlife Biology 20:320-325.
Jeffrey Beck		19		A RSF model is noted in the caption for Figure 1, but it is not depicted on the map. My sense is the suitable habitat identified on the map is based on the RSF analysis. Please clarify.
Jeffrey Beck				

Reviewer Name	Chapter	Page	Line #	Comment
Gail Patricelli		7	21	Very minor correction: Change "phylloplumes" to "filoplumes"
Gail Patricelli		18	6-9	It is unclear how these two mechanisms are identified as the primary drivers of bird distribution; many other possible impacts on distribution are discussed in this report, including degradation of remaining sagebrush habitat. Are these mechanisms relating to extent of sagebrush vegetation cover only?
Gail Patricelli		28	28-31	In addition to water management changes in Parker Meadow, it should be noted that the Parker Meadows lek is approximately 2 km from Highway 395 and <<1 km away from the June Lake Loop road, which is partially closed, but still has local traffic to recreation areas nearby. The lek itself is at the intersection of multiple two-tracks, which are used year-round by hikers to access the popular Parker Lake trail (my team observed the public walking near and through the lek area, often with dogs, in mornings and afternoons during the lekking season). I detail these sources of impact to point out that there are multiple human activities likely to impact sage grouse populations at the lek and in surrounding nesting/brood-rearing habitats.
Gail Patricelli		34	28-41	The Garton et al 2015 analysis assumes, as stated here, that past conditions persist into the future. Even with this assumption, the analysis predicts a worryingly high probability of quasi-extinction of the two core Bi-State populations over the next 30 years. But this report details many reasons why this assumption of unchanging conditions is incorrect, making it inappropriate to base management decisions on these predictions. Given increasing invasive grasses and related fires, climate change, increased human activity, and other impacts detailed in this report, this analysis will almost certainly under-estimate the probability of quasi-extinction.
Gail Patricelli		50	1,2	The use of the word "shifted" implies that normal activity was moved away from the impacted areas into surrounding areas without impact. Wording elsewhere that refers to "avoidance" and "habitat loss due to avoidance" is more appropriate.
Gail Patricelli		58	33-37	The potential for masking of lek vocalizations by industrial noise is addressed directly in: Blickley, J. L., & Patricelli, G. L. (2012). Potential acoustic masking of greater sage-grouse display components by chronic industrial noise. Ornithological Monographs, 74, 23-35. Besides masking, noise may also impact birds through direct disturbance (e.g. annoyance, distraction, fear, startle response); Blickley et al. (2012) found evidence of changes in the stress response on leks with experimental noise, suggesting that noise impacts even those birds who do not avoid leks (Blickley, J. L., et al. (2012). Experimental chronic noise exposure is related to elevated fecal corticosteroid metabolites in lekking male greater sage-grouse Centrocercus urophasianus. PloS ONE, 7(11), e50462. doi:50410.51371/journal.pone.0050462.)
Gail Patricelli		58-59	45-46, 1-3	These studies showing no impact or surface mining are concerning. The Brown and Clayton report is not peer reviewed and is not available online for evaluation. The Peterson et al. 2016 paper examined a single, small lek (~0-20 males) before and during mining activity and is therefore anecdotal. They consider each year's count to be an independent data point, despite these points being temporally autocorrelated and pseudoreplicated samples from a single lek. These results therefore cannot be extrapolated beyond the response of this particular lek to this particular mine. They also found no relationship between locations at which birds were detected and distance from the facility, but there was no baseline for grouse locations before mining for comparison and the observation transects extended <1 km around the facility, focusing on areas perceived to have the best grouse habitat. This study design would be very likely to detectavoidance effects, even if they occur. Based on what we know from other types of industrial development, such as oil & gas drilling, we would expect significant impacts on populations near mining activity, for the reasons cited on page 58. Further caveats need to be included to acknowledge the limitations on the science available to evaluate mining impacts. Based on prior studies, the report is correct on page 61 to state that negative impacts from additional mineral development are likely.
Gail Patricelli		61	37-42	This statement does not have adequate justification. The nearest inactive lek is 4km from the plant and the farthest is 9 km. That is within the range of many of the infrastructure impacts previously discussed. It is unclear why disturbance is assumed to be minimal. Did monitoring occur before and after construction of the plant?

Gail Patricelli		64	19-25	It should be noted that this study measured ICM levels in feces, which is typically considered a measure of baseline cort levels, as opposed to acute, stress-induced cort levels. Cort levels may be negatively related to fitness, but few (if any) studies measure this directly, relying on other assays of health. Studies of birds are also contradictory about whether increased or decreased baseline levels are associated with impacts to fitness-related measures. Blickley et al (2012, PLoS ONE) found an increase in fecal ICMs with noise playback, which was also associated with avoidance and altered display behavior on leks, suggesting that in sage-grouse, higher ICMs are associated with negative impacts. Regardless, the sentence "Research suggests that ICM concentrations and fitness are negatively correlated..." is not as certain as this wording implies.
Gail Patricelli		68	3-8	Does this remain true after the recent regulatory rollback at the federal level?
Gail Patricelli		72	1-5	This increase in cheatgrass cover over a short period of time (3-5 yrs) is very worrisome, and indicates that the threat may no longer be low in the Bodie PMU, as it assessed by the Bi-State TAC 2012.
Gail Patricelli		66	4-27	Recent experimental studies by Kurt Smith and Jeffrey Beck tested the impacts of mowing and chemical treatment on Wyoming big sagebrush and found evidence that mechanical treatments had negative effects on sage-grouse populations. Chemical treatments had only minor positive effects on populations and sagebrush protein content. The authors conclude that treatment benefits do not sufficiently outweigh the risk and costs to be justified in areas with Wyomingensis sagebrush. Smith, K. T., & Beck, J. L. (2018). Sagebrush treatments influence annual population change for greater sage-grouse. Restoration Ecology, 26(3), 497-505. doi: 10.1111/rec.12589; Smith, K. T., Forbey, J. S., & Beck, J. L. (2018). Effects of Mowing and Tebuthiuron on the Nutritional Quality of Wyoming Big Sagebrush. Rangeland Ecology & Management, 71(4), 417-423. doi: https://doi.org/10.1016/j.rama.2018.02.006
Gail Patricelli		98	11-15	In this busy area, which is near Mammoth Mountain ski area, impacts of recreational activities have the potential to be substantial, at least near some leks. In my experience, pedestrian recreational activity near a few Long Valley leks is extensive. People often visit hot springs or go fishing near sunrise and sunset, both critical times for sage-grouse activity. On multiple occcasions, my team has seen hot spring seekers with dogs wandering very near a lek during morning lek times. It is unclear how best to reduce this activity, but quantifying useage by pedestrians and OHV in this area is needed to assess the importance of this source of disturbance. Data from axle counters about the use of major paved roads approaching sensitive sites through the day may provide an index of
Gail Patricelli		99	18-20	More recent results support the negative impact of collars on male vocalizations in Nevada and Idaho populations (Fremgen, Marcella R., Daniel Gibson, Rebecca L. Ehrlich, Alan H. Krakauer, Jennifer S. Forbey, Erik J. Blomberg, Jim S. Sedinger, Gail L. Patricelli. 2017. Necklace-style radio-transmitters are associated with changes in display vocalizations of male Greater Sage-grouse. Wildlife Biology: wlb.00236.
Gail Patricelli		99	23-33	Wyoming and other states no longer allow collars on male sage-grouse, because these results suggest that collared males will not reproduce for the reasons discussed in the previous paragraph. This suggests that researchers should no longer be allowed to use collars on males, when other methods of attachment are known to have lower impacts on fitness.
Gail Patricelli		108	22	Change "gender" to "sex"
Gail Patricelli		113	21-25	The Connelly et al 2004 citation does not support the statement that 50% of males may reproduce. This section of the Connelly report discussed a study by Semple et al (2001) which found that 20% of broods had more than one father, but of the broods where females were observed to mate, the DNA paternity matched the observations from the field (which were extremely skewed). This suggests that field observations showing a small percentage of males mating might be somewhat under-estimating actual matings, due to occasional multiple matings, but it does not support an estimate of 50%. The study by Bush 2009 was in an small and rapidly-declining population at the northern periphery of the range (Alberta, Canada). Caution is required when extrapolating from this population to others, where observations of mating skew are sufficiently strong that this percentage seems implausible. Further data addressing paternity is needed to address generality of this result.
Gail Patricelli		116	31-32	See comment above. The Bush value for percentage of males mating is likely higher than most populations, leading to an over-estimation of effective population size

Gail Patricelli		116	28	Metrics to estimate effective population size are available that are more suitable for polygynous species, such as sage-grouse. For example, Nunney 1993 (The influence of mating system and overlapping generations on effective population size. Evolution, 47(5), 1329-1341. doi: 10.1111/j.1558-5646.1993.tb02158.x), which was used to estimate Ne for Gunnison Sage-grouse in the Stiver et al. 2008 study cited above
Gail Patricelli		117	Table 4	The values in Table 4 are very confusing. Are these ranges? If so, clarify how we should read the upper and lower ends of the range. What do the commas delimit in the numbers?
Gail Patricelli		133	29-32	Is this statement still accurate after the recent rollback of GRSG management plans at the federal level?

Review of "U.S. FISH AND WILDLIFE SERVICE SPECIES REPORT
BI-STATE DISTINCT POPULATION SEGMENT OF GREATER SAGE-GROUSE"

I found this report to be comprehensive, scientifically accurate and relatively complete. It is an impressive document. I have two major concerns. First, I am concerned about the status of the entire population if all but two of the subpopulations are at risk. I believe the viability of the remaining two subpopulations will be substantially reduced if the smaller and more vulnerable subpopulations are extirpated. Second, despite the tremendous conservation efforts made in the bi-state area I am not sanguine about the prospect of these efforts moving forward, especially in the absence of a listing decision. Such efforts are expensive and there will be pressure to cut back or reduce permitting restrictions as we move forward. Just look at what has been happening in Nevada and Wyoming since 2015.

I found a number of areas where I believe the review cold be improved by relying on more current literature. I have identified those and a few other issues below.

Page 5, lines 25-29. Low probability of persistence for all but two populations should be of concern because this will isolate and reduce probability of persistence for the healthy populations.

Page 5, Lines 38-39. I wonder whether efforts will continue without conservation concern.

P. 11, Lines 43-44. Gibson et al. (2014) and Jahner et al. (2016) suggest very low permanent dispersal rates.

P. 13, Lines 24-27. Shrub cover varies substantially from the eastern to the western part of the range. Several studies report more dense shrub cover in the Great Basin than further east (e.g., Kolada et al. 2009a,b).

P. 14, Lines 42-42. Blomberg et al. 2012 show that recruitment of new individuals necessary for population growth occurs following wet winters when nesting habitat is undisturbed.

P. 15, Lines 4-6. Gibson et al. 2016 show that nesting habitat and early brood habitat are spatially linked.

P. 15, Lines 15-17. Atamian et al. 2010 appropriate here.

P. 15, Lines 44-45. Sage-grouse don't really migrate in the strict sense of the term; they undergo seasonal movements between habitats. Just describe length of movements.

Page 16, Lines 33-37. Hagen et al. (2018) estimated adult sex ratio using capture-mark-recapture in a sage-grouse population.

Page 16, Lines 40-45. Blomberg et al. 2014 report lower postfledging survival which was dependent on habitat conditions. Most earlier estimates were not based on modern analytical methods and are potentially (likely?) biased.

Page 24, Lines 4-6. Is it population growth that is declining or the population itself? Text suggests the latter.

Page 24, Line 39. NDOW, no Fish in name.

Page 25, Lines 6-7. I think the authors mean the population, not population growth, is declining.

Page 33, Line 22. I suggest inserting "the probability of" in front of "long-term".

Page 34, Line 2. Stable trajectory just means the trend is consistent. Do the authors mean the population was generally stable?

Page 34, Lines 7-14. I understand the limitations but these lambda estimates don't provide much certainty about population trend.

Page 34, Lines 28-32. I don't find the quasi-extinction thresholds particularly helpful. They don't address potential changes in demographic rates going forward and variation in lambda estimates indicates substantial uncertainty about population dynamics.

Page 37, Lines 13-15. This doesn't seem correct? Maybe depends on how inhabited is defined.

Page 45, Lines 6-8. We must keep in mind that many of these earlier studies could not separate the effects of the roads themselves from the quality of habitats near roads. Large roads are often constructed in topographically simple and low lying areas where habitat for sage-grouse may be of poor quality for other reasons.

Page 46, Line 42. What does close proximity mean, < 1 km?

Page 47. Lines 1-2. Hwy 120 has been there for a long time. Are the declines mentioned here a continuation of earlier decline or do they suggest other changes in the environment for sage-grouse?

Page 47, Lines 18-19. Not sure this terminology is clear. Does it mean the roads were closed to vehicles?

Page 47, Lines 20-23. Not necessarily. If there is any density dependence impacts could merely cause population to settle at a lower level. Thus, persistence in the face of structures doesn't necessarily mean the structures haven't had an effect.

Page 50, Lines 1-2. Evidence suggests that this response results from colonization of the line by predators, rather than the presence of the line itself (Gibson et al. 2018).

Page 50, Lines 13-16. A number of these studies could not differentiate effects of the line itself from those of habitat quality along the line corridor. Probably should add qualifiers where appropriate.

Page 56, Lines 4-6. Ravens are also important predators on chicks.

Page 56, Lines 7-8. Could cite Gibson et al. 2018 here.

Page 57, Lines 6-8. Do the authors mean no new structures or that the rate of development will be constant?

Page 61, Lines 26-28. I believe USGS is finding substantial "indirect" effects associated with noise from a geothermal plant in northeastern NV.

Page 63, Line 2. 20th century?

Page 63, Lines 35-36. Doesn't this contradict the earlier finding by Gibson et al. cited above?

Page 64, Lines 30-33. There is a serious error in Evans approach. She calculated SG/area and ungrazed meadows tended to be larger than grazed meadows so her approach calculated more use of grazed meadows. When the analysis is done correctly there is no difference between grazed and ungrazed meadows. Additionally, these older studies did not account for detection and SG are almost certainly more detectable in heavily grazed meadows than in ungrazed ones.

Page 67, Lines 40-44. Riparian areas may be most limiting for SG so the number of these that are negatively impacted could very well be an issue especially for chick survival.

Page 70, Lines 5-7. The greatest negative impacts of livestock can be erosion and downcutting of channels, which de-water and shrink meadows.

Page 70, Lines 35-37. I don't disagree that we don't have good data on grazing impacts although such data will be forthcoming from several studies. That said I believe grazing impacts tend to be underappreciated because the greatest impacts occur on relatively small mesic areas. These mesic areas control population dynamics over much larger areas because they are absolutely essential for the recruitment of young. Most sage-grouse populations are stable or declining thus negative impacts on any demographic rate are likely to accelerate decline. Damage to, or shrinking, meadows potentially reduces recruitment into local populations with negative consequences for population health.

Page 72, Lines 24-26. But are these sites also becoming less suitable for sagebrush communities in general?

Page 79, Lines 4-7. Blomberg et al. 2012 also relevant here.

Page 83, Lines 22-24. Isn't work by Christian Hagen's students showing that SG are using burned areas in southern Oregon?

Page 86, Lines 39-42. In what way, move upslope, etc.?

Page 88, Lines 38-41. But if areas are less suitable for cheatgrass won't they also be less suitable for healthy sagebrush communities?

Page 92, Lines 11-15. The idea is more complex than this. It is now thought that compensation operates at least in part through individual heterogeneity in frailty (mortality risk). That is, individuals with a higher probability of dying anyway tend to be more likely to be shot by hunters (Burnham and Rexstad 1993, Lindberg et al. 2013).

Page 92, Lines 22-24. Their logic is seriously flawed because much natural mortality occurs during or just after the hunting season but before winter (Blomberg et al. 2013,2014).

P. 92, Lines 25-26. Connelly et al. 2000 should not be cited to support this statement because their methodology was seriously flawed. They "estimated" winter mortality as the proportion of birds marked in spring that subsequently died the next winter. Because most birds in the sample had already died they observed few mortalities during winter. The appropriate approach would be to assess how many birds alive at the start of winter died during winter.

P. 92, Lines 37-39. But 2/3 of the adult population is believed to be female (Hagen et al. 2018 and references cited therein) so these data suggest females are harvested in proportion to their availability. Additionally, Blomberg et al. (2013) showed that successfully breeding females suffered high rates of fall mortality irrespective of hinting mortality, which would be consistent with the idea that hunting is largely compensatory.

P. 92, Lines 39-41. Sage-grouse have almost identical life-histories to dabbling ducks (annual survival of adult females ~ 55%, clutch size 5-10, etc). Harvest is largely compensatory in ducks.

P. 92, Lines 42-45. See above on the mortality rates of successfully breeding adult females.

P. 93, Line 4. See Sedinger and Rotella (2005) in response to Connelly et al. 2003.

P. 93, Lines 24-26. It is possible (likely?) that harvest rates of 30% are approaching additive harvest mortality or may actually be additive. Where we actually have data, based on band recovery rates, harvest rates are typically < 10%, and in most places well below 10% (Sedinger et al. 2010, Hagen et al. 2018, Sedinger unpublished data for Eureka County).

P. 95, Lines 14-15. It's been awhile since I reviewed Gibson et al. (2011) but I believe there are some problems with their analysis. First, they did not directly assess the relationship between harvest rates and survival rates so other factors could have been influencing population dynamics. I also recall that in some cases population decline started before harvest regulations were liberalized or started to increase before regulations were restricted.

P. 99, Lines 18-20. Fremgen et al. (2017) is appropriate here.

P. 99 Line 44- P. 100, Line 2. This is not quite accurate. Gibson et al. showed that visiting nests caused virtually no impact on nest success (~ 1%) because nests that failed after being visited would have failed anyway because of attributes of the nest sites or the females themselves. The fact that nests failed a few days earlier than they would have if not visited caused a substantial negative bias in the "estimate" of nest success but not nest success itself. This bias results from a reduced number of exposure days in visited nests, which reduces the estimate of daily nest survival.

P. 100, Lines 10-12. I believe there is now a manuscript in review comparing VHF with GPS transmitters that confirms Blomberg's earlier finding.

P. 100, Lines 28-29. With the 50% lower survival rate of GPS collared birds loss rates will be much higher.

P. 101, Lines 7-9. Is there any evidence this worked?

P. 103, Lines 27-31. If the mortality rate was as reported here recruitment should have been reduced to a greater extent. I wonder if the 2-4% reduction in recruitment applies only to the effect of increased mortality in the marked sample and not the population as a whole.

P. 108, Lines 31-34. Blomberg et al. 2013a,b show that substantial mortality occurs in late summer and fall, and much of this is associated with predation.

P. 109, Lines 35-38. Many of these studies overestimate nest success because they exposure of the nest to predation during egg laying (Kolada et al. 2009, Blomberg et al. 2015).

P. 110, Lines 16-18. Gibson et al. (2016a,b) provide estimates of prefledging survival and Blomberg et al. 2014 provide estimates of postfledging survival. Blomberg et al. (2013) provide estimates of predation rates on sage-grouse.

P. 110, Lines 24-25. Data don't have to differ from other studies, there just have to sufficient data to overwhelm the effects of the priors during estimation.

P. 110, L. 29. Overall, there is an over-reliance on Connelly's work. Jack deserves much recognition for his contributions but much of this work relied on approaches that are no longer believed to be statistically valid (see earlier comment) and there are often better citations.

P. 110, Lines 29-30. Kolada et al. 2009 and Gibson et al. 2016 along with Coates' work are better references for Nevada and California.

P. 111, Lines 10-11. Gibson et al. (2018) is appropriate here.

P. 111, Lines 22-24. Gibson et al. (2018) also.

P. 113, Lines 6-7. I'm not sure I agree. The increase in ravens is unprecedented and reductions in any component of recruitment could be sufficient to cause population decline.

P. 113, Lines 39-40. Gibson et al. (2018) explicitly show that ravens associated with a transmission line are also associated with local population decline.

P. 114 Line 45- P. 115 Line 3. Jahner et al. (2016) show fine-scale genetic differentiation of sage-grouse populations at relatively small scales (ca. 10 km), indicating little to no demographic exchange. This was from a different population but suggests movement may be quite restricted. Overlap in seasonal habitat use is not an indication that dispersal between breeding populations occurs.

Page 123, Lines 11-13. Where does Nevada's Conservation Credit System fit into this scenario?

P. 140, Lines 32-34. Statewide permitted take, so only some proportion would apply to the bi-state area and this wouldn't apply to the California portion.

Page 142, Lines 3-30. These are great goals but are they realistic. Can fire be reduced? Can new energy development be resisted, check Wyoming? Can mining be regulated?

Page 144, Lines 36-41. This is key. And what happens if marginal populations are extirpated?

Page 145, Lines 1-5. Is this based on genetics or observed movement of radio-collared birds? If the latter there may not be a genetic linkage through interbreeding.

Page 145, Lines 27-36. It would seem that if the strategy is maintaining the core populations, Mt. Grant is very important as it is essentially contiguous with Bodie and likely plays a demographic role in the maintenance of the Bodie PMU.

Page 148, Lines 23-30. Some of the effects reported by Wisdom et al. may reflect confounding between habitat suitability unrelated to infrastructure and the effects of the infrastructure itself. That is, the habitat would have been poor even if the infrastructure wasn't present. That said, Gibson et al. ()2018 showed clearly that transmission lie effects extended to several km even when habitat quality was accounted for.

References

Blomberg, E. J., D. Gibson, and J. S. Sedinger. 2015. Biases in nest survival associated with choice of exposure period; a case study for North American upland game birds. Condor 117:577-588.

Blomberg, E., D. Gibson, J. S. Sedinger, P. Coates, and M. Casazza. 2014. Carry-over effects and climatic conditions influence the post-fledging survival of greater sage-grouse. Ecology and Evolution 4: 4488–4499.

Blomberg, E. J., D. V. Nonne, M. T. Atamian, and J. S. Sedinger. 2013. Seasonal reproductive costs contribute to reduced survival of female greater sage-grouse. Journal of Avian Biology 44:149-158.

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Blomberg, E. J., J. S. Sedinger, M. T. Atamian, and D. V. Nonne. 2012. Characteristics of climate and landscape disturbance influence the dynamics of greater sage-grouse populations. Ecosphere 3:1-20.

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