

Peer Review Report

Reviewer 2

Species Status Assessment for the Grizzly Bear

(*Ursus arctos horribilis*)

In the Lower 48 States:

A Biological Report

U.S. Fish and Wildlife Service

Grizzly Bear Recovery Office

Missoula, Montana

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Reviewer 2

Peer Review of the Draft Species Status Assessment for the Grizzly Bear (*Ursus arctos horribilis*) in the Lower-48 States: A Biological Report.

GENERAL COMMENTS

DO NOT PUT THIS YELLOW HIGHLIGHTED SECTION INTO THE PUBLICALLY AVAILABLE VERSION OF MY COMMENTS AS IT IS DIRECTED TO THE FWS STAFF TO HELP THEM LOCATE THE REFERENCED PORTIONS OF THE DRAFT REPORT. For ease of reading, I printed off this document and the page numbers referenced below in my comments derive from this. Late in the review process, I discovered that the page numbers on the original electronic copy did not match my printed copy (e.g. page 138 in the original was numbered 144 in my copy). I've gone through my comments to add section references in most cases to help locate the portion of the manuscript referred to.

1. Is the Biological Report’s description and analysis of the species’ needs, biology, habitat, population trends, and historic and current distribution accurate and, if not, what information is missing and how is it relevant?

Some comments and suggestions related to this query are included below under other queries. Generally, the report does a pretty good job on biology, population trends and distribution. Comments below for this query for the most part are my views of improvements that would be useful to consider.

Completely missing from this assessment is a categorization and listing of what organizations and agencies involved in recovery efforts are doing, where they are doing it (the “ecosystems”), and the magnitude of their financial commitments. This economic information is a “need” identified in this query #1. Grizzly recovery is and has been a collaborative process and needs to continue to be. This is needed for more than just recognition of the efforts made by participants; it is a vital component of these efforts that institutions have been and will continue to work collaboratively and with significant financial contributions. Recovery efforts are not a cost-free undertaking and it is important to begin to understand what these costs are, what activities incur them, and who is bearing them.

I think this report could usefully initiate the process of developing the monetary value of these contributions by, at least, indicating who is doing what and where, perhaps in tabular form, and providing an alert to collaborators that they will be asked to provide actual quantitative monetary values for the next 5-year report. For this report, this start could perhaps begin by providing number of FTEs (full-time equivalents) for the various agencies dedicating wholly or in part to grizzly recovery efforts broken down by activity. Generally, I think the values should be retrospective and include the period since the last SSA. For example:

activity	GENERAL (NOT SPECIFIC TO ECOSYSTEM	GYE	NCDE	CY	SE	BE	NC

CONFLICT RESOLUTION, BEAR MANAGEMENT SPECIALISTS (FTEs)							
DOI AGENCIES, LISTED INDIVIDUALLY (e.g. FWS, USGS, NPS, BLM, ETC.). CONSIDER, ALSO, TRYING TO INCLUDE LITIGATION DEFENSE COSTS FOR THE FWS.	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs
DOA AGENCIES, NATIONAL FORESTS LISTED SEPERATELY)	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs
STATE WILDLIFE DEPARTMENTS (LISTED INDIVIDUALLY)		Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs
TRIBAL WILDLIFE AGENCIES (E.G. SALISH=KOOTENAI, BLACKFOOT), NEZ PIERCE, ETC. LISTED INDIVIDUALLY.)		Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs	Number FTEs

Other categories listed at the same order as the Bear Management Specialists would include, for example, Research, developed facilities management for bears (e.g. lots of this for NPS and USFS), motorized road/access management (especially USFS), habitat/connectivity acquisition (e.g. by land trusts, state land acquisitions), capital outlays (e.g. equipment, this to be quantified for the subsequent 5 year report), planning (e.g. state agencies and USFS), recreation management, etc. Clearly, it is appropriate to clearly state that whatever values are entered here are approximate and placeholders for more precise evaluations in subsequent 5-year plans. For this report, approximate values might best be obtained by querying appropriate members of the IGBC. A suggested table like this might be appropriate to include as an appendix to this report.

The value of putting some kind of cost estimate on recovery efforts is illustrated by emphasis on the importance of “science-based management” in the “summary of cause and effects in the GYE”

(page 190) and NCDE (page 192). Continuation of science-based management depends on the continued willingness to shoulder these costs by involved institutions so it is worthwhile documenting what these costs are.

Additionally, Chapter 7 (Future conditions) provides alternatives where levels of conservation which is largely a function of costs. Therefore, costs are recognized as an important consideration but no values for existing costs are presented in this report. See also the discussion below of the concept of “conservation reliance” as defined by Scott et al. (2005—a needed additional citation currently missing).

As discussed below, it would be worthwhile too to include contributions from actively participating NGOs in this table too.

2. Does the Biological Report provide adequate review and analysis of the factors relating to the overall viability of grizzly bears in the lower-48 States (e.g., demographics, habitat, disease and predation, and genetics) and, if not, what information is missing and how is it relevant?

I question the 3x weighting given to the persistence factor for “abundance” where “high” is >800. If this 3x factor is maintained, then it should be better justified and supported with reference to modeling efforts or studies. Figure 11 (page 9) in the Boyce et al. (2001...reference below) IBA monograph, shows a flattening of the expected time to extinction for 4 modeled values of growth rate (r_0) using Cabinet-Yaak demographics for grizzly bear populations of 100-150. Based on this, it is hard to justify this high weighting for abundance compared to the other factors in this computation for resilience. At a minimum, I would suggest including, as well, a version of Table 5 with lower weights for “abundance” and see what the outcomes are with regard to calculated “resiliency” value; this would bracket Table 5 with another estimate less weighted by this abundance factor if it cannot be well-supported. It might be acceptable to include these alternative calculations in an Appendix that is identified in the main text.

In similar fashion, it may be that the resiliency factor for population trend should be expressed in some way that reflects proximity to carrying capacity in the ecosystem. A population that isn't growing because it is at or near carrying capacity such as in core areas of the GYE (a fact that is well-developed in this report), isn't necessarily less resilient than a growing one such as in the NCDE with regard to this one factor. The unhunted population in Denali National Park, for example, isn't “growing” (Keay et al. 2018) but is likely just as or more “resilient” than a heavily hunted one outside the park (Miller et al. 2003 citation provided below) and Keay et al. (2018 citation provided below). A positive lambda means, at least in part, that populations are below carrying capacity and there is room for growth. This can occur for reasons other than “resiliency” such as from hunting in the case of Alaska and, potentially in the future, in the lower 48. The current assumption in this report is, in contrast, that unless there is a positive lambda, the population is at higher risk compared to one with a positive lambda. I don't know quite how best to remedy this but, at a minimum, this issue should be recognized and discussed.

In chapter 6 in the section on Inter-ecosystem connectivity for the BE (rated “very low”) (page 206), I suggest this be changed to “low”. It is particularly inconsistent since the text below this ranking

says, “the natural colonization of the BE is now likely; I suggest that the ‘now’ part of this be replaced with ‘more’”.

Further, I think there should be more explicit discussion with regard to the BE of problems associated with black bear baiting in the BE; this greatly increases the likelihood of mistaken identity kills of any bears that move into the BE and any females that manage this are too valuable to be shot by black bear hunters who aren't expecting to see grizzlies. Montana has a bear ID program and this report would benefit from an analysis of how effective this is or isn't in terms of reducing mistaken ID kills which, theoretically at least, should be very rare at bait stations by hunters adequately trained to ID bear species. If this isn't the case, then the reliance on less aggressive approaches to train hunters in ID would likely be less successful than the somewhat more aggressive approach in MT. Perhaps some useful information could also be gained from the bear identification program that exists on Admiralty Island in SE Alaska even though this, like a now-abandoned effort in the Yukon, focused more on identifying bear gender than species (a more difficult challenge).

There is a long section (Genetic health and connectivity, pages 154-157) on potential genetic problems associated with isolation and lack of connectivity. It is noteworthy that there is no evidence of which I'm aware of there being actual documented problems for bears associated with these theoretical concerns which are widespread throughout the world. I think the section on the section on “other indicators of genetic health” is therefore important to provide balance to the genetic concerns associated with isolation that have been widely and used as reasons for not delisting GYE bears especially. It is important to keep this section in and perhaps, even, to expand on it with examples from other mammal species. For example, all living black-footed ferrets (*Mustela nigripes*) are descendants of 7 genetic founding individuals bred in captive breeding facilities whose offspring have been re-introduced into 28 sites. These have been widely re-introduced into habitats (4 species of prairie dogs) dissimilar to that of the founder individuals (a white-tailed prairie dog colony). Although ferrets are not, so far, a conservation success story, I am aware of no evidence that genetic issues are retarding ferret recovery efforts regardless of this very small founding population. There is evidence of small population genetic maladaptation for cheetahs and captive-bred Arabian Oryx. I emphasize that I do not discount the validity of concerns over isolation and increasing homozygosity and the value of larger genetically diverse population sizes over smaller and more homozygous ones. Only I think these concerns need to be put into perspective relative to what is actually demonstrated or known.

I was unaware of and interested in the reference to Kasbohm et al. (1994) in this regard but suggest that unless this is published in a peer-reviewed journal somewhere that consideration be given to eliminating this reference that is a demonstration of an observed genetic problem associated with isolation and inbreeding. I read the related paper mentioned in this section too (Draper et al. 2017) and these authors also found very low levels of heterozygosity in these 2 small and isolated black bear populations in Alabama (one recently established by reintroduction). However, they repeated no observed abnormalities connected to this as reported in Kasbohm et al. (1994). The Draper et al. (2017) paper therefore would seem to support my suggestion that such concerns are more theoretical than demonstrated as both populations had upper limits on population size estimates <166 individuals and point estimates <100.

There is evidence that rare color morphs in black bears (Glacier and Kermodi) may become more prevalent in isolated populations. These are not deleterious but may be worth mentioning in the context of consequences of restricted gene flow.

Lewis, T., et al. 2020. Unraveling the mystery of the glacier bear: Genetic population structure of black bears (*Ursus americanus*) within the range of a rare pelage type. *Ecology and Evolution*. <https://doi.org/10.1002/ece3.6490>

3. Does the Biological Report provide accurate and adequate review and analysis of the current and projected future condition of the species? If not, what information is missing and how is it relevant?

The discussion of the North Cascades should be updated with the recent reversal by the FWS (July 2020) to not proceed with an EIS and the litigation challenging this. This should include a discussion of whether USFS plans on motorized access (e.g. p 104) will be affected by this DOI decision.

In Figure 20 (page 93) it is unclear to me what the letters mean in some of these boxes (e.g. “F, B, S, D” in the box for “large intact blocks of land”. Similarly, for the letters in the boxes for “cover”, high caloric foods” and “dens”. Perhaps these could be identified in the caption for this figure.

The discussion in the section on “Food Synthesis Report” for the GYE (starting page 170) is very well done and the discussion of the errors in the Pease and Mattson (1999) paper well done and clear. This is important because of the court decision with respect to white bark pine and grizzly bear mortality.

It might be worthwhile to mention earlier in this discussion on WB pine, however, the fact that whitebark pine is gone in the NCDE and yet the population there has nevertheless grown and expanded. (This isn’t mentioned until page 174 (GYE diet diversity).

It might be worthwhile to cite Miller et al. (2003) and McLellan (1994—already in Lit cited). Both conclude that brown bear populations are maintained at K by density-dependent effects:

Miller, S. D., R. A. Sellers, & J. A. Keay. 2003. Effects of hunting on brown bear cub survival and litter size in Alaska. *Ursus* 14:130-152.

The discussion of food resources in the North Cascades should include mention of the findings of Robbins et al. 2018 (JWM). “Salmon poisoning disease in grizzly bears with population recovery implications”. This paper suggests that reintroduction of bears to the North Cascades of bears from interior populations with no history of salmon consumption are subject to “salmon poisoning disease” originating in an encysted fluke. Authors of this report should contact Robbins as I am aware, he is working on an update to the paper mentioned and this may now be citable.

In the section “summary of cause and effects on the NCDE on page 193 (second to last paragraph) there are a list of “potential threats” and a conclusion that these “are not a significant concern for the NCDE population. I would say “are not” is an overstatement and it would be more accurate to say, “have not yet been demonstrated to be a significant concern”. I didn’t see a similar sweeping conclusion like this for the GYE and don’t think it is justified for the NCDE. I agree that these are “unpredictable” as stated. Similarly in the next paragraph it would be more accurate to replace “we do not consider them [stressors] to be threats...” with something like “...we do not consider

them *currently* to be significant threats to the long term persistence....” Or “based on current information we do not....” Either way, it would be appropriate to add something afterwards about the general resilience and adaptability of generalist and omnivorous species like grizzly bears.

In Chapter 7 (Future conditions) the best-case scenarios (4, and 5) for the BE do not include or even mention implementation of the existing Record of Decision for reintroduction. Instead, the best-case scenarios here deal only with natural recolonization. Natural recolonization is a very weak reed on which to pin a best-case scenario for recovery in the BE. Scenario 4 (for certain) and perhaps Scenario 3 as well should involve implementation of the ROD which is still in effect. Given the very recent decision to stop the planning process for the North Cascades, I believe too that the best-case scenarios for this ecosystem should also specify active reintroduction as outlined in the draft EIS for this ecosystem. One place where this needs to change for each ecosystem is just below Table 21 on page 233 where active reintroduction into the North Cascades is mentioned.

I found Appendix C (Core habitat analysis for the San Juans and Sierra Nevadas) interesting. However, I suspect including it may create more problems than it resolves with respect to this SSA. If it is included, it would seem to mandate some kind of viability analysis for population establishment in these 2 areas similar to that done for the BE and North Cascades. At least an indication of whether such an analysis is being contemplated so the potential of these areas can be compared to the BE and North Cascades.

4. Does the Biological Report provide adequate review and analysis of stressors and other influences on grizzly bears in the lower-48 States? If not, what information is missing and how is it relevant?

In the list of problems associated with motorized access (page 98) for the North Cascades and elsewhere for the other ecosystems), I suggest that bullet point 2 (increasing displacement from important habitat) be expanded or rephrased to include both high value and lower value areas of occupancy. The “importance” of undisturbed habitats isn’t limited to those areas that are currently recognized as being important in some biological way such as for food, dispersal, general movements, denning, etc. If bears are discouraged or impeded from using areas adjacent to or within these recognized “important” habitats by motorized access or other disturbances then this will have a negative effect as well. This is especially the case if these areas of “lesser” biological importance are also mortality sinks. Bears are wide ranging species who move across a wide variety of places adjacent to or, even distant from, areas of high value for some biological function. Probably worth recognizing this. I am unaware of much discussion of or literature on the importance of habitats not classified as of high biological importance but I think the concept is one that should be better recognized especially in areas like the CY and SE where the total area of available biologically important habitat is limited. I recommend modifying point 2 to include lesser important areas of habitat and listing this first as #1 followed by current point 4 listed second and current points 1 and 3 listed as 3 and 4. Additionally, bullet point 4 (high food value habitat) is a subset of bullet point 2 that pertains and would be better to list listed after 2. I don’t think Figure 20 has to be modified to reflect inclusion of “lower value” habitats as it is adequately captured by the blocks of “Habitat factors” and “large intact blocks of land”. This comment pertains generally to all ecosystems discussed in this report.

In Chapter 5 missing from the list of “stressors” is recognition of the mortality sources posed by habituation or use of attractant areas like apiaries, chicken coops, livestock bone piles, ungulate

carcasses and/or spilled grain on railroad tracks, road kills of ungulates, etc. This would fit as a bullet point along with those currently listed under “sources of human-caused mortality, including...” (page 91). Certainly, these are far more significant stressors than, for example, “earthquakes and volcanic eruptions”.

Additionally, it might be worthwhile to list at least in this list on page 91 potential risks associated with interspecific interactions such as with wolves. Overall, bears probably benefit more from scavenging on carcasses of animals killed by wolves or cougars than they suffer from it but young bears may be killed by wolves occasionally in disputes over carcasses. This is a minor point.

Chapter 5 (“stressors and conservation efforts”) appropriately focuses on stressors. However, some of these activities have, at times, beneficial effects that should at least be mentioned. Vegetation management, for example has problems associated with increased roads, human access and consequent mortalities and disturbance which is adequately covered but, at times and if properly done can also improve availability of some patches of food. Even lands reclaimed from mining (mentioned later with a citation) can potentially provide improved habitat for grizzlies. I suggest that some mention in the introduction to Chapter 5 that not all the activities addressed are universally negative and, if properly conducted, some might have beneficial outcomes as well. The paragraph on page 121 (“Vegetation management projects are designed to:...”) is confusing in this regard as it seems to mix positive (protect avalanche chutes, ...minimize impacts of motorized access) with negative ones (enhance forage production which is value on what species the forage is enhanced for...cattle? or wildlife?).

Certainly, “recreation” has potential and actual positive benefits for bears in National Parks because of the high value of recreational activities involving bear viewing, for example, creates a constituency for bears. One reference to this is:

Miller, S.M., S.D, Miller, and D.W. McCollum. 1998. Attitudes toward and relative value of Alaskan Brown and black bears to resident voters, resident hunters, and nonresident hunters. *Ursus* 10:357-376.

I believe there is similar work for the GYE by Kerry Gunther and his colleagues and perhaps for Glacier NP as well. I would check the book on the grizzlies of Yellowstone by Gunther et al. published a few years ago.

5. Are there any significant oversights, omissions, or inconsistencies in the Biological Report?

The FWS should consider including in the section on “stressors” in the GYE problems associated or potentially associated with genetic isolation. I personally think, based on the many small isolated populations that have existed worldwide for a long time (e.g. Italy-Abruzzo, Pakistan-Deosai) and isolated (large) populations like on Kodiak/Afognak Islands that this stressor’s significance is potentially over emphasized by some. Regardless, however, larger and connected populations have higher probabilities of persistence over time than smaller and connected ones so discussion of this issue is warranted and necessary. In this section it will be important to include the information on connectivity routes to the GYE developed by Peck et al. (2017); this is a valuable paper.

It appears to me that the very important analysis of connectivity routes from the NCDE to the GYE in the paper by Peck et al. (2017) are incompletely discussed in this document. I did a search for

this reference and found it used primarily with reference to other ecosystems (North Cascades) and not with reference to the GYE to which it primarily pertains. It was referenced on pages 50 and 55 four times but not elsewhere.

It is discouraging to see *in litt* cited references with dates >1 year ago in this report and to not even include these citations in the literature cited. This undercuts the report's credibility. To give just 2 examples there are *in litt* citations to Landenberger 2014 and Rice 2019 on page 111. I suggest doing a search for all "in litt" mentions in the text and fixing this including adding the citations to the literature cited. I did this and even found a citation to "Manley 1993 *in litt*," on page 49. This problem suggests that at least some of the current report is recycled text without much additional thought from previous versions of the 5-year report. The reader has no idea what these *in litt* reports are or how much credibility to give the information apparently in them, i.e. they are not valid citations at all.

The section on Livestock Allotments in the GYE (page 110, and page 137) needs to acknowledge the role of the National Wildlife Federation (NWF) and its partners in retiring the livestock grazing allotments on NF lands around and in the GYE. Working with willing grazing permittees, NWF retired over a million acres of livestock grazing allotments in the GYE (see: <https://www.nwf.org/WCR/Where-We-Work>). This was a serious commitment by NWF that deserves to be specifically recognized and acknowledged here. The Forest Service was, of course, an important participant in this effort but the retirements couldn't have been accomplished without the leadership of NWF because this NGO and other NGO partners (Rocky Mt. Elk Foundation, Wild Sheep Foundation, Sierra Club, Western Watersheds, etc.) raised all the money to pay leaseholders to retire their allotments. The Forest Service was legally unable to do this kind of retirements on their own. NWF also did all the negotiating. It is unfortunate and unfair that government agencies fail to acknowledge, by name, valuable partnerships like this with NGOs and this failure may potentially discourages such valuable collaborations. The statement on page 137, "We expect the USFS will continue to implement these provisions that minimize grizzly bear conflicts with livestock" is not correct as without the collaboration with the NGOs the USFS could not retire grazing allotments. I'm not sure if the publication cited (USDA FS 200b, pp 16-17, the forest plan amendment) gives credit to the NWF and other NGO partners or not but if it doesn't, then this statement is not correct in this report and should be expanded regardless.

I think it would be worthwhile to mention as well exactly what the legal constraints are on the USFS that prevents them from actively seeking to retire livestock grazing allotments through making payments to leaseholders. This makes it necessary for NGOs to do this work and raise this money.

I also think the report is remiss to not make any mention of the significant work done by Defenders of Wildlife (DOW) on a number of areas of mitigation including livestock loss compensation (needs mention also on page 147 under the heading "livestock compensation programs") and fencing of attractants including poultry areas, apiaries, bear resistant dumpsters (along with NWF), and some schoolyards. Same for the Blackfoot Challenge in the NCDE. There are some appropriate mentions of the good work done by Vital Ground and The Nature Conservancy (e.g. p. 125) and the Transboundary Grizzly Bear Project (pages 126-127) so the absence of mention of the good work done by NWF, DOW and some other NGOs should be corrected. Without recognition by agencies of their work, collaborative NGOs implicitly cede the mantel of

conservation activism to those NGOS who make a lot of noise and gain publicity primarily through litigation against agency actions (vegetation management, access management, delisting, etc.).

On page 123 in the section on fragmentation in the NCDE there is mention regarding genetic diversity “as discussed below”. Should provide a section reference here to where “below” this is discussed. The report is inconsistent on this and anywhere there is a reference to a discussion elsewhere in the report, the section where this occur should be mentioned as is done, for example, in the following section on private land development where it says (“see *Habitat Fragmentation* above for further discussion”).

On page 94 the term “IRA” is mentioned without providing the term (“Inventoried Roadless Area) to which it refers. Additionally, and more importantly, there is no discussion of what exactly the implications of IRAs are for grizzly bears, how they are established, how they are protected, etc. There is a mention that these are relatively persistent because of the 2011 ruling but still the moderately informed reader would like to know more about what these are especially with regard to the NCDE where these are especially significant for grizzly bear conservation (page 96)

Same comment for the use of the acronym NRA (Wilderness Recreation Area) on page 97. Also, for the “DCA” (Designated Connectivity Area) on page 97; I needed to look this up in the list of acronyms. Need some definition of these similar to what is provided for WSAs on page 94.

Where the acronym DCA (Demographic Connectivity Area) is used on the top of page 102 (NCDE motorized access) I suggest you put in the term as well. Although it may have been, I don’t recall a preceding use of this term or any definition of it especially whether there are any constraints on use in DCAs. The first use I see of this term is on page 51 and there is no definition or description there or on page 52 where a number of DCAs are mentioned. Next mention is on p. 92.

I think the summary paragraph for habitat-related effects on the GYE (page 129) overstates the case made ([these stressors are] “sufficiently reduced so that they affect only a small proportion of the population”). Certainly, “recreation” influences more than a small proportion of the GYE population even though at current management it appears to not significantly negatively affect the whole population. Also, the last sentence in this paragraph refers to “this stressor”; do they mean “these stressors”? The optimism in this final summary paragraph should be qualified to clarify that it pertains only as long as current conditions apply (e.g. especially vegetation, livestock, and road management) and only to the core population (DMA?).

The apparent exact same language is used for the summary paragraph for the NCDE Habitat-related effects (page 130). The comments made above apply here to this paragraph too. It is strange to use the same wording for these 2 very different ecosystems. Additionally, for the NCDE, there is relatively little discussion specifically regarding the expanding distribution eastward onto the prairies. This is hidden under terms like DMA, etc. I think there are differences in eastward expansion of bears from the GYE as there is little suitable habitat in that direction and for the NCDE where there are suitable habitats to the east (biologically at least). Some explicit discussion of the social challenges facing grizzlies moving east out of the NCDE would be appropriate; will this be tolerated or not? How will decisions on this issue be reached (through the citizens’ advisory panel which isn’t even mentioned?).

Where available, it would be worthwhile to include mention of estimated habitat carrying capacity for the CYE, SE, BE, and North Cascades. I know some of this work has been done and it would

be worthwhile to mention it to see what the upside potential is for these areas and, in the cases of SE and CYE, how much of an increase in current population is possible. This is especially the case since population size has 3x the weight of other resiliency considerations and large increases may be constrained by habitat availability.

On page 135 there are 5 listed sources of human-caused mortality. In addition, “unknown and unreported kills” should be listed as these can be significant (McLellan et al. 2018 is one missing reference to this and another is Cherry et al. 2002; Cherry citation is included in the literature cited):

Bruce N. McLellan , Garth Mowat and Clayton T. Lamb. 2018. Estimating unrecorded human-caused mortalities of grizzly bears in the Flathead Valley, British Columbia, Canada. McLellan et al. (2018), PeerJ 6:e5781; DOI 10.7717/peerj.5781.

Cherry S, Haroldson MA, Robison-Cox J, Schwartz CC. 2002. Estimating total human-caused mortality from reported mortality using data from radio-instrumented grizzly bears. *Ursus* 13:175–184.

In the Section on human caused mortalities, mistaken identity killings in various ecosystems there should be a discussion for each ecosystem of how many of these occur at bait stations established for black bears, especially for BE and SE in Idaho. One justification for baiting in these areas is that it allows hunters to inspect their target before shooting it. If this isn't working, then it should be acknowledged and quantified especially in an area like the BE that is so difficult and infrequent for bears to reach. This is especially pertinent since the *de facto* method to repopulate the BE is through natural recolonization. This comment may also apply to the North Cascades if black bear baiting is allowed there.

Additionally, mandatory inspection of black bear kills is required in Idaho and probably some other states as well. This should be mentioned somewhere especially for the BE as there have been claims that grizzly bears are actually resident there. If this were true, then it would be expected that the inspection process would distinguish them in the take by hunters confusing them with black bears. The absence of such identifications is a very strong indicator that—in fact—grizzly bear population does not exist in the BE and this may be true as well for the North Cascades where black bear hunting is also allowed. At a minimum, the number of black bears killed by hunters in the BE and North Cascades should be mentioned in the context that required hide inspections (in Idaho at least) would have revealed if any of these were actually grizzly bears.

In the section of “food storage orders” on page 147 there is mention of the areas with such orders. However, there are no trend data presented showing whether these orders have reduced numbers of bears being killed. Certainly, some analysis of the effectiveness of food storage is appropriate; if clear data are not available or—even--obtainable, then this should be recognized.

In the section on “summary of food resources” (page 180) the lead sentence (“There are no indications...[that diets have changed]” fails to recognize the historical importance of salmon to the BE and North Cascades (now extirpated) populations. The sentence is specific to “the last several decades” but, regardless, needs to include mention of dramatic changes in salmon abundance as it is clear that populations with access to salmon are more dense than populations without such access (Miller et al. 1997, Hilderbrand et al. 1999).

Miller, S. D., G. C. White, R. A. Sellers, H. V. Reynolds, et al. (1997). Brown and black bear density estimation in Alaska using radio-telemetry and replicated mark-resight techniques. *Wildlife Monographs* 133.

Hilderbrand, G. V., C. C. Schwartz, C. T. Robbins, M. E. Jacoby, T. A. Hanley, S. M. Arthur, & C. Servheen. (1999). The importance of meat, particularly salmon, to body size, population productivity, and conservation of North American brown bears. *Canadian Journal of Zoology* 77(1): 132–138. [already in lit cited].

I don't recall mention in the various sections on mortality management of the reality that the kinds of mortality that occurs to un hunted populations like the listed ones in this report is more balanced in terms of sex than for hunted populations where mortalities tend to be biased toward males. This is a worthwhile point to make as it tends to make the mortalities more demographically significant (females, especially adults, are more expensive than males demographically). This point is worth making in the hunting section as well as in sections describing the demographic implications of mortalities (nuisance bear kills, management actions, accidents, etc.). Some of this is in Miller and Tutterow (1999 citation provided below) and also:

Miller, S. D. (1990). Population management of bears in North America. *Int. Conf. Bear Research and Management* 8:357–373.

Chapter 7 (future conditions) must recognize that in all ecosystems grizzly bears will continue to be “conservation reliant” as recognized and defined by Scott et al. (2005). This is a vitally important reference to include and it is currently missing. The need for this recognition of ongoing conservation reliance for grizzly bears is a good reason for the inclusion of the cost estimates for grizzly bear recover discussed in query #1 (missing topics) of this SSA report.

Scott, J.M., D.D. Goble, J.A. Wens, D.S. Wilcove, M. Bean, and T. Male. 2005. Recovery of imperiled species under the Endangered Species Act: The need for a new approach. *Frontiers in Ecology and the Environment* 3:383-389.

Page 151 (Mortality limits for NCDE). Somewhere in this section it should be specified how many bears need to be radio-collared and the distribution of these collars to derive the demographic parameters needed to estimate there are at least 800 bears in the ecosystem. Additionally, it should be clearly acknowledged that the foundations of current estimates are built on projections from the original population estimate completed in 2004 that was based on DNA hair snaring techniques reported by Kendall et al. (2009) for the entire NCDE ecosystem. Estimating lambda using data from radio-collared bears assumes the collars are distributed in proportion to local densities. It should be acknowledged that distributing radio collars based on the relative density of bears in 2004 has become increasingly incorrect over time because local density has changed significantly as the populations has grown (Kendall et al. 2019). If population abundance continues to be estimated using demographic data from radioed bears, it will be necessary to periodically establish current values of local density and redistribute collars to reflect the change in bear distribution. Ways of periodically reestablishing a validity check need to be a source of ongoing research and that will probably focus on using DNA hair-snaring techniques to estimate trends (e.g. Kendall et al. 2019—citation below missing from report).

Kendall, K. C., T. A. Graves, J. A. Royle, A. C. Macleod, et al. 2019. Using bear rub data and spatial capture-recapture models to estimate trend in a brown bear population. Scientific Reports published 14 Nov. 2019. <https://rdcu.be/b54NY>

Page 150 (mortality limits GYE). In this section, I think it worthwhile to mention that the Chao2 process to come up with the population estimate remains broadly conservative even if some of the conservative biases were reduced in the IGBST 2012 report. Finally, somewhere it should be mentioned that the foundation of the GYE population estimate remains the “unduplicated counts of females with cubs” that has been used for decades in the GYE although periodically improved. This is because this foundation is unique among “ecosystems” (recovery areas). Should put the reference to Cherry et al. 2002 after “statistical estimate” and not at end of sentence. Otherwise, it implies that the Cherry manuscript addressed all of the listed mortality causes in this sentence when, in fact, most of these were in IGBST 2012).

6. Are the statements about current and future condition logical and supported by the evidence provided?

In general, yes. However, I think it is worthwhile to explicitly acknowledge the high level of uncertainty association with predictions of future conditions and to include, where available, results of PVA analyses such as those of Boyce for the CY and BE (one reference to IBA monograph below).

On page 227 just above Table 23, it says that Table 23 reflects risk from “stochastic events”. I think this is an incorrect use of the term “stochastic” and, in fact, this term should be eliminated from the final sentence referencing Table 23 and perhaps replace it with “to population persistence” or some other similar term. Similarly, on page 228 the introduction textual sentence for Table 24 and Figure 3 refers to redundancy improvements from “catastrophic events” however, the caption for Figure 24 refers (correctly) to viability and not impacts of “catastrophes”.

There may be other disconnects, like this, between statements in the text introducing tables and figures and I suggest reviewing the report throughout to assure that all tables and figures are correctly referenced in the text with regard to what points these make.

7. Does the Biological Report include all the necessary and pertinent literature to support our assumptions/arguments/conclusions?

In the Section on “Energy and Mineral Development” (page 113) it would be appropriate to include the issue of value of “reclaimed” mining lands as grizzly habitat. There is a report that such areas may be preferentially used by grizzly bears in Alberta (Cristescu et al. 2016) although the opposite may be the case in other circumstances:

Cristescu, Bogdan et al. (2016). Large Omnivore Movements in Response to Surface Mining and Mine Reclamation, Scientific Reports (2016). [DOI: 10.1038/srep19177](https://doi.org/10.1038/srep19177).

On page 117 (Recreation introduction) there is mention of increased human-caused mortality from hunters during hunting seasons and a reference to the Human-caused mortality section (page 132). This section on page 132 deals largely with defensive kills but the problem is larger than that in

areas like the GYE and NCDE especially where bears are attracted to game meat and gutpiles. This is inadequately discussed and referenced especially to the work south of YNP by the GYE study team in a report, I think, by Haroldson. It was also reported on, in Alaska, in an analysis that should be cited of Defense of Life or property kills and also in the human-caused mortality section (page 132, and 136-138) by:

Miller, S.D and V.L. Tutterrow (1999). Characteristics of nonsport mortalities to brown and black bears and human injuries from bears in Alaska. *Ursus* 4:219-252.

In the introduction for the section on stressors, this report should cite a new study about the challenges and behavioral responses (become more nocturnal) of bears in human-dominated landscapes:

Lamb, Clayton T, Adam T. Ford, Bruce N. McLellan , Michael F. Proctor, Garth Mowat, Lana Ciarniello , Scott E. Nielsena, and Stan Boutina. 2020. *PLOS*: 117(30):17876-17883. <https://www.pnas.org/content/117/30/17876>

There was an earlier similar report on activity budgets of brown and black bears in the GYE (Grand Tetons NP) as well:

Contrasting activity patterns of sympatric and allopatric black and grizzly bears. *Journal of Wildlife Management* 74(8): By: C.C. Schwartz, S.L. Cain, S. Podruzny, S. Cherry, and L. Frattaroli. <https://doi.org/10.2193/2009-571>, <https://pubs.er.usgs.gov/publication/70037607>. 2011??

The modeling paper on the importance of specific management actions (Proctor et al. 2004) addresses specifically to the CY and SK ecosystems but is also generally pertinent:

Proctor, M.F., C. Servheen, S.D. Miller, W.F. Kaswork, and W. L. Wakkinen. 2004. A comparative analysis of management options for grizzly bear conservation in the U.S._Canada trans-border area. *Ursus* 15(2):145-160.

Similarly, Mace (2004) is a good review of approaches to access management:

Mace, R.D. 2004. Integrating science and road access management: Lessons from the Northern Continental Divide Ecosystem. *Ursus* 15(1);126-136.

The section on Natural predation and mortality (p 153-154) as well as the section on legal hunting (p. 152) should make brief mention of the controversy over whether sexually selected infanticide (SSI) is related to sex ratio in the adult population as asserted by Scandinavian researchers but refuted by North American researchers (e.g. Schwartz 2003b). This is pertinent because the Scandinavian conclusions are frequently cited to make a case against hunting and this is unsupported by studies in NA. Additional pertinent references to this point for NA include:

McLellan, B. N. (2005). Sexually selected infanticide in grizzly bears: the effects of hunting on cub survival. *Ursus* 16:141-156.

Miller, S. D., R. A. Sellers, & J. A. Keay. 2003. Effects of hunting on brown bear cub survival and litter size in Alaska. *Ursus* 14:130-152.

And references for Scandinavia are:

Swenson, J.E., F. Sandegren, A. Soderberg, A. Bjarvall, et al. (1997). Infanticide caused by hunting of male bears. *Nature* 386:450-451.

Swenson, J. E. (2003). Implications of sexually selected infanticide for hunting of large carnivores. Pages 171-190 in M. Festa-Bianchet & M. Apollonio, editors. *Animal Behaviour and Wildlife Conservation*. Island Press, Covelo, California, USA.

With reference to the same issue, the conclusion of the section on Natural predation and mortality that intraspecific predation is “an insignificant factor in population dynamics” is probably not correct. In all likelihood low cub and subadult survival caused by intra-specific killing in populations at or near carrying capacity is probably the mechanism that mediates population stability in places like the GYE (above McLellan and Miller references, Schwartz 2003b and Schwartz et al. 2006b) plus a new reference given below. The paper below should also be discussed and referenced in the section on Nutritional Ecology (feeding) (page 51 last paragraph) where it discusses importance of body fat to natality.

Keay, J.A., C.T. Robbins, and S.D. Farley. 2018. Characteristics of a naturally regulated grizzly bear populations. *J. Wildlife Management*.

The section on Climate change impact on denning (page 182) should also include in the first sentence references to the following. In addition, these and other work by Karine Pigeon should be referenced on the section on hibernation and denning in the introduction (page 49) although this work was done in Alberta (none of this work is cited)

Miller, S.D. 1990. Denning Ecology of Brown Bears in Southcentral Alaska and Comparisons with a Sympatric Black Bear Population. Vol. 8, A Selection of Papers from the Eighth International Conference on Bear Research and Management, Victoria, British Columbia, Canada, February 1989 (1990), pp. 279-287

Pigeon, KE, G. Stenhouse, and SD Cote. 2016. Drivers of hibernation: linking food and weather to denning behaviour of grizzly bears. *Behavioral Ecology and Sociobiology* 70(10): 1745-1754.

Pigeon, Karine E., STEEVE D. COTE, and GORDON B. STENHOUSE. 2016. Assessing den selection and den characteristics of grizzly bears. *J. Wildlife Management*. 80(5):884-893.

Pigeon, K.E., S.E. Nielsen, G.B. Stenhuse, Steve D. Cote. 2014. Den selection by grizzly bears on a managed landscape. *J. Mammalogy* 95(3):559-571.

In this section too with respect to the GYE, consider adding some discussion of the potential for later den entry caused by climate change to increase mortality because of more overlap with hunting seasons for elk on the (especially southern) periphery of the park. This paragraph references the “human-caused mortality” section for the GYE but, in fact, there is no discussion of this pre-denning source of mortality in that section and there probably should be. I believe the ST has investigated this.

This report makes no or inadequate use of Boyce et al. 2001 (Population viability for grizzly bears: a critical review, IBA monograph Series Number 4). Although this is referenced in the literature cited, I did a search and found no reference to it in the text. Figure 11 (page 9) in this monograph

gives projected (modeled) times to extinction of grizzly bear populations of various population sizes and this certainly merits referencing in, especially, the ecosystems with small populations.

There is quite a bit of discussion of delisting problems in Miller et al. (2013) that merit referencing in sections like “regulatory history” (e.g. page 70, Chapter 3) including:

Miller, S.D., B.N. McLellan, and A.E. Derocher. 2013. Conservation and management of large carnivores in North America. *International Journal of Environmental Studies Monograph* (S. Mahoney, ed., 7(3): 383-398.).

Miller, S.D. 2007. Distinct population segments and grizzly bear delisting in Yellowstone: A response to Rosen. *Ursus* 18:118-124.

Other papers in this monograph may also be worth referencing such as “Enshrining hunting as a foundation for Conservation—the North American Model” by Mahoney and Jackson. Other papers in this monograph potentially worth citing are “The wilderness hunter: 400 year of evolution” by C. Semcter and J. Pozewitz and “Going public: scientific advocacy and North American wildlife conservation” by J.A. Schaefer and P. Beier, and “The role of hunting in North American wildlife Conservation” by J.R. Heffelfinger, V. Geist, and W. Wishart. If the FWS wants these or others in this monograph, I can provide them as this isn’t an open access journal.

The section on regulatory history and recovery planning (page 72 for the GYE) ends with “The Service has appealed this ruling”. Obviously, this appeal has had a decision and this should be updated.

With regard to factors that influence habitat selection for grizzly bears the following paper should be discussed and referenced including in the section on climate change:

Pigeon, K.E., E. Cardinal, G.B. Stenhouse, and S.D. Cote. 2016. Staying cool in a changing landscape: the influence of maximum daily ambient temperature on grizzly bear habitat selection. *Oecologia* 181(4):1101-1116.

I was able to find only one reference to Proctor et al. (2018) and that was in reference to gene flow between Canada and the CY (page 49). The main point of this very important paper involved the importance and success of human interventions (sanitation and other management actions) on grizzly bear population viability of which gene flow is only one index. This paper should be read again and more completely integrated into this SSA in many different sections dealing with conservation/management actions. This includes the discussion on resilience, stressors, conservation efforts, and future conditions. This paper is published in a somewhat obscure journal but is a very significant contribution to what must be done and has worked to conserve and recover grizzly bears.

(Available at: <https://digitalcommons.usu.edu/hwi/vol12/iss3/6>)

8. Are there demonstrable errors of fact or interpretation? Please provide the specifics regarding those particular concerns.

I believe that the discussion of the 4 “density independent effects” in the GYE (second to last paragraph, page 63) actually lists effects that are mostly or entirely density dependent with the possible exception of variations in annual home range sizes in response to stochastic changes in

food availability. I am not sure what I'd include in a list of density-independent effects for grizzly bears or what evidence I'd cite for them. I suggest some hard re-consideration of this list and better citation to sources if these are actually density independent.

The reference to Smith et al. (2018) (page 189, Cumulative effects), is incorrectly cited as being from JWM when it is actually from Journal of Fish and Wildlife Management. I requested the manuscript and the FWS provided it and agree that this is a valuable paper that is correctly cited earlier (page 2, 28, 31, 34, etc.) as describing the template for the entire SSA analysis.

However, after reading this paper I believe it is incorrectly cited as being the framework for cumulative effects analysis (actual draft SSA page 183). I asked for the paper to be sure I understand that the SSA correctly cited it and to be sure I understood what cumulative effects analysis was. There was nothing in the Smith et al. paper that I could find on cumulative effects analysis or its need in an SSA. This section instead discusses what might be termed 'multiple' effects analysis. Instead, this section on cumulative effects seems to imply that the stressors discussed are additive and the fact that multiple stressors are identified amounts to a cumulative effects analysis. It is my impression, in contrast, that cumulative effects analyses imply that the stressors may act in a synergistic way such that the effects of them together is greater than the sum of them individually (e.g. impacts of stressors "a" and "b" that each have effects "x" are $>2x$). Conceptually, this is almost certainly true. Regardless, the SSA must discuss and reference real background and appropriate citations on what cumulative effects analyses is and how it pertains to this SSA for grizzly bears; Smith et al. (2018) doesn't provide a template for this and is incorrectly cited to this effect.

In the last paragraph in this section (page 63) includes a sentence that I don't understand starting with "importantly, annual survival of independent females...remained the same while..." I suspect this refers to annual fluctuations in WB Pine nut production but it isn't clear. Overall, the last part of this paragraph is very unclear about whether density dependent effects are being discussed or outcomes of fluctuations in WB pine crops.

9. Additional general comments.

N/A

GENERAL COMMENTS

EASILY CORRECTED SUGGESTED EDITS:

Throughout "data" is treated as a singular noun and it is plural. This is an increasingly common mistake and may become standard. It isn't essential to correct. I'd personally prefer to do a search and replace for all "data is" with "data are".

References to "unpublished data" (e.g. from MFWP on page 124) should be avoided whenever possible. If essential to include, some clues should be provided on how to obtain it such as someone's name who collected it. Suggest doing a search for "unpublished" and correct whenever possible.

On page 129 (Fragmentation in the NCDE) there is an unclear reference to "the range with the highest density of females with cubs (Costello et al. 2016)" within the NCDE recovery zone. I

assume this is Glacier NP and that the high density refers only to the NCDE but some clarification would be helpful.

Page 131 (Mortality-related Effects in GYC, SE, EE and North Cascades), 3rd paragraph), apparent typo (missing word): “hunters; although [when?] unintentional they are considered....”

In the section on Mortality limits for NCDe (page 151) the acronym “TRU” is used and should be defined/identified here. Listed in the list of acronyms as “total reported and unreported mortalities”.

Typo on page165 (Augmentation in the CYE): “202020a”

In Chapter 6 just before Figure 23 the explanation for the scoring system includes the term “to withstand catastrophes...” I believe that is not what this score represents and this should be rephrased as a metric to “withstand negative/adverse stressors”. Catastrophes are in this report are defined narrowly, unlike “stressors”.

In the section on the Cabinet-Yaak ecosystem just below Figure 15 the figure 0.9 percent annual growth rate is mentioned. Would be good to give a range for this value. Same is true for the population estimate for the NCDE just above (1,068) and later for the SE of a grown rate of 2.5%. For all point estimates like these, it would be good to give the values that represent the limits of the CI.