Comments on Draft (May 1, 2017) Biological Report for the Mexican Wolf

I appreciated the direct clear writing throughout. The coverage of demographics, genetics, habitat, and human-caused mortality was thorough and will provide a good foundation for the recovery plan. I note a few areas where additional information or discussion would be helpful.

Geographic range of *C. l. baileyi* (400-455): I had not previously read scientific literature on the geographic range of *C. l. baileyi*. The range used by USFWS extends considerably north and east of the range described by most historical authorities (as depicted in Fig. 5). I believe (based on the locations of the National Forests mentioned in the Report) that the existing US population is entirely within this northward expansion area. Please indicate on Figure 5 where the current US population occurs, and discuss this geographic relationship in the text. I was troubled enough by this issue (existing population north of traditional maps) that I read several of the referenced papers on geographic distribution. I was surprised that Heffelfinger et al. (2017) – after arguing strongly for basing the range on historical distributions of small-bodied wolves and pine-oak woodlands, and arguing strongly against using dispersal abilities to expand the range – concluded that "Given…. the recent success shown by rapid growth of the wild population of Mexican wolves, the most scientifically sound approach is to base recovery efforts within the range accepted by USFWS in 1996." The idea of using success of the reintroduced population so near the historic range as a reason to expand the range map seems reasonable, but should be explicitly discussed in the Report. In particular, if ability to support a population is a criterion, please discuss the north rim of Grand Canyon, the Sangre de Christos, and other nearby areas as potential population sites.

In the context of recovery of the entire species (*Canis lupus*) in North America, the FWS should discuss whether recovery actions for *C. l. nubilus* is likely to enable it to reoccupy the north rim of Grand Canyon, the Sangre de Christos, the San Juans, and other nearby areas. If not, please discuss which wolf subspecies would be most appropriate to recolonize these areas. If *baileyi* were to recolonize these areas, does FWS intend to remove all such colonists? Are these areas intended to remain wolf-free for the indefinite future? In the context of recovering *baileyi*, it is reasonable to prioritize historic *baileyi* range (i.e., the Sierra Madre Occidental) over these more northerly areas. But it seems inappropriate for the recovery plans for each subspecies to put on "blinders" so strong that large portions of former wolf range are ignored.

The Report explains that the Sierra Madre of Mexico lack elk and have smaller areas with low human disturbance; on the other hand the Sierra Madre is a vast area that comprises the overwhelming majority of historic range. Please add a table outlining how many Mexican wolves can be supported in the MWEPA, in the northern Sierra Madre, and the southern Sierra Madre, and in West Texas. It will be appropriate to have wide confidence intervals and perhaps avoid reporting a mean or median estimate. This will set the stage for criteria and target in the Recovery Plan.

744-746: The idea that there is no compensatory response in deer survival or recruitment seems counterintuitive. I tried to check the two citations for this idea. "Bower" (should be "Bowyer") et al. (2014) is a largely theoretical paper. Although the paper is not specific to southwestern deer

populations, I agree with the basic idea that K varies a lot in environments like the southwest. However, their graphs of population performance versus population density relative to K seem to assume a convex shape, with performance stable at low to moderately large population sizes, and then decreasing rapidly near K. In a major review of >1700 population time series, Sibly et al. (2005. Science 309:607; with comments & a response in 2006) suggested the relationship is generally concave, with population performance dropping rapidly from low to moderately low density, and then relatively flat in the neighborhood of K. Thus density-dependent (compensatory) responses might occur only at low N. I was unable to access the other citation (a book chapter by DeYoung et al. 2009). I don't think this idea (lack of compensatory response) will have a big effect on recovery criteria or recommended management actions, so I don't want to make a big deal out of it. But by the same token, if this idea does not drive recovery criteria or recommended actions, you might want to abandon it, or modify the discussion of this idea.

874: "Our data suggest that probability of an adult pair producing pups in the wild is a function of age of the dam and relationship of the paired female to her mate (i.e., the predicted inbreeding coefficient of the pups)." This seems intuitively correct. In the interest of transparency, please reference this to a report, or summarize the supporting data in 1-2 sentences, or at least provide some idea of what "our data" are (e.g., data on reproduction of 47 adult pairs, where each pair was observed for an average of 2.2 potential breeding seasons).

Wow. Diversionary feeding has been hugely helpful, and probably is the key reason for success in the last 7 years. Congratulations. Please discuss the costs of the diversionary feeding effort, prospects for long-term sustainability of the practice (I see no reason to think we'll run out of road kill or carnivore logs, but it'd be nice to see this affirmed), and any potential negative consequences: Do the wolves know they are being given handouts? Could this make them more likely to interact with humans?. If there are any potential downsides, can they be minimized? Discuss alternative strategies to avoid killing and management removals. For example, is it possible to encourage transfer of allotments to wolf-friendly livestock operators, or to retire grazing allotments? Is it possible to beef up law enforcement in a way that does not backfire? (I've heard rumors that some persons responsible for illegally killing wolves are known with reasonable certainty, but that prosecutions have not been attempted. I can accept that you may not want to pursue aggressive law enforcement. But some discussion would be helpful.).

987 & 1014: "Material in the genome bank… [has] been used successfully in a limited number of instances (Siminski & Spevak 2016)." The reference (S&S 2016) is an unpublished report that I could not readily find on Google, but the title of the citation refers to Mexican wolf. This is an intriguing topic. Please provide more information on the genome bank: How many additional animals beyond the 7 founders are represented? Does it consist of frozen sperm or eggs, other tissues, extracted DNA, or something else? Describe the successful instances – was genetic material inserted into a Mexican wolf embryo?

1111-1112. Your conclusion that "we should manage against" any introgression of non-*baileyi* genes seems a bit strong. The Report acknowledges that genetic exchange occurred historically and eventually will occur again. (Indeed, potential introgression would be a good problem to have, because it would mean that both *baileyi* and northern wolves have expanded.) I agree it'd

be nice for hybridization to wait until the *baileyi* population is large (to minimize swamping) but this language suggests you would remove a single northern wolf that entered the recovery area. Maybe that is exactly what you mean. If so, say so. If not, I suggest softening the language slightly.

Resilience as measured by PVA (1349-1379). Vortex uses a simple ceiling model of density dependence, instead of a more realistic form of density-dependence in which reproduction or juvenile survival could increase dramatically at low population sizes (as suggested by Sibly et al. 2005). As a result, Vortex tends to predict close to 100% extinction risk within a few decades for all populations fewer than 100 or 200 animals. Given many observations of small populations persisting for decades, Vortex probably produces overly high estimates of minimum numbers needed. I emphatically do NOT suggest a new PVA. But it'd be appropriate to mention that these estimates are likely higher than the true minimum viable populations (and the true MVP would be a stupid goal, because we don't really want to manage for the fewest possible animals).

1456-1469 mentions the need for gene flow among the 2 or 3 populations (MWEPA, northern Sierra Madre Occidental, southern Sierra Madre Occidental). I agree that artificial connectivity will be necessary at first (without artificial methods, there will not be 3 populations to connect). It seems appropriate to mention that some potential realizations of the Trump Wall could preclude natural connectivity between MWEPA and Sierra Madre Occidental (with further discussion in the Draft Recovery Plan).

More importantly, it seems the representation goal will require 2 populations in the Sierra Madre Occidental, but the text seems vague about this.

I think the authors should be listed. I value reverse-blind reviews (authors anonymous, reviewers are not) as it reduces the "expert halo effect" and forces reviewers to be polite and constructive. But it would be helpful to know that the authors cover a diverse set of skills. Given the obvious need for recovery within Mexico, it'd also be nice to know that Mexicans co-authored the Report (and the RP when it comes out).

Trivia: line 582: southeastern or southwestern? 1282 gentic = genetic. 1308 intial = initial.

[redacted], May 19, 2017

The Draft Biological Report for the Mexican Wolf (5/1/2017)

Comments by [redacted] submitted on 5/26/2017

The Biological Report brings up a number of concerns about the successful recovery of Mexican wolves. Below are some general comments on the Biological Report and then some specific ones. Separately are comments about the PVA report and the Habitat report that were Appendices A and B to the Biological Report.

General Comments

(1) The advocacy of only two unconnected populations, one in AZ – NM and one in Mexico, is not the best science and is not likely to support recovery. Both the northern Rockies and Great Lakes wolf recovery programs advocated three interconnected populations. Earlier Mexican wolf recovery teams also advocated three interconnected populations, the present AZ-NM one and two additional ones on the north rim of the Grand Canyon and in northern NM – southern CO. Exchange between these populations to form a metapopulation could result in both demographic rescue and genetic rescue so that the overall viability of the introduced animals would be increased by the presence of the other populations.

(2) The emphasis on historical range of the Mexican wolf is not the best science. Modern genetic data have supported that the range of Mexican wolf genetic ancestry was much more widespread than the outdated morphological data. The recent article by Heffelfinger et al. (2017) should not be used for justification of a small range because there are many major problems with this analysis (see below).

(3) The reintroduced population is north of the historical range and is doing well in spite of high human-caused mortality and removals. In other words, the realized contemporary range and habitat of the successful reintroduced population is much more significant than any historic range data accumulated when the Mexican wolf was being hunted to extirpation. Further, the realized range indicates what other habitat would be suitable for range expansion to the north if the wolves were allowed to move, or be reintroduced, there.

(4) Before extirpation of wolves from the western US, they occurred throughout the western US and formed clines of genetic ancestry and morphology over space. In other words, restoration of this pattern should be the goal, not the isolation of Mexican wolves in the southwest and northern gray wolves in the northern Rockies.

(5) Because of the great increase in diversionary feeding since 2009, it is not possible to measure appropriately inbreeding depression for litter size as would be experienced by a population that is not being fed. Further, other factors that influence fitness, such as viability, mating success, and probability of reproduction, might also be impacted by inbreeding. In other words, it is highly likely in a population with only two founder genome equivalents remaining that there would be inbreeding depression and ignoring inbreeding depression might further imperil the population.

(6) The present administration plans to build a high, insurmountable-to-wolves wall along the US – Mexico border. This wall will prevent any connection between wolves from the US and

Mexico. In other words, instead of a metapopulation, the two proposed populations, one in the present MWEPA and one in Chihuahua, will not interact and they would not have any demographic and evolutionary connections. As a result, the presence of the Mexican population likely will not increase the viability of the US population or the overall viability of the introduced animals.

(7) The reintroduced wolf population is inhabiting ponderosa pine – elk country outside of the historical range but this habitat is only a small portion of the MWEPA. In other words, it is not clear that much of the MWEPA in the present day is adequate habitat for Mexican wolves. The presumption of Heffelfinger et al. (2017) that introduction of Mexican wolves to the areas in far southeastern AZ and far southwestern NM would provide suitable habitat for a Mexican wolf population seems unfounded.

(8) It is unlikely that the Mexican population(s) will contribute to recovery of Mexican wolves because of the lack of wild prey, lack of protection, presence of livestock, presence of humans, and presence of roads. Further, because the Mexican population will not have an effective connection to the US population, it will not contribute to overall recovery and could actually act as a diversion from efforts to recover the Mexican wolf in the US.

Specific Comments

line 233. Tribal areas are not in the historical distribution suggested by Heffelfinger et al. (2017), suggesting that the approach used by Heffelfinger et al. (2017) is not supportable.

l. 250. It would be useful to state somewhere what plans and goals the Service has to increase genetic variation from the captive population to the reintroduced population. The reintroduced population has approximately only two founder genome equivalents. How much does the Service think it can increase this low level and how long would this take?

l. 261. It would be useful to have more details on the Mexican population, such as, what is the average inbreeding coefficient and kinship of the individuals. Are they taking natural prey and what kind? Has this population undergone another bottleneck in the process of its establishment?

l. 282. These were not really family groups. Maybe it could said as “The founding wolves represent three different lineages, referred to as the McBride….Ranch lineages with 3, 2, and 2 founders, respectively (Siminski and Spevak 2016).”

l. 285. Instead of “descendants of these seven founders” this should be “descendants of two or more of these seven founders”.

l. 296. Are the parentages of these wolves being determined with molecular data? If so, then a pedigree of the wild population should be possible as in other wolf populations.

l. 311. Why is there no critical habitat for the Mexican wolf as for other endangered species? There should be critical habitat for Mexican wolves like there is for other endangered species.

l. 337. The “Aravaipa wolf” was killed in 1975 or 1976 in Arizona according to Brown (2002) in the second edition of his book, a photo is given on the last page of the book. It is not known whether this was the last individual of a US population.

l. 350. Note that the Bednarz estimate was for all of NM not just NM south of I-40, suggesting that he was not concerned with arbitrarily estimating gray wolf capacity for only part of the state.

l. 351. The numbers in 1915 were already reduced from earlier numbers by wolf killing. Will there be an effort to have a number of wolves approaching this number in NM or approaching the presumably larger number of individuals in NM ancestral to this killing?

l. 370. The very high number of subspecies in wolves, pumas, and other vertebrates designated in the early part of the 20th century, and based on morphology, were developed before modern evolutionary theory. Further, recent genomic examination of these and other species indicate that there generally is not strict geographic borders for genetic ancestry as proposed by subspecies designation. Presently the number of wolf subspecies is thought to be much less than it was and the designation of geographic borders of subspecies mainly an artifact. We know that wolves existed throughout North America and throughout the west.

More locally, wolves existed in northern NM, northern AZ, southern UT, and southern CO. The most appropriate extant subspecies for these areas is the Mexican wolf because of its proximity to these areas (other putative wolf subspecies have been extirpated from any nearby areas). In addition, the reintroduced Mexican wolf population now exists in a habitat similar (ponderosa pine forest) to that in these areas and has prey similar (elk and/or deer) to these areas. Further, molecular genetic data has demonstrated that genetic ancestry from Mexican wolves extended northernly (and westernly), suggesting that Mexican wolves are the most appropriate subspecies for these areas.

l. 380. The conclusions by Cronin et al. (2014) were unfounded and refuted, see the detailed response by Fredrickson et al. (2014).

Fredrickson, R.J., P. W. Hedrick, R. K. Wayne, B. M. vonHoldt, and M. K. Phillips. 2015. Mexican wolves are a valid subspecies and an appropriate conservation target. *J. Hered*. 106:415-416.

l. 392. What is the size (weight, etc.) of non-inbred wolves in the reintroduced population? These data are available and could be used as a comparison. It would be worthwhile to look at Mexican wolf weight (as an indicator of size) in the current population in different environments.

l. 404. The article by Heffelfinger et al. (2017) is based on morphology (mainly size) and represents outdated science. These morphological differences are strongly influenced by the environment (prey base, density, etc.) and were based on wolves killed when the population numbers were already greatly reduced. The molecular data from recent studies are much better indicators of differences between groups and are considered the best science currently available. The dismissal of modern molecular data and focus on outdated morphological data by Heffelfinger et al. (2017) suggests both a lack of objectivity and scientific sophistication.

Although it would be good to increase the sample size, recent genomic studies where the number of polymorphisms is very large somewhat compensates for this. In fact, a complete genomic sequence of a single individual can give much more information about ancestry than morphological measurements from many individuals. The realized distribution of the present day population which indicates similar habitats that it could colonize, in combination with current molecular data, are much better indicators of the potential Mexican wolf distribution than the outdated morphological data used by Heffelfinger et al. (2017).

l. 421. Wolves were present from Mexico to Canada and there was a gradation of both morphological and genetic variation throughout this area. A goal should be to have wolves throughout this north-south area and movement between populations as there was before they were killed out. The southern population is best represented by Mexican wolves, the northern population by northern gray wolves and the area in between by a mixture of these two groups, not dissimilar to that which occurred before they were killed out.

Figure 5, p. 18. The historical range represented here is of no scientific value. For example, it does not even include the area of the reintroduced population or other areas where Mexican wolves could maintain a population, such as the northern rim of the Grand Canyon and northern NM – southern CO. There is no way a viable population could exist in this historical range in the US as portrayed here.

l. 442. Heffelfinger et al. (2017) are discounting the best available science when they discredit the recent articles by Leonard et al. (2005) and Hendricks et al. (2015, 2016). In particular, the specimen examined by Hendricks et al. (2016) in San Bernadino County had a genetic variant at 4 diagnostic autosomal loci for which Mexican wolves are fixed and had the mtDNA haplotype found in other Mexican wolves. Whether this wolf was part of the resident CA population or a migrant from AZ, these data clearly show that Mexican wolf genetic ancestry has extended far beyond the small area near the border that Heffelfinger et al. (2017) suggest.

For consistency, why doesn't Heffelfinger et al. advocate removing Mexican wolves from the MWEPA except for the small area along the Mexican border in AZ and NM? In their advocacy for a very small historic US geographic range, they don't even acknowledge that Mexican wolves are doing well in the pine forests of the MWEPA preying on elk.

l. 485. Service (spelling)

l. 488. Is the yearling survival actually higher in Mexican wolves than in gray wolves, 0.67 versus 0.55? Is this because of artificial feeding in Mexican wolves?

l. 508. How has artificial feeding influenced territory size in the wolves in Mexico?

Table 1, p. 23. Second column, last row, should be 19,085,000.

l. 617. Might note that only 17% of habitat that is considered suitable (68,938/397,027).

l. 657. It states here that wolves are "highly-adaptable prey generalists that can efficiently capture a range of ungulate prey species of widely varying size". Consistent with this, the reintroduced Mexican wolves have mainly preyed on elk, ungulates much larger than their

putative prey prior to extirpation. Why not allow Mexican wolves to colonize habitats further to the north where elk would be a major prey?

l. 696. Does it make sense to feed domestic pigs as supplemental food if the wild wolves are going to prey on domestic pigs?

l. 709. It is very surprising that Mexican wolves are being fed nearly 20 years after they were initially released. Is there a goal to stop feeding them?

l. 765. deer, not dear

l. 783, and following. It is unclear why there is this extensive discussion minimizing the impact of wolves on prey in Yellowstone. Why isn't there a discussion of the impact of Mexican wolves, which were introduced about the same time, on the prey and vegetation in the area of reintroduction? Is it because there are no comparable data or investigation? Is it thought that Mexican wolves would have little impact on prey and vegetation, as is the claim from the interpretation of the "rigorous" study by Kauffman et al. (2010) for Yellowstone wolves?

l. 874. Why not instead " … to her mate, known as the kinship coefficient."

l. 877. Diversionary feeding, which started in 2009, changed the environment and substantially increased pup survival. As a result, comparing litter sizes with the same inbreeding coefficient (offspring, dam, or sire) before and after the diversionary feeding is not appropriate because they have experienced very different environments.

l. 891. The surprisingly very high rate of human-caused mortality, 93 of the documented Mexican wolf mortalities (70% of total), along with the high removal rate, appear to explain much of the early slow population growth. Although it is good to have this documentation, it is not clear that the causes of this high rate of human-caused mortality have been addressed. Is the Service doing something new to address this very high rate of human-caused mortality?

l. 924. How long will the diversionary feeding continue? It seems surprising that artificial feeding of 70% of denning females would be taking place nearly 20 years after the initial reintroduction.

l. 959. This level of mortality in Mexico is not sustainable. Can this be reduced greatly? If not, the likelihood of a viable population in Mexico is low.

l. 976. Hedrick, not Hedricks

l. 984. Is term "damaging loss" a term used in Soule? It is not a term usually used. How about "lowered fitness" instead?

l. 986. Are these options being carried out or under consideration? Is there a plan to increase the representation of under-represented founders? Is there a plan to use the genome bank? What is the predicted impact of these efforts over time?

l. 999. What is the genetic status and the genetic management objectives in the Mexican population?

l. 1007. pairings, not parings

l. 1008. How much impact would lengthening the generation potentially have? It would run the risk of older animals not reproducing at all.

l. 1009. It would appear to be very difficult to have any genetic impact from cryopreserved sperm and eggs in an established population of over 100. How would this be accomplished? Has this been examined as to how long and how many successful events would be necessary?

l. 1043. It would be useful to define more carefully what retained gene diversity means. Does it assume that in some year there was 100% gene diversity? And that since that point, given the known pedigree, that 75.91% of the heterozygosity in the initial year is expected to still be present over the living wolves? Of course, much better would be to have genomic data for all the wolves so that the realized level of gene diversity (heterozygosity) could be known for ancestral and living individuals and the population. This is particularly important because the actual level of heterozygosity in Mexican wolves is very low. Such genomic information could be used to potentially identify genetic variants that are responsible for inbreeding depression or for adaptation.

l. 1044. Are there efforts to change the ancestries from the three lineages? What are the goals? Why?

l. 1047. What does “related as siblings” mean? Does it mean that the mean kinship of individuals is 0.5? Or that inbreeding of an average offspring is 0.25? This very high level of relatedness sounds like a real problem. What is the potential to improve the situation? It seems that introducing a few cross-fostered individuals would only make a small difference. This statement indicates that there is a very small effective population size and it appears that there are only two effective founders remaining. Is this from the estimate of two founder genome equivalents remaining in Siminski and Spevek (2016)? This very low number is quite concerning and suggests that more genetic problems are likely in the near future and that the potential for adaptive genetic change is quite low. Has introducing genetic variation from northern gray wolves been considered?

What is the mean kinship in the Mexican population? Since this population descends from only a few pairs and litters, this might be quite high. What are remaining founder genome equivalents in the Mexican population? Are the Mexican packs nearby to each other or separated?

l. 1059. The results of Clement and Cline (2016) are quite surprising and unsupportable. At first appears that the only explanations for the statistically significant inbreeding depression from the earlier study of Fredrickson et al. (2007) to have disappeared is that it was a false positive or that purging has occurred, but neither of these explanations appear likely. Another possible explanation for no significant inbreeding depression effect from 2009 to 2014 is for the environment to have been improved enough due to diversionary feeding that litter size becomes similar for different inbreeding levels. It is well known that inbreeding depression is environmentally dependent with more inbreeding depression in more harsh environments. If

diversionary feeding were eliminated, it is likely that the negative association of inbreeding and litter size, inbreeding depression for this trait, would again be observed.

In addition, viability and mating success as well as litter size can contribute to inbreeding depression. Just because there is population growth does not mean there is no inbreeding depression. Further, a population can become fixed, or nearly fixed, for detrimental variants. In this case, there would be no evidence of inbreeding depression because virtually all individuals, independent of inbreeding level, would have detrimental genotypes. Given that there are only two founder genome equivalents remaining in the population, this could be a factor. Introducing unrelated individuals to the population, as when the three lineages were merged, can somewhat overcome this. Or introducing some northern gray wolves could have a positive effect, as did introducing Texas cougars into the Florida panther population.

Is there any evidence of genetic abnormalities in inbred individuals? In other wolf populations, inbred individuals have had spinal abnormalities, undescended testicles, or other morphological problems.

l. 1069. What does “genetically advantageous” mean in this context? Maybe this needs to be defined.

l. 1070 (also l. 1078). Has there been an analysis of what impact introducing cross-fostered individuals (in the number recently introduced) would have genetically?

Only one wild population in the US is planned. Is there a plan to bring wolves from Mexico to the US population? Because the viability of the Mexican population is questionable, this eventuality seems quite unlikely.

l. 1071. populations, not popualtions

l. 1102. It is not clear that there would be any problem with matings between wolves from the north and Mexican wolves. Before wolves were extirpated in the 20th century, they presumably moved substantial distances and mated with other wolves. Having these type of matings would be restoring what was the natural scheme before extirpation. For example, 8 Texas cougars were moved to Florida to mate with the endangered Florida panthers. All of their progeny were considered Florida panthers and were protected. An examination was made of the potential for “swamping” and it was concluded that with about 20% Texas ancestry that “bad” Florida genes would be eliminated (genetic rescue) and “good” Florida genes would be retained (Florida adaptation retained). A similar analysis should hold for Mexican wolves, supporting that some gene flow from northern gray wolves would be good for the population and would not result in “swamping.”

l. 1107. This statement about quickly swamping the Mexican wolf genome is incorrect, given the size of the present population and other factors.

l. 1115, Climate change should be included as a stressor.

l. 1133. An earlier analysis from two other Mexican wolf recovery teams suggested that suitable habitat for Mexican wolves is present both on the north rim of the Grand Canyon and northern

NM – southern CO. These suitable areas should be considered particularly since, given their proximity to the reintroduced population, there could be migration into these areas and, given climate change, suitable habitat for Mexican wolves would be predicted to be further north than the historical range.

l. 1143. Is there enough biomass in the areas in Mexico to sustain a population? The estimates from Mexican game farms is not appropriate and the level of natural prey biomass in areas where the wolves are proposed to live is likely to be too low to sustain a viable population.

l. 1204. Because of the very high human-caused mortality, this sensitivity to mortality is very concerning. More effort should be focused on reducing human-caused mortality.

l. 1223. What is ratio for cross-fostering? What is extra cost and effort for cross-fostering compared to releases?

l. 1265. These probabilities of extinction, 45% and 99%, are much too high. There needs to be larger populations and connections between the populations. If feeding is reduced in the MWEPA, what is the expected population size?

l. 1282. genetic, not gentic

l. 1288. As discussed above, this conclusion is likely wrong and is an artifact of the artificially enhanced environment. If inbreeding depression were allowed to occur, that is, more inbred individuals had lower fitness than less inbred individuals, then selection might be slowly purging this detrimental variation. By not allowing inbreeding depression to occur now, there might also be an accumulation of detrimental variation, which could be expressed in much lower fitness when the more benign environment of feeding is stopped. Is there a plan to keep feeding wolves indefinitely?

l. 1321. initiated, not iniated

l. 1328. Does the Service consider avoiding inbreeding depression as a condition for species recovery? In which “r” category does it fit?

l. 1328. Where does human-caused killing fit in?

l. 1335. The MWEPA population does not have any “redundancy.” The Mexican population will not increase its ability to rebound from catastrophic events.

l. 1340. Does representation actually mean ability to adapt? This does not seem obvious from the ordinary use of the word “representation”.

l. 1374. The conclusions from this appears that the Mexican population is very unlikely to persist because the size is too small and mortality too high. In other words, it seems unlikely that the Mexican population will contribute to recovery.

l. 1384. In both northern Rockies and great lakes, the recovery teams recommended three interconnected populations. In both situations, recovery has gone well so why not replicate those

guidelines. Is there some reason to ignore the areas of suitable habitat already identified on the north rim and northern NM –southern CO?

l. 1403. It is unlikely that there will be effective connectivity between these populations and that the Mexican population will be too small to contribute to recovery. In other words, any recovery measures should really consider only the US population.

l. 1418. Why not anticipate gene flow from north, as occurred in the past to increase genetic variation for future adaptation?

l. 1450. reeastablish

l. 1461. No functional connectivity between the US and the Mexican populations is likely so that the effective level of dispersal is likely to be zero.

l. 1468. Is there a plan for artificial connectivity between the US and Mexican populations. It is unlikely that this would benefit the US population which would be larger, better managed, and in better genetic condition. In fact, moving wolves from Mexico to the US might generally be detrimental to the US population.

l. 1476. What does “improve the genetic condition” mean?

Mexican wolf habitat suitability analysis in historical range in the Southwestern US and Mexico by Enrique Martinez-Meyer et al. (April 2017)

Appendix B of Draft Biological Report for the Mexican wolf

Comments by [redacted] submitted on 5/20/2017

General Comments

(1) The Mexican UMAs are the equivalent of game farms, generally with high densities of white-tailed deer for hunting and where predators are killed. In other words, wolves would not be allowed to live in these areas and they are not appropriate areas to estimate prey density in the areas where wolves would be allowed to live. The natural prey biomass in areas where wolves would be allowed to live is likely to be much less and might be at a level that is unsustainable for wolves.

(2) It is not clear that the extent of unpaved roads is taken into account in the Mexican habitat. This access, along with the high population density in some areas, would likely greatly reduce the potential viability of a wolf population.

(3) Is there drug-related activity in the areas designated for wolf populations? If so, how will this impact the wolf populations? And how will it impact the management and monitoring of the wolf populations?

(4) The level of artificial feeding now present in the Mexican population suggests that there is not a sizable enough prey base for a successful population. How long will this artificial feeding continue? Are the Mexican wolves eating natural prey? What kind?

(5) In general, it would be worthwhile to compare as much as possible the environment (including prey biomass) when Mexican wolves were first identified in the early 20th century to what it is now and what it is likely to be in the future because of climate change? These comparisons might show trends that could be used to identify what the range of Mexican wolves could be.

(6) It is important to recognize that Mexican wolves were not found in some sites because of presence of other subspecies of wolves that are now extinct. This does not mean that Mexican wolves cannot exist in those areas only that they might have been excluded because of the other wolves. Remember wolves once existed nearly everywhere from southern Mexico to the Arctic.

Specific Comments

l. 74. There are similar suitable habitats to these higher elevation areas to the north of I-40 that were arbitrarily not considered.

l. 282. Heffelfinger et al. (2017) is not an appropriate reference for genetic analysis, it is a review that generally dismisses genetic information.

l. 284. Why not all suitable habitat rather than arbitrarily restrict it to south of I-40. Others have identified suitable habitat for Mexican wolves north of I-40, particularly on the north rim of the Grand Canyon and northern NM – southern CO.

l. 322. When were these climatic variables measured? Much of the distribution data is from nearly a century ago. Shouldn't the climatic data be from a similar period. It seems inappropriate to associate current climatic data with past distribution data because current climatic data might reflect climate change that has already occurred.

l. 390. Although I am not familiar with the different algorithms given in Figure 3, it is interesting that the BRT algorithm suggests that there is good habitat north of I-40. Perhaps how the input data are determined or how the analysis is carried out inappropriately reduces the range for the other algorithms.

l. 403. "performed better", this seems somewhat arbitrary.

l. 419. The realized distribution of the Mexican wolf population in the Blue should be used in a similar analysis. This would obviously demonstrate that the distribution of Mexican wolves would include very different more northern species and subspecies of animals, including elk.

l. 443. It seems unlikely that a Mexican wolf population could survive in southwestern Texas and makes this analysis of climatic suitability suspect.

l. 452. This does not make sense because the reintroduced population is on the northern edge of this distribution and appears to be growing.

l. 453. Should the MWEPA be rejected as habitat or is this analysis suspect because Mexican wolves are doing well where they are in MWEPA?

l. 472. The low quality of these data makes the subsequent analysis suspect. Shouldn't an evaluation wait for better quality data?

l. 503. Why not use current information from the reintroduced population in the Blue? These realized data might be much better than McBride's data just before their extirpation nearly 50 years ago.

l. 520. These data could be used to determine the realized niche for Mexican wolves.

l. 553. Why not include more northern areas (not including them artificially reduces the suitable habitat)? How much category 70 lies north of I-40?

l. 623. The human density in much of the area in Mexico appears to be higher than recommended for successful viability.

l. 655. Does the road density in Mexico include all the unpaved roads? It should include all roads that provide access to these areas to determine their impact on wolves.

l. 669. The ungulate density probably will be the major factor determining viability of the Mexican population (if human-caused mortality is controlled).

l. 680 and following. There does not appear to be good estimates of UBI in Mexico and estimating on UMAs (managed hunting areas) is probably not a dependable measure for other areas where the wolves might be allowed to exist.

l. 684. What natural prey, and how many, have the reintroduced Mexican population taken? This could give some insight into how the Mexican prey base might be utilized.

l.710. How could the estimates be 10X greater in the Mexican UMAs than in AZ and NM when the habitats are similar? Does this reflect inaccuracies or that the UMAs foster unnaturally higher densities for hunting.

l. 805. There are many elk and mule deer north of I-40 that Mexican wolves could use as prey.

l. 811. What happened to the areas north of I-40 that had all the elk and mule deer?

l. 820. “the UBI in Mexico are less….”

l. 908 and following. Because the availability of prey is so essential to wolf population viability, it does not make sense to consider scenarios that exclude prey biomass.

l. 997. The areas in Mexico do not look good because of the low prey biomass. Why not look elsewhere in the US (north of I-40) for areas with enough biomass to support a population?

l. 1024. Evaluating safety conditions for field crews makes these sites sound problematic.

l. 1027. Is this realistic?

l. 1156. Effective movement across the US-Mexico border is unlikely now and will be very unlikely when the current US administration builds a wall along this border.

Population Viability Analysis for the Mexican Wolf by Philip Miller (5/1/2017)

Appendix A of Draft Biological Report for the Mexican wolf

Comments by [redacted] submitted on 5/26/2017

General Comments

(1) What does the Mexican population(s) add to the viability of the wild metapopulation? What is the viability for the US population alone and for combined US and Mexico populations? It seems unlikely that the Mexican population(s) adds to the overall survival probability or to the survival probability of the US population.

(2) It is likely with the small number of founders in this population that inbreeding will impact other components of fitness besides litter size, such as viability and mating success. Even though there does not appear to be evidence for this now, probably partly because these are more difficult aspects of fitness to quantify and the sample size might be small, it is very possible that these or other traits will be influenced by inbreeding. This impact should be included.

(3) Recent estimates of inbreeding depression in wild populations are often quite high (O'Grady et al. 2006, Biol. Cons. 133:42-51; Hedrick and Garcia-Dorado 2016, Trends Ecol. Evol. 31:940-952). Largely ignoring the impact of inbreeding depression appears contrary to these data. Further, the Mexican wolf population has a smaller number of founders and now founder genome equivalents than nearly all the populations examined in these articles, suggesting that inbreeding depression might be even larger than in those examples. The incorporation of inbreeding depression might significantly increase the probability of extinction.

(4) When is the translocation from the MWEPA supposed to start? On l. 584, it states that year 2 in the simulation corresponds to calendar year 2017. In the document, it says after 2 and 7 years for the north and south areas in Mexico. This seems completely unrealistic. Shouldn't there be some population level for the MWEPA to reach before wolves are translocated from there to Mexico? With 113 wolves in the MWEPA in 2017, it is unlikely to be much higher in 2 years and certainly not near 300. Moving adult pairs and their pups from MWEPA, presumably the breeding pairs in a pack, when the population level is only slightly above 100 would probably have a big detrimental impact on the recovery of the population in MWEPA

(5) The number in MWEPA is arbitrarily set at 300, 340, and 379 and kept there presumably by hunting (the numbers could be much higher, l. 334 says the carrying capacity is 1000) while the numbers for the two Mexican populations set at 150, 200, and 250 (carrying capacities of 300 and 350, l. 335) apparently because of differences in suitable habitat or other factors. In other words, the carrying capacities are reported to be quite different (the MWEPA area is >3 times as good) for Mexican wolves than the areas in Mexico while the "management targets" are more similar. Why?

(6) The MWEPA had stalled until 2009 when artificial feeding was increased and removals were reduced. Assuming that removals were a major factor stopping the MWEPA from increasing before 2009, it seems logical that the planned translocations from the MWEPA to Mexico would

similarly keep the MWEPA from increasing. Further, taking packs from the MWEPA might have an even larger impact on the MWEPA population than the removals did. In other words, what is the justification for such translocations when there appears to be strong evidence from the pre-2009 years that removing wolves inhibits population growth and potentially viability?

On l. 345 it states that the management targets are based on what is "socially acceptable in light of the expected ongoing issues around livestock depredation and other forms of wolf-human conflict." Are these management targets based on some evidence? Are they also based on what the supposed impact of wolves would be on game species, such as deer and elk?

(7) The scenarios investigated here assume that adult mortality is the most important parameter. Are there other parameters that are also important, such as litter size? Has a sensitivity analysis of life history parameters been carried out to identify what parameters are most sensitive to appropriate levels?

(8) The simulation of the impact of these scenarios on heterozygosity is the traditional approach used in PVA and gives the expected loss of genetic variation and other genetic parameters. However, the level of genetic (genomic) variation in Mexican wolves is already known to be quite low. It would be useful to use those genomic values to confirm that the loss of genetic variation predicted by these simulations is consistent with that actually realized in earlier generations. If the management actions go forward as suggested here, it would be useful to document the realized changes in heterozygosity by genomic analysis to determine if the actual changes are consistent with those predicted.

Specific Comments

l. 145. How about a figure with inbreeding coefficient and lineage contribution over time? Or founder genome equivalents over time?

l. 149. It would be useful to give somewhere approximately how many years there are in a generation.

l. 190. How does the value of 0.78 relate to the number of packs, that is, does each mated female have her own pack? Because pack behavior is so important in wolves, it would be good to discuss this connection.

l. 202. Are there any instances of parent-offspring or sibling matings in the population? If so, should they then be incorporated in the simulation?

l. 218. Are these values (95% and 80%) based on observations? Please explain.

l. 225. This assumption seems questionable. The lack of relationship might be based on the more benign environment resulting from artificial feeding. In other words, inbreeding depression might well be present if or when feeding is stopped. At that point, inbreeding depression might even be higher because the effects at lower inbreeding levels were ameliorated.

l. 273. Is it reasonable to use these data? Is feeding going to continue to keep up pup survival? Is the lower rate of removals going to continue? Perhaps scenarios with the earlier pup survival and earlier removal rates should be examined.

l. 305. Is the inhabited area of the MWEPA at a low density for wolves and wolf packs? Is it likely that this area could have many more wolves or is it at or near carrying capacity? Based on these answers, perhaps density dependence should be added.

l. 311. How about human-caused mortality as a catastrophic event? Or does it occur at the same high level every year?

l. 318. Is there any evidence of disease in wild Mexican wolves?

l. 325 Do some canid diseases influence reproduction or other components of fitness?

l. 345. Is this carrying capacity also determined by the wolf-human conflict perceived by the impact of wolves on game animals, that is, too many wolves means too few deer or elk hunting permits? Please state whether this is part of the consideration.

l. 399. The effective dispersal between the MWEPA and the northern Mexican population is likely to be zero when the current administration builds a wall along the border. Is the rate of dispersal 0.00175 different from 0 in the simulations? Does this rate reflect assisted migration? Is this a two-way rate?

l. 402. Does the 37.5% value include the probability of successful reproduction of migrating wolves?

l. 407. It is somewhat unclear why the captive population is so thoroughly simulated. This seems to make the whole presentation and simulation unnecessarily complicated. Presumably it could just be assumed that individuals of a particular ancestry are available for introduction when that is done in the simulations.

l. 480. Is the effect of inbreeding from the three different lineages the same? It is very possible that inbreeding from some lineages has a bigger, or smaller, impact than that from other lineages.

Table 1, p. 12. These values are very different from the wild population. Would this have any impact?

l. 535. Is it likely that wolves will be translocated from MWEPA to one of the Mexican populations?

l. 551. It would be important to know how much larger this impact is than what actually occurs because it might make translocations appear more significant than they are in fact.

l. 635. This notation is confusing (for starters EIS could be left off). Does EIS20_20 mean the number of 20 adult pairs from SSP to SMOCC? In the table, it looks like five years of 2 pairs each or 10 pairs. What does EIS22-22 mean? Where are there 22 adult pairs?

l. 676. About how many generations is 100 years?

l. 753. This is a very big difference from 25% to 30% mortality. In the past, has adult mortality been near 30%? If removals are included then the effective mortality was much larger than 30%, wasn't it? From the discussion, it appears that for a timeframe of 200 years, even with 25% mortality, the probability of extinction might be high.

l. 942. Why is the probability of extinction lower for SMOOC-N at 30.9% mortality than for MWEPA at 30.9% when SMOOC-N is at 200 or 250 and MWEPA is at 300 or more in Figure 3? The early translocations and releases should not have much influence by 100 years.

Figure 11, p. 30. This suggests that translocations would greatly impact the MWEPA population. Why would this be allowed? There appears to be a cost to the US population to having the Mexican populations. Why would this be acceptable?

Addendum

p. 1, l. 13. Why are wolves going to be translocated from the MWEPA when the population number is not close to this value?

p. 1, l. 34. It is surprising to see all these various release strategies from the SSP when there has been such opposition to this in contrast to the cross-fostering strategy. Is there some reason that the problems with releasing wolves from captivity are ignored here that were made into such a big issue by Heffelfinger et al. (2016)?

It appears that these simulations suggest that releasing more wolves would decrease the probability of extinction and retain slightly more genetic variation, not surprising. Are these scenarios with this level of release practical or possible?

Is the plan to monitor whatever scenario is chosen and modify the scenario in the appropriate way to make the population more viable? Or is it likely that once chosen, the scenario will not be modified.

1 **I. BIOLOGICAL REPORT. DOCUMENT REVIEW**

Line	Recommendation, suggestions, comments,
80	Space Protegidas (CONANP)
102	Add; a special thanks to the ranchers and cattlemen who support the program and allow the released wolves stay in their ranches
269	This figure title could change to; Approximate range of Mexican Wolves released Chihuahua State in Mexico. Comment; I don´t know if also the reintroduced wolves are in the state of Sonora? Attached is a figure showing the municipalities with wolf reports in the state of Chihuahua (2002 – 2017).
285	Could add a comment; However, there are reports of wild wolves roaming free in Sierra del Nido in the State of Chihuahua, México, I suggest more research and field work is needed.
331	Resources. add “s”
373	Vonholdt not vonHoldt
379	Vonholdt not VonHoldt
407	Question; Does this range include the intermediate Sierras (Nido, Maynas, San Andres, Namiquipa) as well as the intermountain valleys?? Historically, most wolf’s populations were present in these areas.
409	Bailey 1931. Is not cited in literature
442	Space between words
454	(USFWS 1996) in not cited in literature
541	add; lack of knowledge and attitude against the wolves from residents
541	(Sneed 2001) in not cited in literature
589	Space between words
613	Check the total numbers in Table 1; Total BLM 19,085,000. Total Tribal 9,680,300, Total private 30,727,200 and grand Total 98,239,700
667	Gasaway et.al. 1993 or 1992?
680	Merkle et.al. 2009a or 2009??
717	(Parson and Nicholopolous 1995) Is not cited in literature
718	(Bailey 1931, Leopold 1959) are not cited in literature
746	Bower or Browyer??
775	Estes et.al. 2011. Is not cited in literature
799	2007 in Bailey as 2011 in Johnston citations are not cited in literature
813	(Ballard et.al. 2003) is not cited in literature 2001?
1001	A genetic study of the Sierra del Nido wolves is required in order to know if the released animals from ranch “Los Encinos” still in good genetic conditions, and find out if native wild wolves or hybrids are present in the area
1080	(AMOC and JFT 2005) is not cited in literature
1097	Comment; When McBride trapped the male wolf in Maynaz Ranch, in the State of Chihuahua, the wolf was providing food to a female dog and it´s pups, that were hybrids (half dog and half wolf). One of them, was sent to the SubDirector of Fauna Silvestre in Mexico, city, and another was given to a rancher, who killed it because it started to bite and cause ~~of~~ damage to cattle.
1135	There is a good suitable habitat in Sierra del Nido, Buenaventura and Namiquipa, continuous reports of wolf sights are common from ranch owners and cowboys talks.

1182	Most of the communal properties show a general overuse of their natural resources in their land, overgrazing, soil erosion, over use of trees and wood for house fire, land opening for dry farming and water pollution around their houses. Most of these areas, don´t have any type of management programs for livestock, range management, forestry, soil and water conservation. Because that in many cases the owners do not live in those towns, they show lack or little interest to keep their land and the ecosystem in good condition.
1537	Bowyer or Brower?
1597	Include year after Crowder. 2015
1632	Larsen. 1992 or 1993?
1701	Is not in the MS content
1745	Is not in the MS content
1765	…….Ballard. 2009 or 2009a?
1804	Is not in the MS content

Comments;

According with the review agreement instructions, most of my effort has been focus on the Biological Report. However, both of the addendum reports (PVA & HSA) were also edited in general.

My technical opinion of the reports is; that those are excellent documents, the scientific basis is strong as well as the analysis that it is complete in both documents (PVA & HAS) the conclusions are integrated in the biological report in an excellent way giving a comprehensive value to the MS.

My contribution could be more related for the future of the Recovery Program because the possibility to change and refresh the genetics of the species and obviously it´s PVA, if a serious study is carried on to determine the presence or/ hybridization of the released wolves and the native wild ones in Sierra del Nido. This suggestion is because, according with the Biologist in charge of the enclosure, at the time that it was used (2000); at least two wild wolves came to the enclosure and tried to fight with the captive ones. Also, howls were heard and signs as feces and tracks were found in the nearest canyon west to the enclosure. Then, it's a possibility that the wolves that were released in Sierra de la Campana (el Nido) could breed with wild ones.

I did some comments, suggestions and questions that could support the reports improvement. In the PVA report review, I understood the report but I have not enough expertise to suggest changes or modifications.

Are some Personnel communication citations in the BR manuscript? If is this possible to obtain the source of some specific information?. Like the pack size (2 – 14) where this data come from?, Or the no hybridization with dogs. Because I know that the hybridization was a key factor in the successful trapping done by McBride of the male wolf in Chihuahua. Maybe a double check with other biologist could help, but I don´t know if is too late?.

In order to obtain a better map for future wolf reintroductions, several factors must be considered. The anthropogenic activities in USA and Mexico are totally different. Cowboys

and ranch owners in Mexico, don´t cross and walk the total area of the ranch, they use horses and they only look for their cattle where they detect a sign of their presence, like droppings or tracks, so there are many areas that will be left alone without disturbing them. In USA, camping, scouting and lots of people use the land for recreation and travel, and sometimes they will take their dogs with them, living urine and scat marks in the area.

In Mexico towns and ranches are isolated and disperse, their main activities are dry farming and cattle raising, some people will have goats and sheep and chicken for their own diet that could cause conflict with wolves. Ranch owners are no easy people to obtain permission to work in their properties and never listen about the wolf role in the ecosystems, because they consider the wolf as a cattle´s predator.

Protected Areas in Mexico are not managed and work as ~~there~~ they are ~~is~~ in the US. The land is private or communal and government can´t do anything that the owner would not like to do. Most of the Protected Areas don´t have an approved Budget to operate.

Population of mountain lions in Mexico is not well known, they can affect wolf survival and dispersal. On the other hand small mammals as peccary, squirrels, rabbits, and turkey, are not mentioned as prey.

A key factor in Mexico for the wolf recovery program is to increase the environmental education programs as well as information about the wolf and its role in history as well as in the ecosystem, conservation and protection needs.

Answer to the questions.

1.- Has far as I know, this report provide an adequate review and analysis of the factors related with the Mexican wolf persistence in Southwest USA and Mexico. However, seems like ~~is~~ other available information from Mexico that I didn´t see it (more details in the review comment).

The persistence has an adequate review (PVA Report) that includes several analysis of factors as well as scenarios of the wolves' population responses.

2.- To my knowledge the assumptions and conclusions on current population trends and stressors are logical and adequate. Some specific stressors could be different in Mexico than in USA because land tenure, wilderness activities, law enforcement, security and ranching patterns.
As far as I review the population conclusions are OK. But, it could change completely if the Sierra del Nido population (coming from the Encinos) and the released wolves had the chance to breed with wild wolves, if it is proved and it could enrich the genetic diversity, and lower the risk of the wolves´ population decline.

3.- I agree completely with the final conclusion about the wolves populations in terms of the potential to provide representation, resiliency and abundancy for its recovery. In my consideration other possible populations in the wild must be checked.

4.- Yes, I strongly agree that the recovery strategy as well as the criteria is supported in scientific information.

II.- MEXICAN WOLF HABITAT SUITABILITY FINAL REPORT REVIEW

Line	Recommendation, suggestion or comment
198	Add. Moctezuma- Orozco et.al. 2011
199	In 2005, the Mexican Government released the first pack of wolves in Sierra del Nido.
205	Her or it´s
212	Add. Unfortunately not datas from the released wolves of Sierra del Nido has been obtained.
220	In Carroll et al the year 2003 is not cited in literature
260	Soberon & Peterson 2005). Is not in cited literature
266	(Soberon 2007). Is not in cited literature
424	(Brown 1982 or 1983??
444	Must be western Sonora (The Sierra Madre area of this state) not western. Also, could include western Chihuahua?
469	Space
506	A male wolf was shot in 1968 in the open grasslands east of Sierra del Nido the pack was composed by five wolves and the skin is in NMSU.
613	Mladenoff et. al.
781	(Rstudio Team 2016) or RStudio?
866	Paquet et.al. (2001)
1024	Comment; Are you thinking in the behavior study of released wolves?
1065	….protected areas Sierra Tarahumara and Sierra del Nido)
1095	, Paquet et.al. 2001
1097	, Paquet et.al. 2001
1125	, Paquet et.al. 2001
1130	, Paquet et.al. 2001
1298	In literature cited; Elith J. et.al. 2006. Need the other authors
1487	Is not in the manuscript
1490	Is not in the manuscript
1493	Is not in the manuscript
1496	Is not in the manuscript

Comments;

This Final report is an excellent source of information to know possible habitat scenarios of the future areas for Mexican Wolf recovery efforts. Appears that is not a big difference between habitat suitability scenarios (pessimistic – intermedia – optimistic) under different factors and analysis.

I agree that the deer counts in Mexico have a big “bias” because the economic value of the deer species (white tail and mule deer), the careless of training of the technicians in charge of the UMA and that is not required a specific method to do deer inventories.

The ranching operation in Mexico is different than EUA. Dirt roads, that connect ranches and towns usually are scarce and in bad conditions because there is low or not maintenance at all. Then, the impact of road on wolves could be different than in USA. Mostly horses are use in

Mexico for transportation, very few 4 wheelers and two wheel bikes, because they are very expensive to use, and ~~there is a~~ lack of gas stations where they can get fuel. Some special considerations are the attitude of cattlemen against wolves, they consider wolves bad for cattle business because are predators, and the common use of poison for predator's control.

In most of the figures and scenarios in the Mexican Wolf Habitat Suitability Analysis Final Report, central part of Chihuahua has a high value region for the recovery program. These maps confirm the high suitability of this area. In addition, several recent reports of wolves in Buenaventura, and Namiquipa municipalities and Sierra del Nido (Chihuahua County) in the State of Chihuahua support this comment. Most of this area is private property and have ~~a~~ high white tail deer population.

III.- **PVA MEXICAN WOLF REPORT REVIEW**

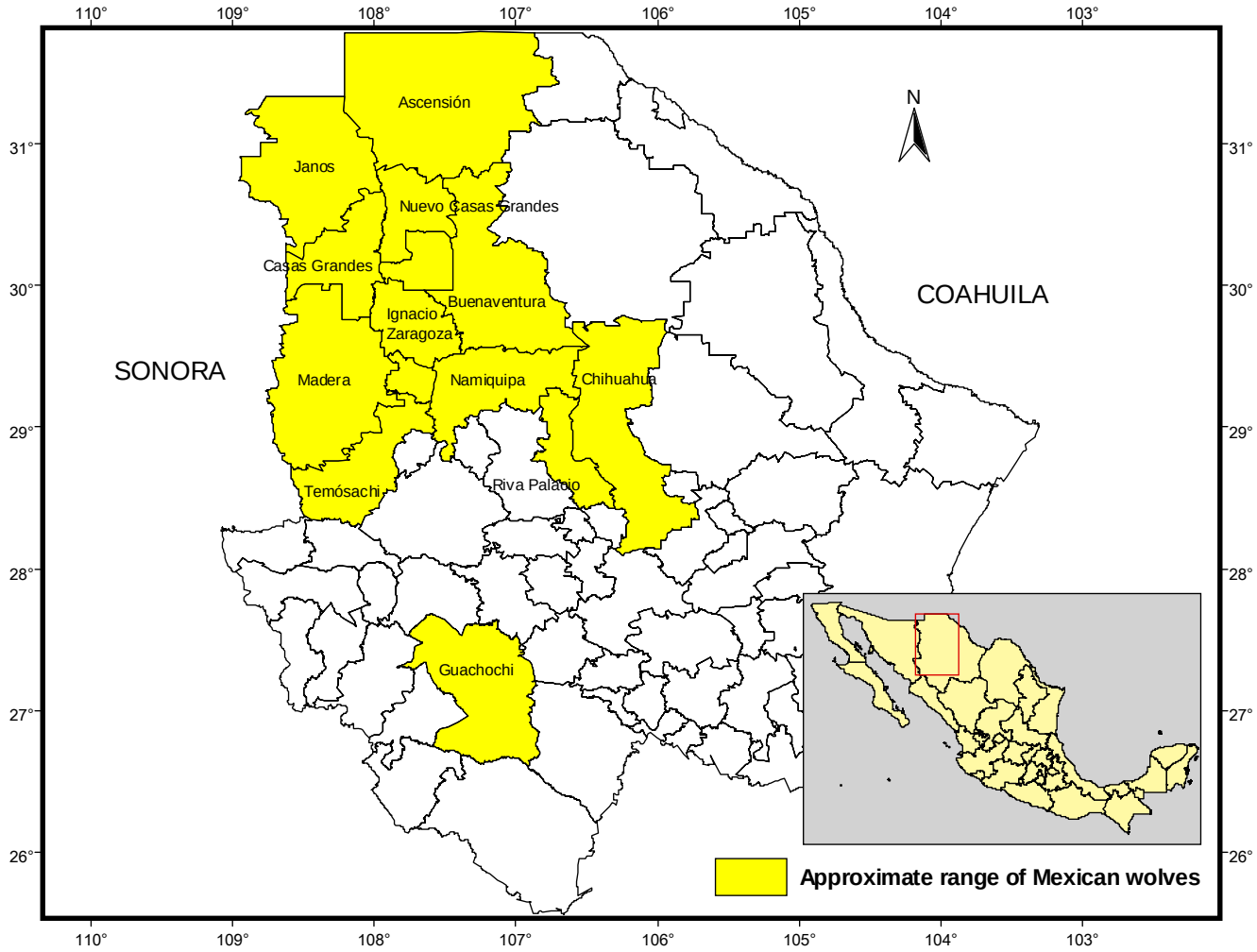
Line	**Recommendation, suggestions, comments,**
153	(Hedrick et.al. 1997) is not in references
228	Fredrickson et.al. (2007) is not in references
333	Martinez-Meyer et.al. (2017) is not in references
388	Martinez-Meyer et.al. (2017) is not in references
413	(Hedrick et.al. 1997) is not in references
1574	Hosmer and Lemeshow 2000) is not in references

* Some articles are not in references, I don´t know if each appendix has his own references

Comments;

This Report is an excellent source of information. The PVA Model develop~~ment~~ as well as the input data for different purpose, is clear defining the ~~used~~ terms and the data analyses. The extinction probabilities is also, widely discussed under different probabilities and scenarios. According with my expertise I have no suggestions for this addendum.

It´s a third population present in Sierra del Nido, Chihuahua, Mexico. Some recent reports of wolves have been obtained by cattlemen (Barraza A., February 2017, Mayagoitia R. 2016, Rodriguez 2016, Bermudez 2016). Hunters, as well as technical wolves reports from SEMARNAT and PROFAUNA (since 2012, until 2017), show that wolves are located in some areas of this mountain, including sites as ranch Las Varas, Cañon del Obscuro, El Mesteño, Terrenates, San Andres, Manta Negra, and other parts of this region. This population could have new genetic to refresh the PVA. However more field data and a study are required to obtain population data (relative numbers and distribution) as well as genetic information. This study could be done by USA and/or Mexican technicians and obtain samples for DNA analysis.



115

116 Figure. Distribution range of Mexican Wolves in Chihuahua, México

117 (data obtained from reports 2002 – 2017).

118

The following represents my comments on the Draft Biological Report for the Mexican Wolf (*Canis lupus baileyi*) (U.S. Fish and Wildlife Service 2017). In so doing, I specifically addressed the three questions posed in an email received from Mr. Dwight Clark (dated: 2/13/2017) and have commented on other issues that I feel are germane and that I have specific expertise or experience in. In particular, I have also reviewed the population viability analysis (PVA) by Miller (2017). Although I perused the final report on the habitat suitability analysis by Martinez-Meyer et al. (2017), I do not consider myself proficient in species distribution modeling and therefore have primarily restricted my comments to the two aforementioned reports. My comments on all three documents are primarily embedded within my answers to the three questions posed by Mr. Clark.

Finally, my comments are my own and although I am a professor at [redacted] my comments are not a reflection of [redacted] opinions or views on the status and recovery of the Mexican wolf.

[redacted] Professor
[redacted]

1. Does the draft report provide an adequate review and analysis of the factors relating to the persistence of the Mexican wolf population in the Southwest and Mexico in terms of demographics, habitat, disease and predation, human-caused mortality and genetics?

The biological report does an adequate job of recounting both the natural and unnatural history of the Mexican wolf, but there are also areas where analyses, attribution, and presentation of information could be improved. One component missing in many instances is a measure of precision. Without having some estimate of variance, means are not very informative.

Demographics

L487-489: There is no mention of the variation (variance or standard errors) associated with the mean estimates of survival, nor is there any mention as to how these estimates were obtained. Were known-fate models used with radio-collared individuals? Were marked animals used in a capture-mark-recapture framework? A combination? This is not clear.

L750-752: There was a nice modeling exercise by Dr. Steve Kohlmann that compared the relative impacts of wolf vs human harvest to the Gila Elk herd in NM. He worked for the NM Dept. of Game and Fish at the time as the elk biologist. It might be prudent to examine his report.

L852, Figure 7: Although this graph reveals that Mexican wolves have been increasing, there is no way of evaluating the uncertainty in these estimates. The figure caption includes "Annual Minimum Population Estimate of Mexican Wolves in the MWEPA" – are these the minimum number known alive? What method of estimation was used? Was the probability of detection estimated?

L872-875: In Table B.2 of Appendix B in Miller (2017), there were 3 models that had substantial weight, all of them included the age of the dam cubed, indicating that the probability of producing a litter may be a non-linear function of age of the dam, and two models implicated supplemental food and the inbreeding coefficient of the pups, respectively. In this set, the beta coefficient for the age of the dam cubed, i.e., the effect size of this factor, was extremely small and near zero, whereas the beta coefficient for supplemental food had a 95% CI that encompassed zero (Table B.3), so it cannot be said that its effect does not equal zero. The effect size of the inbreeding coefficient is not presented so it is difficult to evaluate but based on the difference in AIC, which reflects the difference in the deviance owing to the small difference in K, the inbreeding coefficient of the pups explained little of the variation in the response variable. Thus, it does not appear that any of these three explanatory variables had any appreciable effect on the probability of producing a litter in this analysis. What you might want to explore here is normalizing the covariates to put them on the same scale prior to analysis. This maybe one reason why the Age of the Dam cubed has such a small effect size. Table B.4 is another complimentary analysis that used a different set of data and here the age of the dam is categorical and both the inbreeding coefficient of the sire and pups are used in this model set, yet these were highly correlated (r=0.658) and therefore should probably not be used in the same model set. In this analysis, the inbreeding coefficient of the pups does influence the deviance, but its effect appears much less than the age of the dam; however, together there is strong evidence that both covariates are important (removing the effect of sire results in a model weight for Dam Age + IC

of pups = 0.82). The question here is, which analysis do you believe? Age of the dam cubed appears to have a very small effect size in the first analysis but when categorized a strong effect in the second, and in the first analysis the inbreeding coefficient of the pups appears to matter little, but in the second it appears influential especially when the IC of the sire is removed. This is where a careful *a priori* assessment of which factors to select and their form should be implemented (Anderson 2008). Further, there is model uncertainty here that is not discussed nor is this uncertainty adequately addressed in the PVA (Miller 2017).

L883-887: Again, no mention of the precision of these survival estimates or the methods used to calculate survival.

L897-930: This is a nice summary of how the number of removals most likely affected population growth, but these data, along with other known-fate mortalities and unknown mortalities, should be put into a succinct table. This table should show number of mortalities and their fates, number of removals, estimates of survival and associated variances, and correspond to the phases that are mentioned, which represent different management phases of the project. Further, I think it would be important to show just how many mortalities have been caused by humans, in particular, those due to poaching. You may also want to look at a very recent paper that suggests that the impact of poaching on wolves is typically underestimated (Treves et al. 2017).

Habitat/Distribution

L573-576: Wahlberg et al. (2016) does not appear to be an authoritative source on the water requirements or resource selection of wolves. Such a statement, "The amount of riparian vegetation... is very important to wolves because it provides water, ... cover, ... and often serves as a means of easy movement..." should be supported with evidence. For example, based on allometric relationships for a carnivore in the field ($\ln y = -0.605 + 0.795 \ln x$, where y is ml water/day and x is body mass in g; Nagy and Peterson 1988) a 32 kg Mexican wolf would require ~2083 ml of water per day. Since a typical Mexican wolf consumes from 1.4 to 3.25 kg of live prey/day (see L757-758), and because vertebrate prey is ~65% water (McNab 2002:175), a wolf would acquire 900 to 2100 ml of preformed water per day from simply consuming its prey alone. But water is available in 3 pools: free water, preformed water, and metabolic water. Metabolic water is generated from the oxidation of food: starch, fat, and protein all yield water when catabolized and protein yields the least, 0.40 g water/g food (McNab 2002:178). How much carbohydrate, fat and protein are in a typical deer leg is hard to gauge, but meat is typically about 20% protein, so a wolf eating 1.4 to 3.25 kg of meat would get another 112 to 260 g of water by metabolizing protein. So, if a 32 kg wolf consumed 3.25 kg of meat per day, it would obtain 2100 ml of preformed water and 260 ml of metabolic water at the very least, for a total of 2360 ml of water and it would exceed its estimated water requirement of 2083 ml/day without drinking any free water. Many desert-adapted canids are very efficient at conserving water andn do not need to drink to get meet their water requirements (Golightly and Ohmart 1985, Schmidt-Nielsen 1964) and Mexican wolves might be similar.

My point of this "back of the napkin" exercise is that statements purporting the import of certain biotic or abiotic requirements for an endangered species need to be based on evidence and the

evidence cited needs to be an appropriate source. A counter-argument can always be generated for insufficiently substantiated statements.

L685-686: A summary of wolf resource selection would be valuable to include in this report.

Disease and Predation

There have been rabies epizootics in the Gila and in eastern Arizona primarily due to a gray fox rabies variant over the past several years. This may be one scenario to model as a potential catastrophe in a PVA.

Human-caused mortality

L547-549, L837-847: In a summary of the mortality factors impacting Mexican wolves within the Blue Range Wolf Recovery Area from 1998-2011, >81% of all mortalities were human-caused (Turnbull et al. 2013). Given that the Service now has more data from 2011 on, I think it would be prudent to specifically summarize the mortalities of Mexican wolves in the U.S. and list the factors responsible in the report.

L961: Here you mention that poaching was responsible for the deaths of 6 Mexican wolves in Mexico, but 43 illegal shootings occurred in the Blue Range Wolf Recovery Area in AZ and NM between 1998-2011 (Turnbull et al. 2013) and this is not even mentioned. That does not seem to be a very fair presentation of what has been happening in both countries.

Genetics

L277-285, L377-390: Given that there were only 7 founders, the amount of genetic variation present in the founding population, relative to other small and naturally large wolf populations, should be put into context. There is a rich literature on the molecular genetics, phylogenetics and phylogeography of the Mexican wolf that has been addressed in this document (e.g., Garcia-Moreno et al. 1996, Hendricks et al. 2015, 2016, vonHoldt et al. 2011), but additional, important work has not been mentioned (e.g., Fredrickson et al. 2015, Koblmüller et al. 2016) and the major findings of several studies have not been elaborated on. For example, there is extremely low genetic variation in the current founding population, yet there is notable genetic uniqueness in the Mexican wolf lineage with respect to other NA wolves across multiple genetic markers (mtDNA, mitogenomes, microsatellites, and SNPs), and there is genetic support that Mexican wolves are potentially a unique form that has adapted to local environmental conditions, although the decimation of the Mexican wolf from anthropogenic causes and the subsequent loss of genetic variation in the subspecies clouds this issue. These are important topics to discuss in some detail as they point to a rich literature that shows multiple lines of evidence that this wolf lineage is unique and worthy of protection. I think a stronger case could be made here to support why we need to save this subspecies.

L987: Although the "genome bank" is described later (L1012), you might want to describe it at first mention here.

L1057-1059: The analysis in Appendix C is interesting and from my naive view very solid, but I have a couple of concerns. The first concerns the decision to "throw out" pairings that did not produce a litter. If inbreeding depression was operating, couldn't its potential effects include aborted pregnancies or high-mortality of neonates? I think there needs to be more justification as to why these data are being ignored or at least a more thorough explanation as to how this is handled in the PVA. The second is the comment regarding methodology and potential differences in detecting the number of pups. I would like to know more about how the methods changed and how this may have influenced the probability of detecting and/or counting pups. It does appear that this effect was controlled by sample selection in this analysis, but what does it mean for estimating fecundity for other downstream analyses?

L1076-1087: The cross-fostering appeared to work well and was a neat idea to implement.

L1089-1091: Although I would agree that hybridization with coyotes and domestic dogs should be avoided, hybridization with other wolves, that is, individuals of the same species, should most likely be welcomed as a way to introduce new genes into the Mexican wolf subspecies.

2. Are our assumptions and conclusions regarding current population trends and stressors logical and adequate?

Within the report (L1115-1119) the following stressors are emphasized: "The most important biological stressors, or conditions, that may influence the current and ongoing recovery potential of the Mexican wolf include: 1) adequate habitat availability and suitability; 2) excessive human-caused mortality; 3) demographic stochasticity associated with small population size; and 4) continuing or accelerated loss of genetic diversity in the captive or wild populations."

With respect to #1, the Service should have a ton of data on resource selection of GPS-collared wolves. I think such an analysis is essential and that it should be compared to the work completed by Martinez-Meyer et al. (2017) to address the issue of suitable habitat and its availability.

With respect to #2, a thorough presentation of the impacts of all human-related mortality is needed. The Service has also implemented management actions, such as cross-fostering and supplemental feeding, that appear to have helped, but it seems that other actions may have been implemented to thwart the high mortality caused by humans (L1215-1217). Although it appears that law enforcement has been successful have other programs such as compensation for depredations, alternative forms of livestock husbandry, the use of guard dogs, etc. been considered or have they been successfully or unsuccessfully implemented? A summary of all human impacts to Mexican wolves followed by a description of management actions, successful or unsuccessful, that have been implemented to address these impacts would be useful. A summary would show what the Service has tried, what it hasn't, what has been successful and what hasn't.

Regarding #3, the current PVA (Miller 2017) does not appear to adequately take into account the various types and levels of uncertainty that are inherent to a PVA, and I do not believe that this analysis has adequately separated environmental from demographic stochasticity. With regards

to my first comment, it appears that the "top" model is used from the various analyses described in the Appendices even when there is considerable model uncertainty, and further, uncertainty in the parameter estimates derived from these models also does not appear to be incorporated into the PVA. These types of uncertainties would generally tend to increase the probability of extinction over most scenarios (Bakker et al. 2009). There have been calls for incorporating such uncertainties into PVAs to make them more accountable (Bakker et al. 2009, Elner et al. 2002, Harding et al. 2016) and they should be considered here as well. With regards to my second comment, it does not appear that the Vortex platform allows for a separation of environmental vs. demographic stochasticity or if it does, it was not clear to me how. As an example, let's look at the *Probability of litter production among paired females* (Miller 2017: L205-222). This is a logistic function that predicts the probability that a litter is produced and it contains two main factors: Age of the dam and the inbreeding coefficient of the litter. These might be considered environmental factors that contribute to variation in this probability. That is, a female of a particular age with a litter of a particular inbreeding coefficient will have X probability of producing a litter, change these factors and you change the probability. If one were to draw from a distribution of female ages and litter inbreeding coefficients to estimate this probability, this would be a way to model environmental stochasticity, but not demographic stochasticity. One could implement demographic stochasticity by subsequently using a Bernoulli trial where a decision is made that given these conditions, a litter will be produced or not – this is demographic stochasticity, a random factor that influences a vital rate. It does not appear that this Vortex analysis is accounting for both of these forms of stochasticity or for other forms of uncertainty, thus I believe that all of the estimates of the probability of extinction are biased low in this PVA.

Regarding #4, it appears that there has been a lot of thought into preserving the genetic variation present in the original 7 founders. But therein lies the rub. There were only 7 founders. Allowing breeding with other wolves may be a positive outcome, after all, they are the same species.

3. Does the report adequately consider what the species needs to maintain viability in terms of resiliency, redundancy, and representation?

Regarding resiliency, I do not think the current PVA does an adequate job of handling different levels of uncertainty. Further, I think there needs to be a type of sensitivity analysis that examines how percent change in particular vital rates is predicted to alter either population growth rate or the probability of extinction. Subtle changes in vital rates may expose thresholds that are not readily observable in a series of comparative scenarios where mean vital rates are used.

I would recommend that you also work with another modeling team who will develop a PVA "from scratch". [redacted] Drs. Bakker and Doak [redacted] (Bakker et al. 2009) [redacted] are very good at this sort of analysis and there are also two folks [redacted] Drs. Gebreselassie and Milligan, who have the expertise to program a context-specific Mexican wolf PVA. [redacted] Vortex [redacted] I am familiar with some of the limitations of this platform and have had an opportunity to compare its performance to a more "holistic" PVA (Bakker et al. 2009). I would consider such an alternative because the inferences that can be drawn are far richer.

Regarding redundancy, it was written in the Report (L1398-1400) that "To achieve *redundancy*, populations in these two geographic areas, at minimum, will need to demonstrate sufficient *resiliency* (as described above) such that they provide a true measure of security against extinction for one another." Although population estimates of extinction probability were generated and comparing these maybe sufficient, I wonder what the probability of extinction would be for the metapopulation, whether this might differ than just a product of the individual extinction probabilities for each population, and whether you could examine the probability of extinction of the main population, or any population, at a finer scale. That is, could the current distribution of wolf packs, their particular vital rates, and the "patches" they occupy be used to develop extinction and colonization probabilities for different occupied patches within the MWEPA. Like a classic Levins (1969) metapopulation model. If you think about it, ultimately extirpation of a population is due to the extinction of each pack, but healthy packs can produce dispersers that colonize other available patches previously occupied by a now extinct pack. Understanding such dynamics may prove insightful. Such an approach would be possible in a context-specific, species-tailored PVA.

With respect to representation the target of maintaining 90% of the genetic equivalents of the founding population, and doing so in a combined fashion with the captive population, is a worthy goal and an intriguing approach. I really liked how the various analyses considered both the wild and captive populations together. My only concern here would be to allow the Mexican wolf subspecies to breed with other wolf subspecies if such an occurrence were to happen. Such pairings most likely occurred in the past, and since the Mexican wolf is represented by so few founders, we really do not have an adequate understanding of the genetic variation that originally existed within this ecomorph prior to its decimation by humans.

Conclusions

Clearly there has been an enormous amount of fieldwork, analyses, and socioeconomic/legal action that has taken place over the tenure of the Mexican Wolf Recovery Program and these actions have led to growth in the reintroduced population. This document does a very good job of describing the program's history and current state, but I think that some additional approaches could be explored that may improve the inferences drawn from these data and thus improve the future of the program.

<u>Comments on Martinez-Meyer</u>

Report, L502-503: In the report, there was mention of denning pack home range size and non-denning pack home range size. Thus, data have been collected on radio-collared individuals so a resource selection model could be developed. Shouldn't these data be used to assess/validate the habitat suitability maps generated by the species distribution modeling?

Alternatively, a study could be conducted where occupancy modeling is used to assess resource selection of wolves which is then used to compare to the SDMs (Kéry et al. 2013, MacKenzie et al. 2006:33-35).

Finally, some very smart folks have questioned the "believability" of SDMs (Yackulic et al. 2013), and considering that the analysis in Martinez-Meyer et al. (2017) was based on 41 primary occurrence data points, the results may want to be viewed with caution and at the very least validated with an alternative dataset.

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Mexican wolf Draft Biological Report 5/1/2017

Review by [redacted] PhD, [redacted]

General comments on draft Report:

1. Does the draft report provide an adequate review and analysis of the factors relating to the persistence of the Mexican wolf population in the Southwest and Mexico in terms of demographics, habitat, disease and predation, human-caused mortality and genetics?

The draft report provides an excellent review of the legal, taxonomic, management, and demographic history of the Mexican wolf. I appreciated the level of detail given to the particularly contentious issues (whether current or past), such as taxonomic classification, and how these have either been resolved in the literature, or at least how conclusions were made with respect to this report. It is also worth noting the value of the white paper developed by the Mexican Wolf Tribal Working Group. Clearly, having a better understanding of concerns and priorities of the diversity of sovereign Indian nations is a key element in long term persistence of wolf populations in the southwest.

2. Are our assumptions and conclusions regarding current population trends and stressors logical and adequate?

The history of the Mexican wolf, along with other subspecies of wolves and carnivore species in general, makes it somewhat straightforward to identify and rank threats to recovery efforts. This is not to say recovery itself is an easy task. Further, the intensive efforts to date in terms of reintroductions and monitoring population status leaves little room for ambiguity on the success of those efforts. Despite the great efforts required to recover the species, the report provides compelling information on status and trends of the populations, as well as the primary drivers (ie, stressors) to those populations.

3. Does the report adequately consider what the species needs to maintain viability in terms of resiliency, redundancy, and representation?

Particularly given the inherent limitations to genetic diversity, likelihood of recurring conflicts with humans, limited numbers of animals available for release, and uncertainties about habitat availability (ie, prey base), the report does a good job of addressing these three factors. With specific regard to resiliency, I provide comments and questions regarding the PVA below.

For redundancy, clearly the ongoing efforts to establish population(s) in the SMOCC is critical to meeting the minimum recommendations, although the degree to which these can be considered independent is difficult to determine. Despite the recognized differences between these regions (e.g., elk only in MWEPA), the desired condition of connectivity among these populations suggests that they will not be truly independent, and certain stochastic events could dramatically impact any combination of them (e.g., regional droughts, disease outbreaks). I would suggest noting that the SSP will remain a robust source of new animals for release, which in effect is something of an independent population, despite its captive nature.

As noted in the report, representation here is both a genetic and ecological concern. The former is limited both by the available genetic diversity from the founding animals, and can only be increased through mutations. Further, once in the wild, breeding is not regulated, which may result in less than optimal pairings of individuals, the repercussions of which are essentially impossible to incorporate into models. To the extent practicable, the report specifies adequate details on how to maintain this component of representation, while addressing the limitations.

Ecologically, the populations north versus south of the U.S.-Mexico border provide a considerably larger range of conditions than either population alone. Given the historic range and currently available habitat based on modeling, the report provides adequate consideration of ecological representation as well as spelling out what is thought to be required for its long term persistence.

Specific comments:

- Line 442: I appreciate the pragmatic conclusions of Heffelfinger et al. (2017), and agree with the report to use the area defined by Parsons (1996).
- Line 522: Was removal rule for dispersers (1998-2014, beyond Gila and Apache NFs) addressed in PVA analyses? If the rule is no longer in effect, dispersal success should be higher than the 55% reported through 2015.
- Line 561: just to be nitpicky, these elevations should be reported in meters as well (same with Lines 586, 590). In general, I think the report does a very good job of explaining how landforms affect conditions relevant to wolf ecology, which further supports how the recovery areas have been defined. Conversely, it would help to note how contiguous forested areas (etc) are in addition to percentage of landcover types.
- Line 603: How important are these riparian areas for wolves?
- Line 606: It might be worth noting more about the protections afforded to wolves on state and tribal lands, at least generally.
- Line 617: Have 'suitable' versus 'high quality' habitats been defined yet? It might be worth summarizing the results of Martinez-Meyer et al. (2017) more here.
- Line 644: How large are these protected areas, and what does 'protected' mean here? I know it's discussed more on page 37, but even there the actual level of protection (or enforcement) isn't clear.
- Line 708: Is this rate similar to what is reported for the MWEPA population?
- Line 764: I believe this is the first time scavenging (other than diversionary feeding) is discussed. Could the significance of scavenging change over time or vary by area? A bit more detail would be nice given comments about its significance related to hunting seasons. Also, are you saying that the kg/wolf/day was overestimated, or just the specific impacts on prey species? The high intake rate relative to gray wolves is surprising given the differences in body size.
- Line 808: This may be beyond the scope of the report or recovery efforts, but in the context of adaptive management, I would think it worthwhile to monitor such ecological responses (e.g., trophic cascades) to better inform future augmentation efforts as well as adding to the general understanding of how systems respond to the return of large predators.
- Line 1072: The significance of the founding lines is not very clear. Beyond generally maintaining genetic diversity, what is the reason to develop targets for representation in wild populations?
- Line 1085: Are there plans for additional cross-fostering of pups? Was the successful example the wild to wild litter?
- Line 1091: How are hybridizations detected? I think it would be worthwhile to provide details on what future monitoring efforts for detecting hybrids will look like.
- Line 1108: Is there a trigger point when gray wolves would be used to augment the population for genetic rescue effects? Of course this is a contentious topic, but was key for Florida panther recovery efforts (although I know some authors argue it wasn't, and it was later determined that the translocated animals belonged to the same subspecies). Nonetheless, I'd like to see more detail than just noting that 'careful evaluation of potential effects…is needed'.
- Line 1262: The role of multiple, connected populations is an important buffer against extirpation as well.

- Line 1294: Clearly the role of inbreeding can't be ignored, but extinction is ultimately a demographic process. Even with increasing inbreeding, population growth must be the target.
- Line 1315: Not sure that Doak et al. 2015 makes a statement about 90% persistence over 100 years being a defensible target. They explicitly tried to avoid making specific statements, and instead provided examples.

- Clearly, human-caused mortality is among the most important issues to address (70% of known mortalities 1998-2016). Although there is little reason to expect this rate to improve as wolf numbers increase (as will human densities and access in many areas), other factors are likely to increase as well.

Comments about PVA:

- Line 190: Based on field observations, 78% of wild adult females in a given year are expected to be paired with an adult male. This was based on the average values from two different methods of estimating pairing rates. Was there a sensitivity analysis done for this? Maybe consider using the most conservative value? It appears to have fewer potential biases, which may in fact cancel out. It would be reasonable to expect this proportion to drop as pack density increases. What is known about this from other populations (recognizing that we can't expect them to perform identically)?
- Line 205: I trust the data here, but I'm curious as to why kinship of breeding individuals would affect the probability of litter production, but not litter size. Is this something of a threshold effect?
- Line 265: Why do you have a density dependence effect for adult mortality (Line 300), but not with reproduction? Couldn't the opposite effect be more plausible, given the relatively high level of adult survival and (to-date) limited intraspecific mortality? Further, as competition with other species increases, unless supplemental feeding is increased, I would expect reproductive rates to decline. I know interspecific competition is not considered an issue here, but I personally think this is a potentially important factor that is often overlooked.
- Line 276: Assume equal survival rates for males and females. Is this supported? Even Appendix D gives only results from the sexes combined. For nearly all large mammals, males are expected to have higher mortality rates than females, as has been found with wild wolves elsewhere (e.g., Smith et al. 2010).
    - This question applies to how 'surplus' animals are removed (ie, assuming equal mortality rates across sexes).
- Line 296: Where do these other adult mortality rates come from?
- Line 318: Was any effect of subsequent outbreaks considered? Given the moderate connectivity between MWEPA and SMOCC, this could be an issue (Almberg et al 2010). Also, I think it would be prudent to consider a more frequent interval of outbreaks given there are large human population centers within the MWEPA that likely support much larger numbers of other hosts (e.g., domestic dogs).
- Line 327: Estimates of *K* are rarely, if ever, reliable. Given the lack of data on prey densities in Mexico (and the likely overly optimistic opinion of estimates in the U.S.), it's tough to put any real faith in such estimates. Given how dramatically lower the management targets are than estimates of *K*, however, there is probably little concern here.
- Line 350: Why are equal numbers of pups and adults "harvested" to reach the target abundance? Why not either specify that a particular age class would be targeted for removal (as I assume would be the case in reality), or use the age specific mortality rates reported?
- Line 401: In the interest of transparency, I'd suggest reporting the dispersal rates in absolute terms, for example 1 successful disperser per 100 wolves every 5 years between X and Y populations.

- Line 407: Given the extensive history and knowledge of the captive breeding program, I defer to the authors here.
- Line 624: Is this saying that the mean survival rate for captive and translocated animals is used, despite the substantial difference is adult survival for these groups? Either I'm interpreting that incorrectly, or there should be some justification. Given the example (ie, 4*0.284, which is the captive adult rate), I assume I'm misreading something. Also, is it reasonable to expect that this number won't go up or down with time?
- Table 3: Personally, I would like to see the samples sizes reported within the table.
- Line 1314: Is it feasible to use animals to be removed from MWEPA (to maintain the management target) be used to augment SMOCC populations? Perhaps I missed that detail, but that could help balance the effects of loss on MWEPA with increasing the southern populations (ie, limiting an additive effect of translocations).
- Line 1912: Are there recommendations for how to reduce the number of cryptic mortalities?

New PVA (addendum):

- The dramatic improvements in retaining genetic diversity and bolstering demographic performance in wild populations with more aggressive releases from the SSP (with little predicted effects on the SSP) strongly support taking this approach. Perhaps an analysis to identify the optimal release schedule with the objective of maximizing releases while maintaining predetermined conditions with the SSP would be more useful than focusing strictly on the responses of wild populations.

Comments on habitat model:

General comments:

As is noted repeatedly within the report, the number of assumptions, potential biases, lack of data, and reliance on information from other populations makes it difficult to place a great deal of faith in these results. General patterns may hold true, but the objective was defensible estimates of *K* for these recovery areas. I think the authors did a good job in not attempting to oversell results, however, which is difficult to do when so much effort has been put into a project. As I am not an expert in some of the analytical frameworks used, I primarily focused on questions that apply to many methods, such as explaining how scale and resolution were determined, and clarifying certain points. I think these analyses lay the groundwork for important future efforts as more data on wolf habitat use and prey densities for these areas become available.

Specific comments:

- Line 241: suitable and high quality may not be the same thing. Habitat is a word that is often misused or at least misinterpreted, so keeping with strict definitions may help readers keep the distinctions straight.
- Line 259: I'm not sure that predators or pathogens would be viewed as requirements, at least not in the eyes of the focal species.
- Line 305: where did this value (25km) for filtering come from? Regardless, more detail here would help the reader interpret your methods.
- Line 327: how what this resolution selected? Was it for consistency across datasets, or some ecologically meaningful size? Again, more information on how values, scales, and resolutions were chosen would be helpful.

- Table 1: what was the time period from which these climatic variables were calculated? Are they historical to align with records of wolf presence? Multi-year averages? How well do they correspond to current conditions (if historical), or if not, how well to contemporary values correspond to when wolf records were made?
- Table 3: there is not nearly enough information provided here for readers to assess the meaning of these results.
- Line 451: given the mountainous terrain, I'm not sure how to interpret such broad results. Clearly there is a relationship between elevation and landcover in many area, but it isn't clear how your results reflect that, at least not with such broad generalizations as are reported.
- Line 503: what kind of vegetation could present a barrier to wolf movement? Some landcover types that are considered "matrix" actually present no barrier to movement, leading to increased permeability because the animals pass directly through them.
- Line 516: with GPS collar data, would it be possible to perform an RSF analysis and take advantage of the rich dataset instead of subsampling to make it fit another framework?
- Figure 7: the title doesn't appear to match the figure (i.e., landcover suitability vs. land cover map). I also suggest making these figures and captions/titles such that they are stand-alone.
- Line 650: would it be possible (or did you) validate the composite roads map? There's really no sense of accuracy, despite this being an important part of the model.
- Line 682: why not just use the lower value to be conservative?
- Line 734: It really sounds like the UBI modeling can't be very reliable given the biases and assumptions of the estimates of prey density. Is it possible to "validate" the estimates in any cases? This seems like a pretty big issue. If not, is it worth including it?
- Line 781: Is the code available?
- Line 803: wouldn't this be the ungulate biomass available to hunters, cougars, coyotes, and bears as well?
- Figure 15-17: the units in the legend should either be defined or put in relative terms.
- Table 10: It would be interesting to see how much high-quality area is in large, contiguous blocks (eg, $>\underline{1}$ average home range size) compared to the total.
- Line 1032: detail, but it says that the largest patch was in AZ-NM, but the one in Durango is listed as being larger.
- Line 1033: Is 1,500 km2 significant (e.g., $\underline{\geq}5$ average home ranges)? Putting such thresholds or bars in context would be helpful to interpret their ecological meaning.
- Line 1083: what is the objective for identifying municipalities with high-quality wolf habitat? To most readers, this won't mean much without a defined objective, such as identifying which administrators to contact. Its relevance to recovery or identifying wolf habitat isn't clear.

The Fish and Wildlife Service created an informational packet of the following materials related to the Draft Mexican Wolf Recovery Plan, First Revision. We have broken the packet into smaller sections to allow for easier readability.

The contents of the Packet are as follows:

- Draft Biological Report for the Mexican Wolf, May 1, 2017 version
- Population Viability Analysis for the Mexican Wolf (05/01/17) and Addendum (05/22/17)
- Mexican Wolf Habitat Suitability Analysis in Historical Range in Southwestern US and Mexico, April 2017 version
- 5 peer reviews received on the above documents

The U.S. Fish and Wildlife Service provided the above versions of the Draft Biological Report and two supporting analyses, "Population Viability Analysis for the Mexican Wolf" and "Mexican Wolf Habitat Suitability Analysis in Historical Range in Southwestern US and Mexico", followed by an addendum to the population viability analysis, for peer review from May 2, 2017 to June 2, 2017. Five peer reviewers provided comments to the Service through an independent contractor, Environmental Management and Planning Solutions, Inc.

FWS is providing this packet as supplemental background information to the public during the public comment period for the Draft Mexican Wolf Recovery Plan, First Revision. Peer reviews are anonymous at this time but FWS will provide peer reviewers names and affiliations when the recovery plan and biological report have been finalized.