

Minnesota DNR Review of Draft Lynx SSA

We appreciate the opportunity to review the draft Lynx SSA and hope our comments are useful to the review process. We do have a variety of concerns and suggestions, some editorial and some substantive. We recognize many of the comments are critical of a variety of statements and conclusions, but do wish to stress that our intent is to improve the scientific credibility of this document and any conclusions that may arise from it. We recognize the tremendous effort that went in to preparing this and the difficulties in conducting such work in the face of much uncertainty, regulatory frameworks, and time limitations. We commend the effort even though we may disagree with many conclusions. We also realize that some of our structural or organizational suggestions may not be consistent with your current SSA process guidelines/rules, but nevertheless chose to offer those concerns and suggestions herein.

We will start with some overarching thoughts as well as concerns on a few mechanistic ideas that are woven throughout nearly all areas of the document.

- 1) We believe this document could be reduced in length by 50% or more by reorganizing it and reducing speculation and redundancies. There are so many points in the document, many speculative, which are repeated dozens of times that it detracts substantially from the usefulness and readability. If there are points that are repeated so often, then in our opinion that point should become a heading with 1 clear and concise discussion of why it's relevant, and all supporting literature. An example is the argument about bobcats increasing and outcompeting lynx. Later we will question the merit of this idea some, but this idea is mentioned MANY times in various sections. If it is believed to be so relevant, then it needs to be a focal section, thoroughly critiqued in 1 spot, and then dropped. We would also add that we even question the need for Chapter 2. We don't see this as a broad literature review document for lynx or hares. It should have a much more targeted focus on current status and projected changes, reporting only literature that is directly related to any proposed cause-effect process you deemed to be of relevance to future changes. And importantly, these literature sources should be thoroughly critiqued, not just reported. We recall few instances of actual questioning of the merits of any study, unless there was already another citable article published that challenged it. Every study should be independently critiqued if it is a study used as the mechanistic basis for some proposed future change.
- 2) We question why climate change (or Vegetation Management, Wildland Fire, etc) needs to be a specific section in this document. It leads to a much broader discussion of these topics than necessary, lengthens the document noticeably, and we would argue it detracts from what should be a more complete and mechanistic discussion. To be fair, under many of these sections you do use sub-headings focused more on the mechanistic relevance to lynx (e.g., hare habitat). But forcing these ideas into, for example, a Climate Change section leads to too much superfluous general climate change discussion, in our opinion leads to a tendency to 'force' a climate explanation on every observation at the expense of other possible hypotheses, too often separates potential positive and negative impacts for any category, and leads to far too much conjecture. We question the usefulness of any paragraph filled with "might", "could potentially", "is thought to", "he/she speculated that", "may have effects", etc. We certainly understand the document needs to consider threats and must involve *some* speculation, but speculation should not be pervasive, and it also needs to consider possible positive or mitigating

aspects of “change” that could offset threats and include all ‘reasonable’ hypotheses for an observation besides just a climate explanation, etc. And for each topic, this balanced assessment needs to be in 1 place only. For example, a focus on “Changes to Hare Habitat” should be a main section heading that includes relevance of climate change, vegetation management, and human encroachment (as opposed to these being in separate discussions).

- 3) To summarize much of the above, we would suggest that the non-process-oriented portion of this document should start with what is now Chapter 4 – provide the best available information on ‘where we are’ today, and how it compares to the past. Then a shorter chapter to explain how you reviewed the literature, laying out what you or others conclude are the key factors that are relevant to the near future (e.g., hare abundance/distribution, lynx denning habitat?, human-caused lynx mortality, connectivity of populations, competitors, etc). List only the strongest citations for each of those mechanisms so others can determine if they agree on the merits of the study. Then the last chapter tackles potential changes (positive and negative) for each of those ‘change mechanisms’ (e.g., hare habitat, etc.) in succession, including anything related to climate, development, or veg management in the same section. Projections should focus only on perhaps the next 20-30 years (see our next comment), and be based largely/only on specific attempts that have been made to quantify/map projected changes (not just purely speculative “could effect”, “might happen”, statements). Each sub-heading in this category should end with a final ‘net conclusion’ for this variable. This will then further allow a critique by others on the ‘trustworthiness’ of the projections.
- 4) We’re sure we are paraphrasing here (or maybe you would just disagree), but we would summarize your conclusions like this: lynx distribution/numbers now may not be so different than historically, with perhaps some lynx reductions in places, but possibly some increases (Maine). But the 3 R’s are pretty good at this time. In the near future (next 20-40) years, things may not change that much, but possibly some reductions in some units. But things look bad further out (by turn of century), largely (but not solely) a result of climate change. We do not know how far out you are REQUIRED to consider, but regardless of whether this scenario proves accurate or not in the future, we would argue that ANY prediction this far out should not be considered trustworthy. The uncertainty here is enormous, and we do not feel it is often properly acknowledged, and in fact sometimes implied, with your word choices, not to exist. We’re not cynical of climate change, but very skeptical of our ability to predict the actual future for specific species in specific areas. There is uncertainty in the climate scenarios, uncertainty in our knowledge of the relevant biological mechanisms, and it fully ignores (or at least can’t know) other non-climate changes (bad or good) that could occur (adaptation, mitigation, economic forces that affect habitat and populations, global politics, etc.). With all this uncertainty, we disagree that a Species Status Assessment with many potential implications should have so much speculative leeway that far out in time. If that much speculation is allowed, one could easily construct a positive or neutral scenario that far out, at least in terms of the climate angle (e.g., see the book “Landscapes and Cycles” by Jim Steele – not that our mention of this is an endorsement of any specific idea he presents).
- 5) Smaller point, but the Literature needs to be cleaned up. We did not even cross-check a majority of the citations, but it was not uncommon when we did to find some error (e.g., year mismatch) or omission (a listed citation not in the Lit Cited).

Because some of the future threats you identified hinge on mechanisms you suggest will change as a result of climate change, and they are repeated in many places, we will focus our next set of comments on some of those ideas and where we either disagree or at least feel the idea is not sufficiently critiqued.

Lynx need deep/fluffy snow or bobcats can't catch hares in it

- Certainly no disagreement from us that lynx have adaptations for deep snow, but this is not an argument that they NEED or REQUIRE it as often stated or implied. Also safe to say there is a good CORRELATION between lynx distribution and deeper/fluffier snow. But:
 - We are unaware of any study that rigorously disentangles the correlation between lynx, snow, boreal forest, roads/humans, and hares. They clearly need *hares*, which appear to do best in boreal-like forest, which is found where there is deep/fluffy snow and fewer roads/humans. Lynx obviously can catch enough hares during the 7 or so snow-free months of the year to clearly demonstrate they don't need snow for that purpose.
 - Even in northern areas with 'great' snow conditions, lynx still 'crash', suggesting that snow per se is not the driver of the decline (even if it may influence synchrony or lynx ecology).
 - On p. 61, you mention Stenseth et al. (2004) saying only that they estimated that snow density affects lynx hunting success – no details. It is not until 6 pages later (p. 67) that you add the note that they estimated that lynx hunting efficiency for hares may be HIGHER in compacted snow than fluffy snow (if nothing else, this is another example of why combining points into more focused sections is needed). Our main question, however, is why you take a result that could be considered a positive (or at least mitigating) 'response' to the supposed negative effects of climate-induced increases in snow density, and immediately try and assume a negative effect. Specifically, on P. 67 you state that this higher hunting efficiency by lynx on compacted snow may cause a (positive) numerical response by lynx, but you quickly follow with the assertion that this could actually be bad (drive hare population to low levels), citing Stenseth et al. (2004, 10633). I found no such suggestion in that paper, and more importantly, it would be speculation only and ignore the fact that for hundreds of years lynx (along with other variables) have already been driving hares to low numbers (i.e., the hare cycle) without negatively affecting long-term hare persistence. More compaction is bad because lynx will eat too many hares, and by reverse inference, if snow got increasingly deep/fluffy they wouldn't be able to catch enough. So any change from right now is bad???
 - A possible response to this concern is that maybe it's *hares* that need deep/fluffy snow, and not lynx per se. We will discuss the color mis-match idea next, but outside of *maybe* that we find little data to suggest hares do NEED a specific depth/type of snow. Once again, we obviously know hares have persisted in spite of 7-8 months of snow-free conditions. Even in the boreal forest, hares experience high mortality from a wide variety of mammalian and avian predators, yet they have evolved to 'deal with this' and persist where habitat is good. It would appear that the strongest case to be made is that cover type (e.g., spruce/fir or similar coniferous/mixed) and horizontal complexity is the primary driver behind their distribution, not snow or presence of only a certain number (or species) of predators.

- The color mis-match idea is certainly an interesting one that does relate directly to snow. And there is now some data suggesting differential survival of hares based on color mis-match (presumably this would have been the finding 100 years ago as well, since snow has always been variable in timing and not all hares turn white on November 1st). But to go beyond that and suggest long-term population consequences is premature, and we would argue not very intuitive. The fact that there is wide individual variability in the timing of pelage change (p. 68), combined with hares being an r-selected species (i.e., high reproduction and well adapted to deal with fluctuating environments), suggests to us that there is a high likelihood that timing of coat color change will evolve if snow conditions change. At the southern edge of their range, snow conditions have likely fluctuated for eons. Ignoring that for the moment, we would say that the way to describe the Zimova et al. (2016) conclusion on p. 68 would be that IF there is no selection operating on the timing of molt, and IF there is no compensatory reproduction/mortality in response, and IF the high-emissions scenario happens, then in maybe 80 years or so hare population growth might drop below 1.0 (they do not estimate it would drop below 1.0 in the moderate emissions scenario as you state). Is this really something to base management decisions on? Even in the absence of climate change, one can come up with theoretically-possible 'disasters' for any wildlife population/species, but it doesn't mean they have much applied value now.
- This point relates both to the above discussion as well as the competitive exclusion idea we discuss next. But on p. 66, with similar statements scattered in dozens of places, you state that “Bobcats.....are unable to hunt hares successfully in areas with deep/soft snow”. You list 2 citations, and we would say that neither contains any data (nor cites any) demonstrating variable hare hunting success by bobcats based on snow conditions. We already know that in notable parts of lynx range, coyotes and lynx are coarsely sympatric and both species prey extensively on hares. In examining Figure 5.1 of Krohn et al. (2005), it would appear to us that if the foot loading – leg length arguments were drivers here, bobcats should be more equipped to pursue hares in deep snow than coyotes – they have similar leg length, but lower foot loading. We see little data to support the conclusion that snow, at least via its effect on hare hunting success, is solely or even largely responsible for the mostly allopatric distributions of lynx and bobcat.
 - In this same sentence, you state that bobcats “....experience high mortality in deep snow winters (Litvaitis et al. 1986, p.116)”. This citation is not in the Lit Cited, but we think we know what it is and on p. 116 there is no such statement. They do mention that snow may affect mobility of bobcats, and one of the citations they list (Petraborg and Gunvalson 1962) does anecdotally suggest bobcats can become stressed during severe winters, but that much of the actual mortality was attributable to vulnerability to human-caused mortality in those situations. And even then, no evidence that bobcat populations were then or shortly thereafter excluded from those areas. So unless we missed something, we don't see much data to support the claim that bobcats experience high natural mortality directly from deep snow.
- We're speculating more now (but see p. 69 in Werdelin. 1981., Ann. Zool Fennica 18:37-71), but while bobcats are certainly less-adapted to deep/fluffy snow than lynx, we

believe reduced prey diversity in northern areas may be more limiting to bobcats than snow directly. From our limited understanding, Eurasian Lynx are the precursor to both species, and the first wave of arrivals to NA came south, glaciers eventually restricted them from the north, and they evolved into bobcats in an area with more diverse prey (and less snow). The second wave of Eurasian Lynx immigrants arrived in the north after the glaciers, and already snow-adapted, but lacking a similar-sized prey than they were accustomed to (roe deer). Hares were the most abundant and closest-sized option, so Canadian lynx evolved as 'hare-addicts' in an area with lower prey diversity (and more snow, which they were already well-adapted to). They didn't later expand further south because there were no hares there, and the nearest niches to the south were already filled (bobcats, etc). Our point here is that if anything occurs to cause hare habitat to contract northward (for any reason, including climate change), we agree that it is likely to impact southern lynx. But we see little data to support the notion that snow will change and all of a sudden allow bobcats to move in and either kill lynx or outcompete them for hares. Kapfer (2012) concluded that snow and temperature did not appear to be directly limiting bobcat distribution in MN, and found more support for deer density limiting the northern edge, and others have also recognized the likely importance of ungulates to northern bobcats, especially in more severe winters (see discussion in Anderson and Lovallo 2003). So while snow could still be a relevant indirect variable (if changes allow deer increase), we would argue that whether bobcats advance northward in meaningful numbers will be as or more dependent on whether non-hare prey diversity/density increases in those areas than on snow conditions directly. As such, and with no clear data to suggest bobcats will kill lots of lynx, we currently doubt the merits of the lynx-hare-bobcat competition argument. We recognize the largely speculative nature of our own comments here, but as we have argued above, we believe these ideas are no less supported by data than some of the existing arguments in the document.

Competitive exclusion

- Some of our above points are relevant here as well, but a few additional notes. The idea of outcompeting or excluding is based on the premise of overlap, at least initially. If lynx range were to contract (say because hare habitat contracts), and bobcat expand into areas lynx USED to be, then they remain allopatric and competition or exclusion is not relevant. So in using these terms, it assumes bobcat/coyotes/etc invade lynx-occupied areas first, kill or outcompete them for hares, thereby excluding or notably reducing lynx from areas they would otherwise have remained. To this idea:
 - The primary study commonly pointed to for bobcats excluding lynx is Parker (1983) on Cape Breton Island. In that paper the authors do not provide any data demonstrating that bobcats excluded lynx from the lowlands, they just point to an apparent correlation when in 1955 a causeway was built, bobcats apparently crossed, and lynx were eventually found primarily in the highlands. But there is no data provided to clearly demonstrate lynx were present in notable numbers in the lowlands prior to that, or what the actual mechanism may be (did bobcats kill lynx, or eat all the hares?). Additionally, a casual review of the history of the island notes that the mining and steel

industry blossomed after 1900, and that after WWII (when the causeway was built) other industry and human development ensued. Presumably the causeway allowed more human disturbances, and possibly more human-caused lynx mortality, starting in 1955. The authors state in their paper that “Whether the decline in lynx densities was coincidental with the dispersion of bobcats or a direct result of that phenomenon is uncertain”. And in a 2001 Nova Scotia Lynx Status Report, the same author reports that “there is no historical correlational evidence that either [bobcats or coyotes] has adversely affected lynx densities or range limits in the past 20-30 years”. Given the publication date, that would refer back to either 1970 or 1980, so it is not necessarily inconsistent with the earlier speculation (which referenced 1955 up until the 1983 article). But there is some inconsistency, and given their earlier conclusion of “cause uncertain”, it is not a particularly well-supported example to serve as the ‘poster child’.

- We could not retrieve the Robinson 2006 thesis, but the other citation on this point that you list (Peers et al. 2017) on p. 66, along with Murray and Boutin (1991) listed elsewhere, does provide some evidence of local niche separation with either bobcats or coyotes, but importantly these findings come from areas WHERE THESE SPECIES ARE SYMPATRIC. Presumably this is to be expected in that there must be at least some niche separation for 2 species to co-exist in the same general areas. These are important and useful studies, but they do not show any demographic effects on lynx nor imply “outcompete” or “exclude”, only some degree of coexistence through smaller-scale niche partitioning.
- So collectively, while this may be a reasonable idea to consider, we argue it currently has little solid data behind it. Yet you probably state or infer this “outcompete or exclude” concept dozens of times.

Are disturbances good or bad?

- Starting on p. 70 you discuss forest disturbance events. Perhaps because this is in the Climate Change section, itself a part of the Threats section, the ‘tone’ of this entire discussion in our opinion is negative. For example, there are a lot of terms like “dramatically affected”, “stressed”, “increase vulnerability to”, “extensively damaged”, etc. We’re not suggesting these are incorrect statements where used, but this section does not provide a balanced review of how disturbances can be good or bad for lynx or hares. There is just a theme of negativity because these disturbances may be driven by climate change. In only 1 place from p. 70-72 is there any hint that disturbance can be good for hares/lynx (as well demonstrated in Maine and elsewhere), yet this ‘good disturbance’ is quickly turned to a negative point by suggesting this particular example of disturbance may not happen again due to climate change. Ten pages or so later (Vegetation Management, Wildfire, etc sections), there is additional discussion of disturbances. We do feel many of those discussions are more balanced, but we strongly argue that these discussions all need to be together. As but 2 examples: 1) on p. 70 you say “Increased fire frequency.....could affect connectivity and gene flow in lynx populations”, which hints that the assumption is it will be a negative effect. Then on p. 84 you note that “Because of (1) fire’s important role in creating and maintaining high-quality early-successional hare habitat in most lynx habitats in the contiguous U.S.,.....”. These ideas need to be more concisely discussed together, examining net potential changes. 2) On p. 70 you note that “For example,

drought can weaken trees, increasing their vulnerability to insects and pathogens”, then on p. 71/2 “Widespread clearcutting following the most recent spruce budworm outbreak in Maine was the primary driver creating the current broad distribution of high-quality lynx habitat”, then on p. 79/80 “Removal of larger trees from mature multi-story forest stands to reduce competition and increase tree growth or resistance to forest insects may reduce the horizontal cover (e.g., boughs on snow), thus degrading the quality of winter habitat for lynx”. Individually, each of these statements may have some truth in selected situations, but it is not helpful or useful to have them scattered about. The potential negative AND positive consequences of them need to all be in one spot under more mechanistic sections, objectively balanced (even if they must be under a “Threats” section). Headings like “Future Changes to Hare Habitat” are more meaningful, where you combine positive and negative possibilities/data related to climate change (e.g., disturbances can both create hare habitat or have negative effects), forest management (some is good or could mitigate, some can be bad) , human encroachment/development (presumably not much good here), etc. And then each section can end with 1 forecast, even if “Too much uncertainty to make defensible predictions of the future”.

Cyclicity is not necessarily “good”

- There are several places in the document where you state or imply that cyclicity is inherently a good thing, or a change to non-cyclic is in itself bad. Examples include:
 - P. 34 - non-cyclic or weakly-cyclic hare populations are unlikely to allow the rapid lynx population recovery observed in northern lynx populations when hare numbers increase dramatically after cyclic population crashes
 - P. 65 – The collapse of cycles in herbivores with high-amplitude population cycles also would imply collapses of important ecosystem functions such as pulsed flows of resources and disturbances throughout the ecosystem, including declines in predator communities (Schmitz et al. 2003, p. 1202; Ims et al. 2008, p. 85).
 - P. 65 – If diminished amplitude of the hare cycle in Canada persists, it will likely translate into a reduced potential for lynx to expand into new or unoccupied habitat in Canada or the adjoining U.S. (ILBT 2013, p. 69).
- While change from cyclic to non-cyclic (or pulsed to non-pulsed) is certainly a sign that something is changing, and possibly an indicator of an emerging concern, we do not find these arguments compelling at all. Average population density will be higher for ‘stable’ populations than fluctuating populations (e.g., Boyce and Daley. 1980. Am. Nat. 115:480-491.), all other things equal. This implies lower persistence for fluctuating populations (e.g., Inchausti and Halley. 2003. J. Anim. Ecol. 72:899-908.), again all other things equal. So cyclic behavior can’t be considered inherently good (in fact, it can be considered bad) and these statements should be removed in our opinion.
- Diminished amplitude does not necessarily mean there will be less dispersers on average, only that dispersal will be less pulsed. It has flaws for the same reasons above. Sending out 0 dispersers for a number of years, followed by 100 for a few, cannot automatically be viewed as better than 50 every year (or from above principle, maybe it would be more than the average for a fluctuating population). And since this idea you reported rests on the assumption of climate change induced alterations to snowpack and cyclicity, we would note that there is a

logical but perhaps speculative argument to be made that more compacted snow could increase lynx dispersal distances and have positive effects on colonization of patches.....all other things equal.

Hare Range/Density contraction

- On p. 68, you state that hare range is contracting “....because of changing snow conditions and reduced survival because of delayed pelage changes (Diefenbach et al. 2016, p. 245; Sultaire et al. 2016a, entire). Shortly thereafter, “Loss of snow now contributes more than loss of habitat in determining the range of snowshoe hares in the Great Lakes region (Sultaire et al. 2016a, entire). We do not believe these are currently defensible statements.
 - While we won’t question the conclusion of a possible range contraction too much, we would note that comparisons of finer-scale species presence at 2 distant points in time (each based on 1 or 2 years of presence data) is less than desirable for detecting a range contraction. Especially at the edge of a species range, it is highly likely that this has always been a fluctuating boundary, so it takes more continuous time series data to truly assess a systematic range contraction. Even if we assume those range contractions in PA and WI are accurate:
 - neither study contains any direct data whatsoever to support the notion that it is due to reduced survival because of pelage mis-match as you state.
 - In another paper you cite later (Sultaire et al. 2016b), where they included more detailed vegetative metrics in their analysis, they state, for example, “As we predicted, landscape-scale forest amount and local vegetative cover were 2 important constraints of the snowshoe hare range limit”. Vegetative metrics constituted 3 of the 5 variables (the other 2 being snow-related) in their top model, all 12 of the top models contained vegetation metrics, and no snow-only model was even in the top 12. So even if snow is relevant, clearly so is vegetation and it is not mentioned.
 - Neither area of apparent hare range contraction in those 2 studies seems immediately relevant to lynx (you concluded that resident lynx did not historically nor do currently occur in Wisconsin, and I’m sure this would be true for PA as well). Range contraction anywhere may still be a relevant observation, but we would argue these observations are not very relevant to lynx at the moment.
 - In none of the discussion in this section (or those initial 2 papers) do we find any consideration of non-snow alternative hypotheses, outside of some discussion about predators in one of the WI papers. This is perplexing in that there are other hypotheses that seem just as reasonable as snow, in our opinion. For example, WI and PA are 2 states with the highest deer densities, we know deer increased dramatically in many areas from the 1970’s to present, and there is extensive literature (including some from PA and WI) on the effects of deer browsing on understory (i.e., important hare habitat), and past research to support various vegetation connections in the demography of a hare cycle. In WI, the area where hare range is suggested to have declined is also quite correlated (based on our visual exam) with the area of WI that has the highest

deer densities. And the second Sultaire paper we mentioned above clearly found vegetation a relevant explanatory variable. Other unconsidered and speculative but reasonable hypotheses: 1) increasing data (e.g., several MN studies) showing the impacts of northern expansion of exotic earthworms on forest understory (hare habitat); 2) in PA, there has been notable recolonization/expansion of both fishers and bobcats which could play a role in hare dynamics; 3) related to #2, we're not aware of anyone considering the idea that as a result of widespread predator reductions that likely had lingering effects all the way through the 1970's, perhaps hares had expanded into areas of otherwise marginal habitat, and now some contraction could arise in part from natural recolonization of native predators. While we really don't want to suggest the review needs more speculation, we do believe that your discussion here is not supported and should simply say that "There is some evidence that range may be contracting (so far in areas not too relevant to lynx), but that the cause-effect connections are unknown and could include snow, deer, predators, fragmentation, etc., etc.

- Finally, we would note that while we would not consider our data well-suited to examine hare range contraction in MN, data from 2 separate surveys here at least does not suggest any 'lynx-relevant' contraction of hare range, and more importantly, both surveys suggest hare numbers have been increasing for nearly 20 years in much of northern MN, completely contrary to many of the mechanistic suggestions presented in this review (e.g., snow is supposedly getting 'worse' for hares, bobcats have increased significantly, etc). The most parsimonious albeit speculative explanation for this in MN is a notable increase in young forest.

We will now list our remaining comments by page numbers:

- 6) Page 8 – assumption that lynx require deep-snow. As stated above, we do not find much data to support the idea that they require specific snow conditions. We do not believe one can say much beyond they require hares, and thus hare habitat/populations should be a main focus here. The rest is speculative.
- 7) Page 8 – assume hares have limited capacity to respond to disturbances. The Maine (and probably MN) story shows otherwise, even if the disturbances weren't climate-change induced. And for an r-selected species, it is not intuitive that they can't respond to disturbances. In fact, their demographic traits (other than maybe dispersal distance) are finely honed specifically to be able to rapidly respond to changing conditions. What matters is knowing any thresholds for when the type, size, or frequency of the disturbance is too much, and I'm not sure we know that. And we certainly can't predict the exact magnitude of disturbances well into the future.
- 8) Page 8 – assume changes to Federal Land Management Plans have been positive for lynx, and will continue to be so. While perhaps reasonable, it clearly is an assumption of unknown significance. Is there any specific study that has attempted to quantify hare/lynx response to changes in Federal land management plans?
- 9) P. 8 – projections to year 2100. We know we're reiterating now, and do it again later, but.....we would not personally trust any projections much more than 10-20 years out, even if our

speculative bet was in agreement with that in this document. With thousands of modelers/analysts and millions of monitoring dollars, few if any predicted the financial collapse even 1 year out. This is not a realistic time frame given the massive amount of uncertainty here, even just in the biological mechanisms.

- 10) P. 10 – “The western geographic units (units 3 through 6) may be more likely to support resident lynx longer than units 1 and 2 under projected climate change scenarios given the higher percentage of land managed specifically for lynx conservation and their greater topographic potential to facilitate the upward elevational shift in in lynx habitats projected by climate models”. At least in the context of regulation/ownership, this is a big assumption. It assumes a cause-effect with Federal regulations, and fully ignores non-regulatory factors in Units 1 and 2 that may have even done more for lynx on the private/state/county lands – e.g., disturbance/logging that may have created more favorable habitat in these Units in the past 2-3 decades. Just because something was not done in the name of lynx conservation doesn’t mean it isn’t beneficial to lynx.
- 11) Page 11 – Resiliency section – you acknowledge much uncertainty, then go on to say AS snow conditions become less favorable, bobcats LIKELY will outcompete/displace lynx, and this in turn WILL reduce lynx abundance. We know how hard this would be to do, but just to make our point, can you assign any probability of these things occurring in the face of all the uncertainty? We presume not, and we question some of these ASSUMPTIONS anyway. Just say “Future effects cannot be predicted with confidence”.
- 12) Page 20 – second full paragraph, first 2 sentences – “Additionally,....”. We think this is a very accurate and informative statement, yet the document then proceeds thereafter to make many assumptions and use leading words (will, require, likely to, etc), largely ignoring (or using citations that ignore) the vast amount of uncertainty on many mechanisms. As stated earlier, we think this could be avoided if you were to limit your forecasting to a period of time that one can put some faith in the projections.
- 13) P. 26 – “.....and the amount of structure (e.g., downed, large, woody debris, tip-up mounds) seems to be more important than the age of the forest stand for lynx denning habitat”. An important caveat is that you won’t get a lot of COARSE woody debris, including large tip-up mounds, if a certain percentage of the forest was not allowed to attain older age. So age does still matter, at least based on what the literature has found for lynx denning habitat.
- 14) Last sentence on p. 29, continuing to p. 30 – “These factors probably further reduce the likelihood that an individual lynx in the southern periphery of the range will survive, reproduce successfully, and have one or more offspring recruited into the resident breeding population”. We would agree, but to some extent these things are ‘normal’ for a species at the edge of their range, we can’t really quantify “reduce the likelihood”, and from your own conclusions it appears that current lynx distribution is not much different today than historically. So is this really meaningful?
- 15) P. 31, last paragraph – we find little in this paragraph that is anything but speculation. The most defensible statement is “....the influence of predation (and we would add, hare competitors) on lynx populations is unknown”. What more really needs to be said? We have already questioned the merits of several of the statements/assumptions in this paragraph, to which we would now add that Gonzalez et al. (2007) does not demonstrate lynx NEED snow for 4 months – all they did was look for correlation with snow - no assessment of how hares factor in,

whether snow is correlated with hares or hare habitat, no lynx/hare survival experiment, etc. In fact, one of the co-authors of that article later published a relevant paper on wolverines/snow (McKelvey et al. 2011. Pop. Ecol. 53:263-266) arguing that this type of correlational analysis cannot lead to defensible cause-effect conclusions. This section also states “Lynx also need landscapes where they are unlikely to suffer reduced fitness because of competition with other hare predators, or encounter traps or other anthropogenic causes of mortality” – we would argue that could easily describe much of Canada where lynx are secure. There are of course other hare predators there (to varying degrees, weasels, raptors, red fox, fisher, marten, coyote, wolf, wolverine, and even red squirrels have been documented to prey on hare leverets). We’re not even sure if lynx are consistently in the top 2 - e.g., see Tables 6.3 and 6.4 in Hodges 2000. Consumption of hares by all these CAN reduce lynx fitness (to an unknown degree), yet lynx are abundant there. And some lynx certainly “encounter traps” in much of Canada and Alaska. The themes of paragraphs like this are not well supported by data, are filled with speculations, usually negative, and should be eliminated without more support. “We don’t know”, as you started with, is sufficient.

- 16) P. 33 – “Neither the Montana nor Maine estimates incorporated rates of immigration/emigration”. We would also just note that none of the lambda estimates in that paragraph include confidence intervals, and had they, it would not be surprising if many encompassed both positive and negative values for lambda.
- 17) P. 34, first full paragraph – we would eliminate most of the first sentence and say “In summary, lynx need.....landscapes with hare densities capable of supporting.....(i.e., the second sentence). The rest is speculation.
- 18) P. 36 - “In its 2003 remanded determination, the Service determined....”. We don’t like the remainder of this sentence/discussion being presented as either/or. There is good reason to believe that both dispersers and resident breeders (some which may have been dispersers) are important.
- 19) P. 38 – last 2 sentences in first paragraph – We have touched on some concerns related to this before, but re-state that we don’t really agree with the logic that competitors are known to be some big driver here. The presence of more generalist predators to the south may indeed contribute to reduced cyclicity (not necessarily reduced hare abundance). It is much more likely that reduced hare habitat quality is what reduces hare abundance in the south, and more generalists, due to prey-switching, reduce cyclic tendencies (which also means there may be reduced troughs as well, not just reduced “potential for high-density hare populations”). Average density of a fluctuating population will be lower than that for a stable population, other things equal. From this, one COULD actually argue that generalists can be good for lynx. But in fact lynx are not better off in the south because all other things are not equal – hare habitat is generally worse or patchier.
- 20) P. 38 – “Long-term snow conditions presumably limit the winter distribution of potential lynx competitors such as bobcats (McCord and Cardoza 1982, p. 748) or coyotes”. In our opinion, “presumably limit” is not useful. While we did not have a copy of the book that chapter was in, we do have the newer version and only see some anecdotal reference to this idea, which we have already questioned. Of course snow has “effects” on animal movement/etc, and maybe more so on coyotes/bobcats than lynx, but I am unaware of any data to support the idea that this alone creates anything but possibly local-scale allopatry, with no demonstrated effects on

bobcat/coyote demography, or then in turn on lynx persistence. Repeating these ideas over and over is misleading. This idea needs to be critically 'vetted' in one spot, which we believe leads to a conclusion of "effects unknown" as Murray et al. (2008) basically concluded, and then no need to mention again.

- 21) P. 43 – "Siren (2014, p. 10) suspected that the relatively few lynx detections documented in 2012-2014 may be related to the presence and abundance of bobcat, coyote, and fisher populations in much of northern New Hampshire". This article is not in Lit Cited (nor is Siren 2016 cited elsewhere). And this sentence, along with the one at the end of the paragraph, is just more speculation.
- 22) P. 44 – "In Minnesota, research conducted since the 2003 remand has demonstrated the continuous presence of a resident lynx population". Though as you note at the end of this paragraph, influence of immigration is unknown, so I guess "resident population" depends on how you define resident, and population.
- 23) P. 44 – "and there is no indication of substantial immigration from Canada after the 1980s (Squires in Lynx SSA Team 2016, p. 20)". What monitoring was in place starting in 1980 that could confidently identify "immigration from Canada" if it occurred.
- 24) P. 53 – Unit 2 NE MN – since it was noted for Maine, Minnesota has also distributed the "How to Avoid Lynx" brochure to trappers at our fur registration stations and made it available at our website. And we don't think it is correct to state that in 2015 we added more trapping regulations for lynx avoidance. Administrative procedure just required that we re-issue the same emergency rule that was in place before. No changes have been deemed necessary.
- 25) P. 54 – "which requires Montana to implement a set of *reasonable* restrictions on trapping in lynx habitat". Time will tell if it is effective (they only had ~ 1 lynx take per 3 years before). Reasonable is in the eye of the beholder, and is unnecessary here. Why use it?
- 26) P. 55 – "....the use of body-gripping traps (foothold, conibear, snares, etc.) for trapping other furbearers is prohibited in Washington (except for damage control or nuisance wildlife, which requires special permits). This avoids the potential for lynx to be incidentally captured in traps set legally for other animals". True, and by our own arguments above we would say this next point fits the "effects unknown" summary, but using the argument this document has suggested many times (competitors are assumed to have an effect) would suggest that your statement should be modified to note that the trapping prohibition in WA could also have *negative* effects on lynx via 'allowing' more potential competitors. Same goes for Colorado statement later.
- 27) P. 57 – Unit 2 NE MN – while we can't honestly say how relevant it is, MN state forest management is also FSC and SFI certified. We also question whether regulation is the only relevant factor here – no doubt a fair amount of logging in MN has offered some lynx/hare benefits, but it is mostly driven by economics not regulation.
- 28) P. 59+ - as argued elsewhere, we think the Climate Change section should be 'dis-banded'. Mechanistic sections (hare habitat, lynx survival, competition, etc) should be the focus, with all potential (positive and negative) changes related to climate change/veg management/etc falling underneath there, and only if there is some defensible connections. As is, there is unnecessary definition of what "climate" means, general discussions that "climate change may be bad for wildlife", then even much discussion of why in the past you concluded climate change was not likely relevant in this case, but that now you think it is. And then many of the specific ideas you put forth are repeated many times. Focus on the mechanistic connection, cite any literature

that actually attempted an analysis (not just said “might affect”) on how climate change may explicitly affect that variable, and then critique whether the science really demonstrates a causal link to lynx/hares. There is so much uncertainty in all of this that it does not warrant repeated speculative statements, nor does repeating it make it any more true.

- 29) P. 65 – “Greatly reduced lynx fur harvests in Canada beginning in the mid-1980s may be linked to climate warming (Yan et al. 2013, p. 3269)”. While using “may be linked” may make this defensible, we do not feel it is objective. It is well known that the end of WWII, as well as the mid-1980’s, both ushered in a period of declining fur prices, and both preceded by high fur prices. While there is likely no data that can now re-create the past truth (only look for correlations), the most parsimonious conclusion is that these declines in lynx fur harvests were a result of (possibly lagged) declines in trapper effort, and possibly overharvest preceding this, as suggested by Poole (1993) and Mowat et al. (2000). Yan et al.’s attempt to consider this alternative (their appendix S6) is not compelling to us, and would argue that parts of it make our case. But absent discussing our specific concerns with them, at best we would say one could only conclude that “climate change” (not just climate) could only have potential relevance to the 1980/90s decline, not the 1950’s decline. And even then, this idea would only become an alternative, also untestable, HYPOTHESIS to what we would argue is a more parsimonious explanation (fur prices/effort). But nothing to do with this point, which includes some suggestive literature, is even mentioned here – only that it “may be linked to climate warming”. It also may be linked to fur prices, fuel prices, other economic opportunities for a trapper (e.g., job growth in the 50’s and 90’s), weather affecting trappers (not lynx), etc. The wolverine article we mentioned earlier (McKelvey et al. 2011) is also relevant here in terms of harvest data concerns, as well as the concerns with snow correlations.
- 30) P. 66 – “lynx are subjected to niche displacement to habitats of inferior quality, which probably limits lynx survival and productivity at the southern edge of their range (Peers et al. 2016, entire; Robinson 2006, pp. 120). Our bigger point here is once again this is purely speculation, at least the second part of the sentence. But another point here is that we’re not sure it is even stated correctly – by our read (of Peers), they concluded that lynx might be displaced FROM the supposedly poorer lynx habitat, not INTO it. For example, it says “[lynx] avoid competition at large scales by restricting their niche to highly suitable conditions....”. As such, this would not necessarily lead directly to reductions in survival or productivity, though it could affect density (but all is still speculation, as is almost all of this paragraph).
- 31) P. 67 – “...coyotes were deemed the most likely to pose local or regionally important exploitation impacts to lynx”. Yet there is really no data of any demographically “important” effects and they do co-exist in many areas. And as noted before, Murray et al. (2008) concluded there is insufficient data.
- 32) P. 67 – “The hybridization rate is currently low between the species (0.24 percent) but could increase as bobcat populations move north with climate change (Murray et al. 2007, p. 1465; Koen et al. 2015, p. 528). We suppose anything “could increase”, but it also might not. Is there more support for one speculation? If the bobcat/lynx ‘boundary line’ just moves north as some predict, why would the rate of hybridization be expected to increase. It would only be expected to POSSIBLY increase, we think, if bobcats advanced north but lynx did not contract.
- 33) P. 67 – “The diverse predator community could explain why hare populations have declined and seem to remain low in Maine (Scott 2009, p. 43)”. We could not acquire this thesis, but it’s clear

that it is more speculation (“could explain”). And based on how the sentence is worded (just says “Maine”), it doesn’t seem correct or at least appropriately qualified. Over the last 30ish years, haven’t hares been quite abundant and lynx doing well in Maine (better than historically you conclude)? To what part of Maine does this refer? Is there evidence of increase in predators in that area, evidence that hare mortality from them has increased, etc.? Elsewhere the focus seems primarily that hare HABITAT quality may have (or be starting to) decline after peaking in the 1990’s. What data even leads to this specific speculation that predators may be to blame?

- 34) P. 69 – “Some areas of the DPS (e.g., Maine, Minnesota) lack potential elevational refugia and, therefore, lynx populations are anticipated to decline accordingly (Carroll 2007, pp. 1098–1102)”. There are a whole lot of “If’s” behind this “are anticipated to”, so we see little reason to report this as though a fact. At best, it should say “the potential for latitudinal contraction could be comparatively higher in these Units due to minimal elevational relief”.
- 35) P. 72 – After concluding there are no real current problems, you state “However, exotic species could be introduced in the future as boreal systems are increasingly exploited for forest products, mining, energy production, and other natural resources (Schinder and Lee 2010, entire)”. All sorts of things COULD happen – we might develop a highly effective control for some exotic species. But if you are going to make negative speculations, then they at least need to be based on some attempt at analysis. What exotic pest is deemed most likely, what is the specific mechanism that will ‘transport’ it to the boreal forest, what is the causal link to lynx persistence (e.g., some disturbances, exotic or not, could be ‘good’ for lynx/hare habitat)? This is a Status Review and should only include best knowledge of current status, with clear and defensible shorter-term forecasts about future change. This speculative sentence, which is not the only one of its kind, assumes negativity and is uninformative – delete it.
- 36) P. 72 – “For example, in January, 1998 a severe ice storm extensively damaged the canopy of many northeastern U.S. and eastern Canadian forests, causing moderate to severe forest damage to over 10 million acres in the Northeast U.S. and southern Quebec (Jones and Mulhern 1998, p. 19; Irland 2000, entire; Millward and Kraft 2004, entire)”. But if nobody has documented specific effects for lynx, why say this? There was also a blowdown of trees in Minnesota’s BWCAW that affected ~ 400,000 acres, and probably?? improved habitat for lynx/hares. Unless there is some reasonable data to show a connection to lynx/hare demography (e.g., the Maine story), simply say disturbances are projected to increase, some could be good for lynx/hares, some bad, but we can’t predict the future. It would shorten the document a lot.
- 37) P. 72 – “No apparent climate-influenced parasites or diseases have been identified that would affect Canada lynx or snowshoe hares, but lynx experts believed this is difficult to predict and remains a possibility (Lynx SSA Team 2016, pp. 27, 37-39)”. Same general comment as #35.
- 38) Starting on p. 73 – Vegetation Management, Wildland Fire Management, and Habitat Fragmentation sections– this is probably redundant with one of our initial comments, but we see no need for these section headings (or Climate Change), nor the need for much of this information to be anywhere in this document. Use very mechanistic headings (e.g., “Projected Changes to hare habitat”), concisely discuss in one spot all relevant processes (disturbances, veg mgmt., human development/fragmentation, or whatever) for which we have supporting studies (e.g., hares depend on X, not just “X might affect Y”) and for which we can demonstrate

reasonable confidence that changes will occur in the NEAR future. We see little if any need/value for general reviews of all the hare habitat literature, different ways commercial timber management takes place, how such methods may or may not affect hare habitat, what economic trends may occur, whether/how the forest industry (or other land management agencies) might adapt to projected changes in forests from climate change, what historic fire regimes (or human policies toward them) have been or might be, how humans fragmented the landscape in the past, why snow is supposedly so important (again), what fragmentation means, more general review of lynx/hare literature, whether lynx have been documented to cross/use/get hit on roads, how many ski resorts there are out west, what locatable or salable minerals refer to, that utility lines are often along road corridors, etc. This is completely unnecessary, at least for our conception of what a Species Status Assessment should contain. Besides just adding an enormous amount of superfluous information, it forces the constant repeating of many highly speculative ideas. For this reason, we will not offer many specific comments on these sections, instead hoping that much is simply discarded. But we will offer a few.

- 39) P. 86 – first paragraph – yes, fire frequency and size COULD increase, and yes, this does HAVE THE POTENTIAL to cause temporary adverse impacts on hare habitat, but depending on details, it also COULD be positive (e.g., be a counter-balance to historic fire-suppression policies). I don't think there is sufficient predictive capability to decide how this will play out. You correctly note here that any negative effect may only be temporary and followed by positive effects, but add that even so it would likely (any citation?) reduce landscape-level hare densities, and therefore lynx numbers. Possible, but are periodic reductions in landscape-level hare densities not a historical reality of boreal landscapes and lynx-hare dynamics. Even lynx, along with other cycle contributors, can cause landscape-level reductions in hare density. 'Stability' is not the norm in these settings, and temporal/spatial variability should not be viewed as abnormal or bad. Useful conclusions can only be drawn if we can predict with high confidence how big/how often/where fires would occur in a given area, and we can't. Could be good in some areas, bad in some, good at one point in time, bad at another.
- 40) P. 87 – "Lynx must contend with aspects of their habitat at the southern extent of the boreal forest for which they are not as well-adapted". As a side note, in our opinion the only thing in these areas that they are demonstrably not able to 'deal with' is lower hare density. Regardless, this general idea is nonetheless true, but also true for every species at their range limits and it should be noted that this is "the norm" for lynx in the DPS. Historically, lynx in these areas have almost certainly been *comparatively* rare, ephemeral, unstable, patchy, or variable.....and yet persistent over the long haul. The problem in our opinion is that ESA generally ignores everything north of the border, expects consistent 'safe population levels', thereby ignoring historic reality (instability, especially at the range edge), and then leads to assessments that portray all of this historic reality as now being "risks". Certainly humans can, and have, altered the system. But so has 'nature', and we do not have 1,000 years of lynx/hare abundance data to offer any clues of just how much natural variability there was in their southern numbers. There have always been a lot of 'undesirable' conditions in the DPS for lynx, and it is important to not lose sight of this, but I think it commonly does in this section. In the beginning, a conclusion is that habitat loss/fragmentation has been relatively low in the DPS to date, then much discussion of why fragmentation can be bad. There are suggestions that the DPS naturally has patchier

habitat, then reasons why patchiness is 'bad'. Examples of why fragmentation may be bad, then a paragraph that concludes "...lynx showed no sensitivity to the degree of forest fragmentation in areas of high or low suitable habitat". Notes that the snow environment in the DPS is [naturally] patchy and marginal in space and time, then speculative discussion on why this is bad. Distinguishing 'bad' from 'normal' is not just semantics to us. It is, or should be, a very relevant focus. Even setting that aside for the moment, we note the following: P. 91 – "Roads, development, climate change, and forest management fragment snowshoe hare and lynx habitat in the DPS. We know little about how hare and lynx respond to these anthropomorphic changes to their habitat, which requires additional research (Murray et al. 2008, p. 1464; Squires et al. 2013, p. 194). Enough said.

- 41) P. 89 – "Hares fluctuate less dramatically in the southern part of the range of lynx, thus there is more competition for a limited resource and exploitation competition inflicted by generalists (e.g., coyotes) and other predators (Buskirk et al. 2000a, p. 95)". Hares may fluctuate less dramatically in the south BECAUSE of more generalist predators, but I don't see that the lack of fluctuation per se leads to more competition. In fact, by definition, generalists are typically 'prey-switchers'. Even if there are more species of predators, it doesn't mean there is more pressure on any one prey species, other things equal. We're not even sure if there is data to show that there are more hare predator species in the DPS compared to the north or whether annual hare mortality is lower in the north, but we think data in Hodges (2000) suggests "no" to both those questions. Our comment here also applies to the last sentence in the second paragraph on this page.
- 42) P. 99 – "Hares do not seem to cycle in this region, but underwent a 50 percent decline starting in 2006 and have remained at lower levels". Perhaps a citation for these observations is listed elsewhere in the document, but we would like to see it in order to evaluate just how much confidence is behind it.
- 43) P. 101 – Unit 3 discussion – "Regulations prohibit lynx trapping and require measures to reduce the likelihood of trapping lynx incidentally when legally trapping other species". Can't this be noted for all Units?
- 44) P. 120 – "Of those 42 hybrids, 13 unique individual lynx-bobcat genotypes (5 Female, 8 Male) were also identified (Catton et al. 2015, p. 1)". We would clarify this sentence to say "genetic analysis indicated that those 42 samples were from 13 unique individual hybrids".
- 45) P. 120 – "The DNA analyses also showed persistence of individual lynx in Minnesota of 2 years (N = 27 lynx), 3 years (N = 11), 4 years (N = 5), 5 years (N = 6), and 1 female lynx tracked for over 5 years, who produced 7 kittens in Minnesota (Catton et al. 2015, pp. 3-5)". Small note is that the 2016 report is now out with slight updates if interested. But our main point here is just that since specific numbers are reported, we think total sample size is relevant. This was based on 236 individuals whose initial detection was not a mortality, meaning also that 78.4% have not been detected in more than 1 year.
- 46) P. 121 – "Identified factors affecting the current conditions of lynx in Minnesota include reduction in habitat quality or quantity, habitat fragmentation, climate change, increased access for competing carnivores, and human-caused mortality". This is a general statement that could be used for any wildlife species, not based on any specific "identified factor affecting the current condition of lynx in MN". Admittedly, that may just be due to lack of data, but the wording of this statement is, for the most part, not supported by any specific data or analysis.

- 47) P. 121-122 – Factors Affecting Current Conditions (in MN) – Starting with the second paragraph in this section, a majority of the content is just generic statements unsupported by specific data/citations. Regardless of generic statements of “could affect”, “might impact”, the best available data, imperfect as it may be (but consistent across 2 surveys; Erb 2015), is that hare numbers in northern MN appear to have increased over the past 15-20 years, yet this is not mentioned anywhere. And this, in spite of, or coincidental with, a notable increase in bobcats over the same time, a reduction (we assume, but didn’t specifically confirm) in snow conditions, and at least no clear indication of any notable change in lynx. In regards, to snow-compacted trails, we reiterate that Murray et al. (2008) stated in their review that “the issue of snowmobile trails harming southern lynx populations has been adopted without strong empirical support”. You also say that “Throughout the SNF and northern Minnesota, human activities have reduced connectivity between patches of suitable lynx habitat”. We’re hard pressed to believe this is the case, as there are few major roads/barriers in this area (and we know lynx can and do cross or go around them), secondary forest trails are unlikely to affect/impede lynx, and they are a highly mobile species. And see the conclusion about NE MN having only minor fragmentation on p. 189, which basically contradicts this other statement. If there are any key factors “affecting current conditions”, we’d argue it has been the logging increase that began in the mid-80’s and continued perhaps to the present, and this has probably been beneficial to lynx, or at least hares.
- 48) P. 122 – 2 sentences – “Bobcat and coyote populations already appear to be increasing in Minnesota (Erb 2014, p. 40)”, and “...similar to bobcat, wolf populations may increase with changing snow conditions and prey availability as influenced by climate change”. First comment is that an important clarifier to the coyote increase is that this applies only to non-forested portions of MN. In forested areas relevant to lynx, coyotes have not increased, and in fact have likely decreased (presumably due to wolf presence in the northern forests; Levi and Wilmers. 2012. Ecology 93:921-929). Other than the observation that wolves COULD kill a lynx, nobody has really suggested that wolves are likely to be a lynx competitor, and we would agree. So rather than your hinting (in our opinion) that a climate-induced wolf increase could be bad for lynx, we would argue that it could be good (keep coyotes suppressed, if that really matters to lynx), or at least a mitigating factor to any possible bobcat increase (if bobcats really affect lynx). The other noteworthy of mention for this section is that if deer (and bobcat and wolves) do increase, it is at least something very amenable to management action (increase deer hunting quotas) if there is the political support to keep deer densities lower (as has been considered in the name of moose management).
- 49) P. 122 – last sentence in this section on hybridization – see our comment #32.
- 50) P. 156+ - As we began to read this section, we were happy to see full acknowledgment of the vast amount of uncertainty in longer-term forecasting here. But before we read on, we already knew that such forecasting was nevertheless done, in spite of this. We certainly recognize that decisions often have to be made in the face of much uncertainty, and that the process required to make projections can often be fruitful. Nevertheless, we put little faith in long-term projections in these situations, even if it may serve as a useful academic exercise. And we question whether long-term forecasting is even REQUIRED in this situation. That needs to be justified. We know that one argument here is that this document is not a “decision document”. But it will obviously be used in a decision that has many implications, so we think it behooves

the process to only present information which can be defended as reliable. We would note that most wildlife 'system dynamics' are nonlinear, and chaos theory tells us that even in deterministic systems (e.g., IF we knew all the biological/climate/management variables AND they were constants), future projections are still highly sensitive to initial conditions so even minor errors in our knowledge of the 'start conditions' can lead to exponentially diverging projections into the future. And there is indeed much uncertainty in our knowledge of the current state of the system (i.e., no reliable population estimates). So what is a reasonable time to consider? We don't have the magic answer, and from a 2009 Memo we saw from the Solicitor to the Director of USFWS, neither did they. But from my read of the Memo, we strongly doubt there is "...not only the foreseeability of the threats, but also the foreseeability of the impacts from the threats" 87 years out (to 2100). The Memo also notes that "...the foreseeable future extends only so far as the Secretary can explain reliance on the data to formulate a reliable prediction. What must be avoided is reliance on assumption, speculation, or preconception". I would argue that even in the shorter-term, MANY statements rely on quite a lot of biological assumption, speculation, and perhaps preconception. The Memo also notes that "The further into the future that is being considered, the greater the burden to explain how the future remains foreseeable for the period being assessed". On this point, we note that this seems in contradiction to your (correctly) stated observation that the further out you look, the LESS confidence you have. Finally, ".....the mere fact that someone has made a prediction concerning the future does not mean that the thing predicted is foreseeable for the purpose of making a listing determination....". Putting all this together, we simply do not believe projections to the year 2100 should even be included – the process of having discussed it internally is fine, but all that should be reported is that it was concluded to be unreliable. We think a priori considerations alone should make this clear, and we think this is reinforced by Figure 7 which shows significant variability in the assessments of lynx experts (i.e., the difference between Median-low and Median-high projections). And we would argue even this significantly underestimates reality (i.e., the true range of uncertainty is much wider...on both ends); lots of psychological studies clearly demonstrate there are many cognitive biases that lead us to have more confidence than we should (for a shorter popular discussion, see <http://www.nature.com/news/how-scientists-fool-themselves-and-how-they-can-stop-1.18517>, or perhaps the book called "The Black Swan" for a longer commentary). By reporting estimated persistence to 2100, even with the table showing (underestimated) uncertainty among the experts, there will become a de-facto assumption by many readers, including many decision-makers we suspect, that it is trustworthy (in spite of the uncertainty). For example, toward the end of the Executive Summary, which may be all many will read or later cite, it only says "...the probability of the persistence of resident breeding populations will decline in all geographic units, with the negative DPS-wide trajectory continuing to the end of the century...". And "The probability of losses in resiliency, redundancy, and representation puts the Canada lynx DPS at increasing risk of extirpation through the end of this century". Not a lot of uncertainty expressed there. Besides the fact that it is absolute probability values, not "increasing risk" or "will decline", which matters, these statements portray confidence that simply cannot be scientifically justified.....even if there is valid reason to believe it COULD be true. There is also a large body of psychological research showing that saying and then repeating ideas leads people to BELIEVE they are true and supported by data, even in cases where they are known to be false

(which we are not suggesting here). For the integrity of the document and process, we believe some of these statements and approaches need to be changed. At most, we can't see projecting beyond 50 years, and to be honest, we have little confidence in this case even past 20 years. Besides, aren't the SSAs to be done every 5 years (or 5 years post-delisting if by chance that happened), meaning you can update if more confidence is developed in the data and our ability to forecast? Saying "we don't know" is far more defensible than speculative guesses.

- 51) For the sake of time, and because we've already noted both our broad forecasting concern (#50) and numerous more specific comments above, we have opted to not review in detail all the information contained in Chapter 5, and to some extent even many non-Minnesota sections in Chapter 4. Nevertheless, a scan of these sections suggests that many of my above concerns also apply to comments made in these sections, and should you agree with any of them, then we feel changes need to be made in these sections as well.
- 52) P. 158 – "In each geographic unit, the experts we consulted expect the probability that resident lynx populations will persist will decline in the future, although uncertainty about persistence probability increases with time from the present (Lynx SSA Team 2016, pp. 36-49; also see 5.2, below)". While one can assume our forthcoming comment is already understood by all those that may read/use this SSA, that is probably wishful thinking and so we feel it needs to be made – except for a few highly improbable situations that could hypothetically occur, the estimated probability of persistence will always decline the further out in time you project, even if there are no KNOWN threats. The potential for *some* type of major negative event always becomes higher the further out you consider. This is true for individuals (which don't live forever), populations, and species. It is true for lynx, as well as humans. So a decline in estimated probability of persistence with longer timeframes is 'normal'. I believe this point needs to be explicitly acknowledged – a decline is not automatically a concern, it is the actual estimated probability that may matter (and we have already expressed concerns about the reliability of the actual numbers, especially those projecting more than 20-30 years out).
- 53) P. 158 – "Although all five geographic units that currently support resident populations (all units except the GYA) are expected by lynx experts to continue to do so through mid-century, only one (Northwestern Montana/ Northeastern Idaho) had an expert-estimated probability of persistence greater than 50 percent (i.e., persistence more likely than not) by the end of the century". We will assume we are mis-reading something here, but when we look at Figure 7, year 2050, average projection, it looks to us like the experts project that 4 of the 6 units, with a 5th close, are expected to persist with probability > 50%. Can this be clarified?
- 54) P. 163 – Unit 2 NE MN – Very little to add beyond that which we've said. We question what we think can only be called the assumptions of direct impacts of snow, bobcat competition, and hybridization concern. We do think the fate of the boreal forest will be crucial for hares/lynx, but question the ability (regardless of mechanism) to produce a reliable estimate of persistence probability 87 (or 50) years out.
- 55) P. 186 – "In response to a 2008 court ruling, the MN DNR began to draft a plan to address incidental take of lynx that may result from otherwise legal trapping in Minnesota. This plan is still under development by the MN DNR and will be designed to reduce the likelihood of incidental take from trapping (ILBT 2013, p. 49)". The State of MN completed and submitted an ITP/HCP to the USFWS in 2008. We also implemented regulatory changes, approved by the Court, to reduce incidental take of lynx.

56) P. 191 – “The lynx is state listed, however, and Minnesota's Endangered Species Statute and the associated Rules impose a variety of restrictions, a permit program, and several exemptions pertaining to species designated as endangered or threatened”. Lynx in MN are actually listed as a “Species of Special Concern” and thus not specifically covered by rules specific to our State ESA.

57) Pp. 190-192 (but also applying to pp. 183-190) – we would just start by saying our comment #50 applies here as well. To that we would add that we feel the tone of much of this section goes even beyond the concerns about speculation we have referenced above. We feel, correct or not, that much of this comes across as biased and ‘scare tactics’. Examples:

- a. Under the possibility of Federal protections being removed, and even if a state harvest did not occur, you say “Under the state statute, a person may not take, import, transport, or sell any portion of an endangered or threatened species. However, these acts may be allowed by permit issued by the DNR”. Notwithstanding our correction noted in # 56, were you really suggesting we might issue such permits liberally and jeopardize lynx? If not, what is the point?
- b. “There is a closed season on lynx, and it is expected that intentional take would continue to be prohibited until the population reached sustainable levels defined by the state”. You can expect this, and we could do it (though we doubt it), but what is the point of this unless you are implying it is a “threat” and using it to create fear about a post-delisting scenario? Are you questioning our ability to responsibly manage a harvest?
- c. “Without Federal listing driving voluntary conservation guidelines, however, there would be little or no motivation for private forest landowners to intentionally engage in forest management to benefit lynx”. What evidence are you suggesting there may be that private forest landowners have been compelled by Federal law to adopt voluntary guidelines now?
- d. “Without Federal-listing, these projects [wetland permits, highways, powerlines, etc] would not consider impacts to lynx critical habitat. The Core Team concludes that a future scenario without Federal listing would result in increased habitat loss and fragmentation and would result in reduced justification for habitat protection initiatives in northeastern Minnesota”. Can you even document (not speculate) whether Federal laws have ‘saved’ lynx or lynx habitat on Federal Lands (or on federally-funded projects) to date? In reality, there has likely been little if any practical effect of the federal nexus on county/state/private lands, but do you have any documentation of how non-federal-nexus-projects on those other, mostly state/county, lands in lynx range have harmed lynx? And if the focus is just the Federal lands, what are the truly ‘foreseeable’ projects expected to occur, what are the suspected effects on lynx, what mitigation could occur (even if not legally required), and will Federal land managers in fact disregard lynx if they are delisted (if so, THEY might want to reconsider). And while you are correct that federal listing certainly adds ‘legal teeth’ to many things, the assumption that nobody will care about or advocate for lynx habitat needs in our state forest wildlife management practices/policies is not valid. Fear of what COULD happen is not a justification for keeping a species listed.

- e. “In a future scenario without Federal listing, Minnesota’s incidental take planning effort for trapping may be further delayed or halted and may result in the diminishment of protective measures to minimize injury, take, and mortality of lynx. As it is, approximately 16 lynx have been reported to be incidentally trapped in Minnesota since listing, resulting in at least 6 mortalities”. First, there have actually been 24 accidental captures and 11 mortalities in the 17 years since delisting. Second, we HAVE lynx avoidance regulations in place and have not documented any need for changes at this time. Are you implying we need to make changes, that we will drop existing changes if de-listed (but still state-protected), or that incidental take is or would be a population-level concern even though it was not deemed a threat at listing? We see this as biased fear, and one that suggests you believe the state is unwilling to address new documented concerns should they arise.
- f. “It is unlikely that lynx would become a legally trapped furbearer in Minnesota (although a legal wolf hunt was reinstated post-delisting of that species in Minnesota, so it may also be suggested for lynx)”. Side point – you could clarify by adding that the first wolf season was ~ 20 years after the wolf population in MN/WI/MI surpassed federal numeric recovery goals. Main point – why do you feel the need to say this unless you are assuming we will start a season, and would do so in a manner that would jeopardize lynx? And if this is your assumption, is this not a catch-22 situation – if you delist, it indicates there are no serious threats, and then you turn around and imply a threat from delisting. Would there not be a 5-year PDL monitoring plan? Would you not be able to re-list if we in fact enacted all the changes you suggest we COULD and harmed lynx populations?
- g. “Illegal shooting and non-reporting would likely increase without federal protection. High-profile law Federal enforcement cases may have helped to reduce illegal shooting of lynx”. Is there any data to support this idea? We’ve never met a poacher who self-reported or even cared about the law (state or federal). And for accidental take, if anything, reporting *could* actually increase.
- h. “With a diminished snow regime, populations of bobcats would be expected to increase and expand north and eastward into areas currently occupied by lynx. Incidental take of lynx from bobcat trapping and hunting activities would likely increase without Federal listing”. I would describe this as an “IF, IF, IF, Then POSSIBLY” statement. Plus, you have repeatedly suggested, we argue with little to no supporting data, that bobcats will somehow outcompete/exclude lynx if they do expand, so how could incidental take increase if the lynx will have ‘moved out’ or been killed soon after bobcats arrive? Plus, would not the limited accidental take of lynx that might occur be offset by the removal of the supposed lynx-killing/competing bobcats and coyotes by trappers/hunters? Our next comment is partially relevant here too.
- i. “Similarly, fisher, fox, and coyote populations may increase in a diminished snow regime in northern Minnesota and trapping would be expected to occur there that may lead to greater incidental take of lynx”. Trapping already occurs “there” for these species, coyotes are unlikely to increase anyway (unless wolves recede, and if that occurred, red fox might decrease). So how much would the potential impact of an unknown amount of a suspected increase in incidental take be offset by a suspected increase in trapping

of these suspected lynx competitors. And setting this aside, this logic suggests you have quite a bit more confidence than we do in being able to predict future fur prices and trapping effort.

- j. “We believe that despite a closed hunting and trapping season, incidental take would continue and possibly increase and could become a significant threat to a population of lynx that will likely be significantly diminished by mid- to late-century”. See many comments from a-i.

Thank you for the opportunity to review the draft. We hope our comments were helpful and look forward to your responses.