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We are extremely grateful to the peer reviewers for their clear and constructive feedback on our report entitled "An integrated population model to inform harvest management of mourning doves in the Central Management Unit of North America". Here we describe to the Central Management Unit how we have addressed the main comments and critiques, which are prefaced by "*Our Response" using a different font. We also reference page and line numbers in the revised version of the report to facilitate navigation to our changes. All minor grammatical edits were implemented.

Sincerely,

David Koons

Dr. David Koons
James C. Kennedy Endowed Chair in Wetland and Waterfowl Conservation

On behalf of Mark E. Seamans and David L. Otis

Comments and suggestions provided by peer reviewer Todd Arnold:
This manuscript outlines a rigorous and comprehensive approach to managing mourning doves using an integrated population model that relies primarily on harvest and banding data. I have no outstanding or serious concerns, but offer up a variety of comments for possible rewording of certain sections.

Page 1, paragraph 1, sentence 2: A decline of 25% sounds potentially serious, but putting this into the context of what has happened to other bird populations (e.g. citing the Rosenberg et al. 2019 paper estimating 2.9 B birds lost, 29% decline across the North American avifauna) would minimize its impact.
Our Response: *This is an excellent suggestion to put the trend into the context of what has happened to North American avifauna in general because mourning doves have declined at a similar rate. We have revised p. 1 par. 1 to acknowledge this.*

Page 1, paragraph 3, sentence 3: I seem to recall another David Koons who published a substantive critique of the midcontinent mallard AHM model, but perhaps I'm mistaken? :-
Our Response: *Touché good sir. We have revised the noted sentence to read as "An example of a derived strategy is the adaptive harvest management of mallards (Anas platyrhynchos) in the midcontinent of North America (Nichols et al. 1995), though the need to more actively adapt these models and overcome shortcomings in the ability to accurately forecast abundance have been noted (e.g., Koons et al. 2022)."*

Page 2, paragraph 1, "even when some data are missing": This list of citations really needs to conclude with a "but see Riecke et al. 2019, Paquet et al. 2021" on the dangers of completely latent vital rates (i.e. they also absorb bias from other parameter estimates).
Our Response: *We have reordered statements in p. 2 par. 1 to accommodate insertion of the sentence "But practitioners should be aware that completely latent parameters in an IPM can absorb bias from other parameter estimates (see Riecke et al. 2019, Paquet et al. 2021)."*

Page 4, middle of page: The Zimmerman et al. (2010) paper on Bayesian estimation of fecundity from harvest data seems so important to this model development than I think you should cite it here.
Our Response: *We cited this important paper elsewhere but great point to bring recognition to it specifically for the way in which we estimated fecundity from harvest data on p. 4 par. 2 (which we have now done).*

Page 4, 2nd paragraph: For mallards, spinning wing decoys created a sea-change in juvenile/adult vulnerability to harvest (one that occurred about 1999 and seriously messed up fecundity estimates throughout most of the history of the midcontinent AHM models). I don't dispute that temporal variation is unimportant for MODO, but think very carefully about this assumption.
Our Response: *Excellent point; please see the heavily revised p. 4 par. 3 that addresses the validity of our assumption and acknowledges the need to evaluate it periodically.*

Page 5, top: Weegman et al. (2021, Ecol Applications) provide a very different perspective on this concern about independence, including an example where banding data provide both survival and abundance estimates, but with minimal impact. You could simply append the reference with (but see Weegman et al. 2021).
Our Response: *See our combined response to the next comment.*

Page 6, top: Statistical independence does not require absence of overlap between data sets – indeed absence of overlap violates that assumption. If 10% of the population contributes survival data and 10% is included in count surveys, then 0.10 * 0.10 = 0.01 should be included in both data streams. Analyses like Padding and Royle's 2012 analysis of bias in waterfowl harvest require that these 2 samples are statistically independent, with an expected amount of overlap (good luck with that!).
Our Response: *We have acknowledged these points in the revised p. 6 par. 2.*

Page 6, end of 2nd paragraph: Can you cite someone, unpubl. Data or pers comm, for the recent reward band study?
Our Response: *We now cite M.E. Seamans unpublished data on p. 4 par. 1 and throughout.*

Page 8-9: re. the poor fit of GOF tests to HIP data. Can you explore this result a bit more (as in where was the lack of fit most evident)? From other taxa (woodcock, waterfowl), I suspect that the lack of fit might have been most severe during the first few years of transition to HIP (but I guess your IPM starts well after implication of HIP).

Our Response: *We noted on p. 9 1st paragraph of Results that the lack of fit was attributable to disagreement between the data and model predictions in 2016 specifically, but that fit was generally good in all other years. So it had nothing to do with the transition to HIP from the previous harvest survey. Based on a track-changes comment we also inserted the statement "Moreover, the mild disagreements among datasets yielded greater uncertainty in IPM estimates of harvest compared to those based solely on HIP data (e.g., Fig. 2), thus accommodating model uncertainty for use in decision making."*

Fig. 7: I'm not a huge fan of yield curves for wildlife populations, given that populations in temperate environments exhibit dynamics more like Keeling curves than logistic growth curves. However, I acknowledge the near absence of theoretical frameworks to provide proper alternatives (Boyce et al, 1999, Oikos at least point out some of the issues). However, I can't imagine how ignoring seasonal variation would lead to overharvest – it would lead to more conservative approaches – so if you're fine with that (and I think "precautionary principle" is always a good approach to harvest management), then no worries.
Our Response: *These are valid points, which we now acknowledge in a new paragraph added near the top of p. 9.*

<u>Comments and suggestions provided by peer reviewer Jeff Hostetler</u>:
The authors developed a very impressive modeling effort for several streams of Mourning Dove data to help set adaptive harvest regulations for this abundant but declining species. I found a lot to like in their modeling approaches and presentation of the results, but the report was in places quite dense, and the presentation of the methods could be clearer.
**Major Concerns:** None

**Minor Comments:**
Lines 48-49: It would be be good to add Zipkin and Saunders (2018) to this list of references.
Our Response: *An oversight of ours, which we have now added to p. 2 par. 1.*

Line 189: I think the math here might be deceptive. Surely there wasn't a separate variance for every region and year combination! I understand there were different random effect variances for different vital rates – this is how these variances should be labeled. For example, this variance could be labeled with an epsilon subscript or superscript.
Our Response: *This refers to the defined parameterization of epsilons in the text following Equation 2. The reviewer is correct, the epsilons for group-year combinations (g,t subscripts) were constrained by a common temporal variance for a given group (just g subscript); the t subscript on sigma was dropped in the revision.*

Equation 3: This variance should also be labeled (with an alpha subscript or superscript?).
Our Response: *Good point, to distinguish this variance term from that for Equation 2, we opted for a different font for sigma (that appears quite different than the typical font) rather than using subscripts.*

Lines 205 – 207: As the authors mention elsewhere, simulation studies have not found many cases where this really matters (Abadi et al. 2010; Weegman et al. 2021).
Our Response: *See revisions made in response to reviewer Todd Arnold who made the same comment.*

Lines 207 – 212: This sentence is confusing me. I recommend rewording it (doing so may require breaking it into two or three sentences or leaving out part of it).
Our Response: *On p. 6 par. 2 we have restructured this confusing sentence to read "Out of caution about independence among data, one can alternatively estimate latent abundances within an IPM using a rearranged Lincoln likelihood that a) makes use of total harvest data each year ($H_t$) within the IPM that b) acknowledges that each $H_t$ is an uncertain estimate attained from the HIP survey (i.e., has a time-specific sampling variance $\sigma^2_{H,t}$), while c) depreciating latent estimates of abundance by harvest rates estimated from banding data within the IPM. In other words the banding data are used once, as opposed to a double usage that occurs when external Lincoln estimates of abundance are supplied as data to the dove IPM (once to externally estimate harvest rates or direct recovery rates to derive Lincoln estimates of abundance, and again to estimate survival and harvest rates within the IPM)."*

Lines 268 – 288: I've read over the Density Dependence section a few times now and it's still not sinking in. Was the two-step process described in lines 276 – 288 (it looks like more than two steps to me) run after the exploration of IPMs with DD described 269 – 275, or part of it, or what? Can the explanation for why density was treated as a separate parameter from abundance (equation 9) be fleshed out a bit more? Were the last few steps (lines 281 – 288) run within an IPM, using separate BUGS code, done all as derived parameters in R, or what?
Our Response: *The phrasing of sentences in this sub-section were overhauled to be as crystal clear as possible about our 'multiple imputation' approach to modeling density dependence. See bottom of p. 7 and top of p. 8.*

Line 355: would it still be correct to add "into the IPM" after the parentheses? If so, I think that's a bit clearer.
Our Response: *Correct, we have added this phrasing at the end of the Figure 3 header.*

Lines 358 – 259: I'm confused again. This sentence doesn't appear to be in summary of any of the previous results. Was something removed?
Our Response: *In the text immediately preceding Fig. 4 we deleted 'In summary'.*

Lines 386 – 390: This ends with "We note the uncertainty in these estimates that is crucial for probabilistic decision making," but I notice that none of the estimates presented in this paragraph include a measure of uncertainty (SD or BCI). Those should be included if possible.
Our Response: *Good point. We have now included 90% BCI on these statistics in the text immediately above the Fig. 7 heading.*

Lines 399 – 400 (Figure 7 caption): What is the grey area? 80% BCI? 95%?
Our Response: *90% BCI, which has now been added to the Fig. 7 heading.*

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Lines 403 – 440: I like this section, but it's as much Methods as Results. Maybe it should be bumped up a level (i.e., INTRODUCTION, OVERVIEW OF IPMs, STUDY POPULATION, METHODS, RESULTS, STATISTICAL DECISION ANALYSIS, CONCLUSIONS, etc.).
Our Response: *After thinking about a number of other limitations relative to long-term management objectives, we have opted to remove this section entirely.*

Lines 422 – 430: The subject of these sentences changes in a disconcerting way. It starts with what decision makers could do, then continues with "we next." I understand what is meant, but this should be fixed (probably by adding "we set 90% thresholds" to the first part).
Our Response: *See above.*

Lines 451 – 456: Good caption but see comment on Figure 7 caption.
Our Response: *The grey area is now described as the 90% BCI in sustained yield.*

Lines 466 – 467: Given that this report was completed in 2024, abundances in 2022 and 2023 are no longer hypothetical. I'm not suggesting rerunning anything with new data, but a short statement here about how these predictions line up with the new estimates would be useful.
Our Response: *Excellent suggestion. Estimates of abundance for 2022 and 2023 have only recently become available, and they happen to be higher than the IPM forecasts. We now highlight this, but have moved the statement to the 3$^{rd}$ paragraph of the Conclusions, have revised the text, and have added a new Fig. 8 to emphasize the need to iteratively update the IPM as new data become available.*

Lines 673: This reference (Koons and Otis 2022) is not in the main Literature Cited section, and this appendix doesn't have its own Literature Cited. Also, is "have not" the proper tense, or should this be "had not"?
Our Response: *The Koons and Otis citation was in the main Lit Cited, but we mistakenly provided the year as 2007 instead of 2022. This has been corrected. For conciseness, we provide all citations in one Lit Cited section. Moreover, we changed the wording to "…we have only recently evaluated the IPM goodness of fit (GOF) to data".*