

Peer Review Summary Document

(09/17/2012)

Peer Review Plan

http://www.usgs.gov/peer_review/docs/owl_competitive_interactions_2892.pdf [56.4 KB PDF].

Title and Authorship of Information Product Disseminated

Competitive Interactions and Resource Partitioning Between Northern Spotted Owls and Barred Owls in Western Oregon, By J. David Wiens.

Peer Reviewers Expertise and Credentials

Peer Reviewer #1: Professor, Oregon State University

Peer Reviewer #2: Research Wildlife Biologist, U.S. Forest Service

Peer Reviewer #3: Professor, Oregon State University

Peer Reviewer #4: Professor, Oregon State University

Peer Reviewer #5: Research Manager, U.S. Geological Survey

Charge Submitted to Peer Reviewers

The reviewers were asked to make an objective evaluation of the research.

Summary of Peer Reviewers Comments and USGS Response

All five reviewers provided minor editorial suggestions in addition to the comments noted below. All minor editorial suggestions have been incorporated where they improved the clarity of the text. The peer reviewers' comments below correspond with the same numbered comments written in the margin of each reviewer's copy of the draft manuscript.

Reviewer #1:

Pp. 43, Comment [EF1]: Maybe should be expressed as the mean number of young produced as opposed to the mean number for the 3-yr period? That way it would be easy to compare with other studies. **Author Response:** Deleted this sentence from the text because it contained extraneous information. The mean number of young produced per pair per year has been provided in Table 3.17.

Pp. 45, Comment [EF2]: Glenn et al. did not study owls in less fragmented old forests, did they? **Author Response:** No they didn't; deleted citation.

Reviewer #2:

Pp. 13, Comment [BA1]: Little data from nonbreeding season. What effect of bias here?
Author Response: *Have now emphasized the potential limitations of small sample sizes in the Discussion section on Pp.56–57. Also refer to author response to comment [BA9].*

Pp. 17, Comment [BA2]: Can any of this be diagrammed for easier interpretation?
Author Response: *Did not attempt to diagram estimators of home-range overlap because these are 3-dimensional measures that would be too difficult to diagram clearly; believe sufficient information on these measures is provided for the reader to interpret the results, including the formulas used for calculations.*

Pp. 33, Comment [BA3]: Which one? **Author Response:** *Revised for clarity.*

Pp. 40, Comment [BA4]: Time of year (season) of deaths? **Author Response:** *The time of year (season) in which deaths occurred are provided in Table 3.15.*

Pp. 41, Comment [BA5]: I think you need to include models with these effects in Table 3.16. **Author Response:** *Models with these effects were included in the analysis and are displayed in Appendix I; added a reference to Appendix I here to promote clarity.*

Pp. 48, Comment [BA6]: Are these probabilities? **Author Response:** *Revised sentence to indicate that numbers are probabilities.*

Pp. 53, Comment [BA7]: How do you know this? **Author Response:** *Revised sentence to indicate that this is one possible explanation for the pattern observed.*

Pp. 53, Comment [BA8]: ??Really speculative. **Author Response:** *Deleted sentence.*

Pp. 54, Comment [BA9]: What about the nonbreeding season? When prey abundance is most limiting. **Author Response:** *The reviewer made this comment before reading Pp. 56–57, which discusses a greater potential for exploitation competition during the winter when prey is most limiting.*

Pp. 56, Comment [BA10]: What about seasonal abundance of prey and effects thereof? Doesn't this mean that there is higher potential for competition for prey during winter?
Author Response: *Restructured the related sentences to emphasize these points made by the reviewer. Also inserted a brief sentence describing the potential limitations of sample size.*

Pp. 57, Comment [BA11]: This is highly speculative by these 2 studies. I suggest you omit this portion. **Author Response:** *Deleted sentence.*

Pp. 64, Comment [BA12]: Don't think you showed a depletion in food resources. **Author Response:** *Revised as indicated.*

Pp. 107, Comment [BA13]: I think this column needs a bit more explanation. **Author Response:** *Added text to table footnote "c" to promote clarity.*

Pp. 111, Comment [BA14]: I think you need to designate the B's that are significantly different from zero. **Author Response:** *As described in the text (Methods, pp. 26), regression coefficients that are significantly different from zero are those with 95% confidence intervals that do not include zero. Note that 95% confidence intervals are shown in Table 3.9.*

Reviewer #3:

Pp. 22, Comment [DH1]: Accuracy estimate? **Author Response:** *Unclear what the reviewer is referring to here; measures of precision for mean estimates are shown in Table 3.11.*

Pp. 23, Comment [DH2]: You used Simpson index for diversity. With the probability transformation done in it, no one has come up with a defensible ecological interpretation. I know you gave the standard line about combining richness and evenness in one number; how do you interpret it ecologically. Stick with richness. If evenness is important to you then also use an evenness index. **Author Response:** *As described on pp. 23, the reciprocal of Simpson's index was used as a measure of diversity in owl diets, which is standardized such that higher values indicate a more diverse diet. Provided the standard definition of this index but did not necessarily interpret this as a measure of dietary evenness. Provided a clear ecological interpretation of this measure in the Method (pp. 23) and Results section (pp. 38) that is consistent with the primary citations for the method.*

Pp. 22, Comment [DH3]: Minimum size? **Author Response:** *As stated in the Methods section on pp. 11, the minimum mapping unit size was 0.5 ha in all analyses.*

Pp. 107, Table 3.5, Comment [DH4]: You include in the table and reference in the text models with delta values >4 . The literature on AIC says use a delta value of 2 or 4 as a cut-off for models to consider. Someone else on your committee commented on this too.

Author Response: *Consistent with Burnham and Anderson (2002);, based inferences on statistical models with delta-AIC <5 . Models with delta-AIC >5 are discussed because they represented alternative hypotheses of interest, but they are not used as a basis for inferences and conclusions. Displaying the full range of alternative models considered in a table is appropriate and not aware of any literature that says otherwise.*

Pp. 43, Comment [DH5]: Some context for the SO success numbers is needed. What are comparable numbers elsewhere or before BO. In the exam, you said that weather and the transmitter could play a roll. Put this in. **Author Response:** *Added some brief discussion to the Discussion section (pp. 62) where measures of reproduction are compared to a previous study of NSO in my study area. Added a sentence and supporting citations at the bottom of pp. 63 line 3 to emphasize that there are many factors that can influence reproduction of spotted owls beyond just the presence of BO.*

Pp. 58, Comment [DH6]: I still don't agree with Barry about older forest being limiting. At the very least, you need to say this is in the context of a high density of BOs. **Author Response:** *Revised sentence as suggested to be within the context of a high density of BOs.*

Pp. 66, Comment [DH7]: Older riparian hardwoods and SO is a new relationship and one that is exciting. And riparian hardwoods really are disappearing pretty fast from the Coast Range through normal succession and fire exclusion. I think you can make a bit more noise about the SO/riparian hardwood association. Perhaps it even bears some relationship with the hardwood use in the mixed forest of SW Oregon by SO. **Author Response:** *Added some brief text to the top of pp. 68 to emphasize the importance of older hardwoods to SO and BO.*

Pp. 68, Comment [DH8]: Did SO population drop in central coast range before BO showed up? If so, this suggest habitat loss has been as much or more important than BO. Has SO habitat quality continued to decline over the last 50 yrs.? If so, hard to separate habitat from BO effects, even with removal of BO. **Author Response:** *The reviewer appears to be speculating here on possible causes of SO population declines, which was beyond the scope of my study. Chose not to speculate on the relative contributions of habitat loss and barred owls on population declines of spotted owls in this manuscript because my research was not designed to address these research questions (also refer to Pp. 64).*

Reviewer #4:

Pp. 11, Comment [WR1]: I suggest that you put the information on the accuracy assessment in the main text. It will also be important to talk about the accuracy of the forest structural data. Did you do an accuracy assessment on the forest structural data?

Author Response: *Information on the accuracy assessment of the forest cover map is included in the main text on pp. 11 and information on the accuracy assessment of the forest structural (GNN) data is also provided in the text on pp. 12. Additional information about accuracy of vegetation maps is provided in Appendix B (Pp. 123–126). Added some clarifying text to Appendix B as suggested.*

Pp. 44, 66, 68 Comment [WR2]: I suggest you discuss, near the end, potential effects of barred owls as a new and highly dense generalist species on prey species of concern such as red tree voles, mollusks, or amphibians (salamanders) on the species of concern list in Oregon. Also, there are literature cites that you use that describe the effects of exotic generalist predators on local native prey. For example, coyotes moving into Olympic National Park, causing a major decline of Olympic Marmots. **Author Response:** *Added a brief paragraph at the end of the Management Implications section that discusses this potential as suggested.*

Pp. 66, Comment [WR3]: Give cite. **Author Response:** *Inserted citation.*

Reviewer #5:

Last sentence of abstract, Comment [CP1]: This statement seems to be presented as a recommendation for a scientific endeavor rather than a management action. The former is appropriate for USGS, but not the latter. However in this particular case, we know that the Fish and Wildlife Service is in the midst of a formal decision-making process about this very subject. Therefore, this statement could also be interpreted as advocacy for a particular management action. You could make the same point and avoid the appearance of advocacy by saying something like: Experimental removal of barred owls on at least one study area would verify my results and test the hypothesis that competition with barred owls is causing declines of spotted owl populations. **Author Response:** *Revised as suggested.*

Acknowledgements, Comment [CP2]: This is a standard disclaimer we put on any publication that has product names. **Author Response:** *Inserted disclaimer.*

Pp. 16, Comment [CP6]: Why? Don't need to change the ms if you don't think it's relevant, but in most of the other analyses involving forest types you found distinctions between the importance of > 60 yrs old vs. >120 yrs old, but here you don't ask if the two ages of forests might affect your models differently. **Author Response:** *Added text stating that old and riparian-hardwood forest types were evaluated separately in my analysis (this*

was previously missing). The sentence describing how mature and old age classes were combined into a single covariate is correct.

Pp. 25, Comment [CP9]: you use "exploitative" elsewhere, and I think that's a more common usage - might want to change this to be consistent. **Author Response:** Correct - revised as suggested.

Pp. 40, Comment [CP12]: What's the difference between disease and severe bacterial infection, and between bacterial disease and severe bacterial infection? **Author Response:** Good comment; revised to combine disease, bacterial disease, and severe bacterial infection into a single category.

Pp. 44, Comment [CP13]: The ms is consistent with Table 3.4 in calling > 120 yrs old forest "old", but on pg 31 you parenthetically describe "older" as > 60 yrs old - but it's ok because it's clear what you're talking about in that section. But seeing "old" and "older" so close together in this context has me questioning what "older" means in this case, forest > 120 or forest >60? **Author Response:** This was an error. Should say "old" forest here not "older" forest; correction made.

Pp. 66, Comment [CP14]: Not essential, but since this is a pretty pivotal interpretation for managers, would be great to provide a citation, if you have one handy, for this interpretation by policy makers. **Author Response:** Deleted sentence because it was not essential.

Pp. 66, Comment [CP15]: isn't land use change an equally important, and synergistic, factor in species' movements? **Author Response:** Revised sentence to include land use change.

Pp. 66, Comment [CP16]: This can be read as advocating protection of old forest, which is a management action. You could make a nearly identical point and avoid advocacy by saying something like: This further supports the conclusions of Dugger et al and Forsman et al that the existence of a new and potential competitor like the barred owl makes the extent of old forest habitat even more important to spotted owl populations because any loss of habitat will likely further constrain the two species to the same set of limited resources, thereby increasing competitive pressure and leading to further negative impacts on spotted owls. **Author Response:** Revised as suggested.

Pp. 67, comment [CP17]: The first part of this sentence gives a management prescription which isn't USGS's role. Additionally, your thesis shows that reducing contiguous older forest would increase competitive pressure, but not that maintaining current forest conditions would reduce competitive pressure. You could make the same point by saying something like: While it is unknown if barred owl removal is an effective and feasible means of reducing competitive pressure on spotted owls, it is clear that further loss of contiguous older forest on the landscape would increase competitive pressure, as my analyses and others (e.g. Dugger) demonstrate that these two factors can act synergistically to influence spotted owls. **Author Response:** Revised as suggested.

The Dissemination

The published information product (dissertation) is available through the Oregon State University library (as of April 2012) and can be accessed electronically through the library's ScholarsArchive (<http://ir.library.oregonstate.edu/xmlui/handle/1957/28475>).