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August 5, 2004

TO WHOM IT MAY CONCERN:

From: Robert G. Anthony and Eric D. Forsman

Subject: Response to the Ecological Society of America's review of: Status and Trends in Demography of Northern Spotted Owls, 1985–2003

We have reviewed the comments and suggestions by the four anonymous reviewers on the above manuscript. In general, we found their comments and suggestions to be complementary of the report and helpful in improving it. We used most of their minor suggestions for improving wording, sentence structure, and providing clarity concerning our methodology. In addition, we have made some important changes to the report, some based on suggestions of the ESA reviews and some based on comments from the coauthors of the report. Here are a list of these important changes:

1. We omitted the results of the meta-analyses of fecundity and survival for the 8 monitoring areas, because these results were a mirror image of the results for all 14 study areas. This eliminated a lot of redundancy and some confusion by one ESA reviewer in the results and discussion. We believe this has made the report more concise and less redundant.
2. We omitted the original table 16: Evidence ratios and beta estimates for linear and quadratic time trends in apparent survival on owls. Much of this information was in table 15, and this table did not provide additional information that was germane to our results. We believe this also made the report more concise and less redundant.
3. Based on the ESA reviews, we added a section on "Possible causes of population declines?" where we merely speculate about the causes of the population declines that the data suggest. This section replaced the former section on "State and Study Area Specific Inferences", which originally contained some of this information. This section speculates about the causes of the declines with a more complete list of the possible causes for each study area individually.
4. We added two paragraphs to the Summary and Recommendations to explain why we chose not to provide management recommendations to the federal agencies to address the population declines. In these two paragraphs we merely point out that this is beyond the scope of our study and that decisions about management of the species and its habitat is currently being conducted in the Status Review by the U.S. Fish & Wildlife Service.
5. We attempted to focus more on the biological interpretation than on the statistical interpretation of our results. We made changes in wording and sentence structure in the results

and discussion to address this comment.

6. We computed weighted mean estimates of lambda for all study areas, the 8 monitoring areas, non-monitoring areas, density study areas, and territory study areas. The means were weighted by 1/standard error for each individual estimate of lambda. These weighted means are slightly different but more appropriate than the arithmetic means that were included in the previous draft.

7. Reviewer #1 asked about our assumption of a 50:50 sex ratio of juveniles. On some study areas we do have data from samples of juveniles that were sexed from blood samples. Those data indicate a sex ratio that is close to 50:50 and we have provided two citations to support this assumption.

As is the case with most reviews, we did not agree with all of the suggestions by the reviewers. Below we list the suggestions that we did not agree with and provide reasons for not making the suggested changes:

1. Reviewer #1 asked us to show the reader how good the models were in accounting for process variation by presenting R^2 values for the best models.

Response: Calculation of R^2 values in modeling of survival in program MARK is not appropriate and generally does not apply, because we have no way to partition the “residual” variation in comparison to how much of the variation is explainable. We do not have a reliable way to do this, so one is deluded if they think R^2 is informative in our analyses of survival rates. The informative results in model fitting in survival analyses are about parameter estimates and confidence intervals around regression coefficients and other measures of evidence for biological effects of the covariates. We provide these estimates and confidence intervals throughout the report.

2. Reviewer #1 asked us to set some limits on what we believe are the acceptable strengths of evidence for effects on fecundity or survival when evaluating regression coefficients and confidence intervals. He/she mentions some of Fisher’s work, but does not provide a citation.

Response: The idea with presenting probabilities is to avoid specifying an exact cutoff. Our feeling is that the *evidence* in this case is the estimated parameter values and their confidence intervals, and that is sufficient. Interpretation of this evidence is a value judgement. Thus, we are reluctant to use statistical methods that suggest exact cutoffs because this seems too much like the old paradigm of significant at <0.05 and not significant at >0.5 . We have no idea what the reviewer is referring to concerning Fisher’s advice without a precise literature citation. The 0.05 level of significance is a standard only because R.A. Fisher suggested it in the early 1920’s. However, one should discount p-levels of 0.05 as having any magical importance, because the 0.05 level is somewhat arbitrary and p-values can be very misleading depending on the power of statistical tests. We have chosen to present point estimates and 95% confidence intervals as the strength of evidence, or not, of an effect. Thus, we are not inclined to make the change suggested by the reviewer.

3. Reviewer #1 asked if there were any tantalizing trends in results that tempted us to test additional models beyond those that were *a priori* hypotheses (data dredging).

Response: In general, we did not test models beyond those that were in our original model set, because we are very much against any data dredging. However, we did make one exception to this when we tested two additional models in the meta-analysis of survival. In this case, we were very careful to let the reader know exactly what we had done, so they could make their own judgement as to its appropriateness.

4. Reviewer #4 suggested that we compute time-specific analogues of the projection matrix elasticities as suggested by Nichols and Hines (2002) in order to better understand the components of lambda.

Response: Since we did not compute lambda from Leslie projection models, and provided good rationale for this in the methods section, these computations are not particularly appropriate at this point in time. Besides, this suggestion would seem to mix two very different conceptual approaches to estimation of lambda, which would confuse interpretation of the results without merit. We see no good reason to make the suggested change.

5. Reviewers #1 and 4 suggested that we should add management recommendations that might be appropriate in light of our findings.

Response: We do not believe this would be appropriate, because modification of existing management guidelines for the spotted owl is as much a social-political decision as it is a biological decision. The federal agencies within the range of the northern spotted owl have set aside huge areas in late-successional reserves for spotted owls and other forest species, and it is not at all clear if additional protections on federal lands (or non-federal lands) will reverse the declines that are taking place in the owl population. Given this uncertainty, we are reluctant to start making management recommendations, except to say that we think that management agencies need to consider the population trend data as part of any process in which they evaluate the need for changes in the legal status or management of the owl. We do think that it is appropriate for us to make recommendations regarding changes or additions to current research and monitoring programs to help address the impact of different stressors (e.g., barred owls) on the spotted owl population. We added some sentences to the end of the discussion to address this issue and point out that a Status Review is currently being conducted by the U.S. Fish & Wildlife Service.

6. Reviewer #2 suggested that we add a section on the scope and limitations of the study.

Response: We have already discussed the scope and limitations of the study in multiple

places in the methods, results, and discussion (i.e. number and distribution of the study areas, coarse-scaled nature of the barred owl covariate, sample size for the Marin study area, and spurious results in a couple places). We believe that these limitations are most appropriate where the issues arise, because of the length of the report. We think this is a adequate and much better approach.

7. Reviewer #4 was not convinced that estimates of juvenile survival rates were confounded by emigration from the study areas and suggested that first year survival was possible because undetected emigration is rare.

Response: We disagree with this suggestion strongly. In fact, we go to great lengths in the methods section to explain why we chose not to estimate juvenile survival rates and how this influenced our decision to estimate lambda with the reparameterized Jolly Seber method. We also provide sufficient citations to support our contention that juvenile spotted owls do disperse (emigrate from study areas) at a rather remarkable rate and over some fairly long distances.

8. Reviewer #4 agreed with our decision to use the reparameterized Jolly-Seber model to estimate lambda, but listed a couple of limitations on this approach that he/she thought should be brought to the reader's attention. Specifically, he/she wanted us to point out that (1) the method does not allow you to separate out the relative contribution of different vital rates (survival vs reproduction) to lambda, and (2) the use of lambda RJS conflates the two sources of recruitment (birth and immigration).

Response: We have no problem listing these limitations, but we also think these limitations need to be viewed in terms of the strengths of the RJS method relative to other methods. The problem is that we can't tell owls that were born on the study area from owls that immigrated onto the area. Thus, the limitation is with the data. The same problem applies to the Leslie approach, and the fact that the Leslie approach ignores immigration (or rather assumes immigration is zero) does not fix this problem. So, we would argue that combining immigration and in situ reproduction is a strength of the RJS analysis. We would like to be able to separate these sources, but do not have the data to do so. Therefore, we think it is far better to recognize this limitation and handle it properly than to ignore one aspect of recruitment as the Leslie approach does. The same argument can be made concerning deaths and emigration, but we accept these confounded processes as a combined source of loss of owls from the study area. We attempted to reach the proper balance in the methods section where we discuss the virtues of the two types of analyses in length.

In summary, we believe that the ESA reviews were helpful in improving the manuscript. They confirmed that our analytical and statistical techniques were up-to-date and the report was rigorous, well presented, complete, and objective. We used the review comments to eliminate some sections of the previous draft and add additional sections. They did not prompt us to do any further analyses based on our reasoning above. We believe the report is more concise and clearer than the previous draft and can now be considered a final report to the federal agencies.