

20 August 2007

Paul Phifer, Project Manager  
Northern Spotted Owl Recovery Plan  
U.S. Fish and Wildlife Service

Dear Mr. Phifer:

I received a letter (FWS/R1/RD) from David Wesley requesting my feedback regarding the use of my published research in support of habitat targets within the Draft Northern Spotted Owl Recovery Plan. This letter is in response to that request.

First, a few general comments. In the introductory and closing paragraphs the letter thanks me for my assistance to the Recovery Team and I would like to make it clear that I was never asked to assist the team in any way. During the development of the Plan, I was never asked about the research that was cited extensively in the Plan nor was I requested to provide feedback on the use of that research within the Plan. Thus any assistance I may have offered was indirect and involuntary.

I restrict my comments here to the issues and questions raised in Wesley's letter. This restriction is not to be interpreted as approval of other aspects of the Plan, especially regarding the two Options.

Following are my comments on the specific aspects of the Plan as requested in Wesley's letter.

#### **Recovery Criterion #4**

At this point there is no scientific support for the figures. I address the basis for the provincial percentages (habitat fitness potential) in comments on Appendix D, below. The description of the use of "Biomapper-style" habitat typing is insufficient to evaluate its application. I also could not find any aspect of Criterion 3 that justifies the extrapolation of home range scale analyses to provincial scale targets.

I have several concerns regarding the habitat definitions given. The terms "high-quality" habitat and "suitable habitat" appear to be considered equal in the Plan, but they are not considered so by ecologists. Most wildlife species have a broad range of habitat that might be considered suitable, but the term high-quality is reserved for those habitats within which the highest fitness occurs. The definition given for "high-quality" here (used by 90 percent of spotted owl pairs) seems to more accurately reflect suitable habitat. There are a number of other terms used to describe spotted owl habitat throughout the Plan, and not only are these confusing, but they are often equated with little or no rationale. Terms such as "nesting" habitat or "roosting" habitat are assumed to be identical to more specific habitat definitions within some of the cited research papers. For instance, in Olson et al. (2004), I modeled survival probability as a function of the percent of mid- and late-seral conifer forest in 1500m circles surrounding spotted owl activity centers; in the Plan this is equated to "percent nesting habitat". Although these definitions may be correlated, it is unreasonable to expect they are exactly equal; that is, that 70% of mid- and late-seral conifer forest is the same as 70% nesting habitat, unless nesting habitat is specifically defined as being mid- and late-seral conifer forest as we defined it in our study (and it is not in the Plan).

Differences in habitat definitions are one of several reasons why I stated in Olson et al. (2004) that the results from that research should not be used to write management prescriptions. By all appearances, the Recovery Team ignored this advice and did so anyways in the setting of province-specific percentages. I continue to stand by the statement in the paper and believe that using my research in this manner is wrong.

### **Appendix A, pp. 112-115**

I have only a couple of minor comments on this section on “Habitat Characteristics”. First, it is curious that the one study highlighted in this section is one of the few that is contrary to the majority of evidence regarding habitat characteristics of spotted owls. Second, the statements made relative to the comparison of Dugger et al.’s (2005) results seem to re-iterate concerns I have (further discussed below) that there is a lack of understanding of what habitat fitness potential is and how it is computed.

### **Appendix D**

I have a number of concerns with Appendix D. First, the title implies that the appendix describes what habitat fitness is and it does nothing of the sort. The term used in both Franklin et al. (2000) and Olson et al. (2004) is “habitat fitness potential”, and leaving off the word “potential” is a key error. The measure does not estimate actual fitness but the potential fitness that might be conferred to owls occupying a territory with certain habitat characteristics. The appendix also does not show how habitat fitness potential (also designated as  $\lambda_h$ ) is calculated so that its relationship (or lack thereof) with survival and reproduction, or the more commonly calculated population growth rate ( $\lambda$ ) is not apparent. This, I believe, has led to a number of errors of interpretation throughout the Plan, which further has led to the misapplication of habitat fitness potential as the standard for setting province-specific habitat targets. It is not clear in this Appendix, (and it should be), that habitat fitness potential is a value computed from values of survival and productivity that in turn were calculated from models relating those parameters to habitat measures. None of these numbers were estimated from population data directly. Further, habitat fitness potential is not a measure of population growth rate, but it is a relative measure of how owl pairs might perform based on specific territory characteristics and assumptions that the modeled relationships between habitat variables and survival and fecundity parameters are accurate. There are several important aspects of owl population dynamics included in estimating  $\lambda$  that are not included in the computation of  $\lambda_h$ , including territorial occupancy probability and recruitment.

Aside from these errors of omission, what IS in Appendix D is what I consider a pseudo-analysis. Figure 2 is based on 6 points taken from a figure in Olson et al. (2004) that were selected examples, not a representative sample of owl territories. Further, the values of habitat fitness potential given with the figure were calculated from the habitat configurations in the sample landscapes, thus the exact relationship between the amount of habitat and the habitat fitness potential values is known and no analysis of any kind is needed to estimate it. The shape of the curve in Figure 2 is due to the modeled relationship between survival and amount of mid- and late-seral forest, and the fact that this has a greater influence on the calculated values of habitat fitness potential than does fecundity. Again, a true understanding of how habitat fitness potential was calculate appears to be lacking.

In summary, here are my responses to the 3 questions posed in the letter:

1. Does the draft recovery plan summarize and represent your relevant data and analysis correctly in the sections noted above?

The short answer to this question is “no”. There are many places in the plan where my research results are not interpreted correctly and used in ways that I not only did not intend for them to be used, but specifically warned against in my published paper. I am particularly concerned that my research is being cited as the basis for the provincial habitat percentage targets listed in the recovery criterion, as by no means do my results support these targets.

2. The draft recovery plan proposes to use habitat fitness percentages as targets for the individual provinces across the range. What do you see are the risks or advantages of using the habitat fitness theory to establish habitat targets in relation to achieving the recovery criteria, specifically the population-related Recovery Criteria (i.e., criteria #2 and #3)?

This question re-iterates my contention that the concept of habitat fitness potential is not well-understood by the drafters of this plan and others. “Habitat fitness percentages” doesn’t make sense, even as a shorthand phrase. Even assuming the errors and misconceptions regarding habitat fitness potential are corrected, I see a number of risks to using habitat percentages based on habitat fitness potential measures as targets for individual provinces. First, there is the matter of scale that I pointed out above: there is no reason to expect that habitat fitness potential measured at the home range scale may be directly translated to habitat percentages at the provincial scale. Second, the owl territories for which habitat fitness potential was estimated are not a sample selected from across entire provinces, but come from specific study areas (usually only one per province). Thus they cannot be considered representative of all habitat conditions within the province. Third, there has not been any relationship established between habitat fitness potential and actual owl fitness, even on a small scale. Fourth, the habitat percentages given as targets are absolute values and do not take into account the variation inherent in the estimation process. Standard statistical analyses and common scientific procedures require the estimation and reporting of confidence intervals, which are used to express the precision of the estimates. A value such as 70% might not seem so useful as a target if the estimate it is based on has a confidence interval of 40-90%.

3. The draft recovery plan specifically proposes habitat fitness percentage targets for each province. Given the proposed percentages (...) to what degree do you expect the recovery criterion to be met, specifically the population-related Recovery Criteria ...?

Given the complete lack of established relationships between province-wide “percentage of habitat capable acres”, habitat fitness potential as calculated using

completely different habitat definitions applied at the owl territory scale, and population-level trends in spotted owls, it is impossible to predict whether meeting the habitat targets will lead to either of the recovery criteria being met. One of the greatest errors in the plan is to ignore the importance of establishing these relationships. Thus in my view the habitat targets are completely arbitrary and thus are more likely to not be successful in enabling the recovery criteria to be met than they are.

I further would summarize the major flaws of the Plan, as related to the issues above, as follows:

1. Lack of consistency in habitat definitions.
2. Misuse and misunderstanding of the habitat fitness potential concept.
3. Lack of incorporation of uncertainty estimates in establishing targets.
4. Over-emphasis of some research at the expense of others.

Finally, I reiterate that my comments here were restricted to the areas as requested by the letter I received. They in no way are meant to imply my agreement with other aspects of the plan.

Sincerely,

Gail S. Olson, PhD.  
Research Scientist  
WA Department of Fish and Wildlife  
Olympia WA 98501  
Email: [olsongso@dfw.wa.gov](mailto:olsongso@dfw.wa.gov)