25 June 2007

Dr. Paul Phifer
Project Manager
Northern Spotted Owl Recovery Plan
U. S. Fish and Wildlife Service
911 N.E. 11th Avenue
Portland, OR 97232-4181

Dear Dr. Phifer:

As requested by Dr. David J. Wesley in his letter of 10 May 2007, I am providing my comments on the Draft Recovery Plan for the Northern Spotted Owl (*Strix occidentalis caurina*). I have confined my comments largely as a response to the three questions that Dr. Wesley posed in his letter. These questions referred to Recovery Criterion #4 (pages 32-36 and 48-51), pages 112-115 of Appendix A, and Appendix D (pages 134-136) where some of my previous research work (i.e., Franklin et al. 2000) was used to develop these sections of the plan. Below, I have responded to each of the questions (indicated in bolded italics) that were posed in the letter from David Wesley.

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

I felt that the draft recovery plan misinterpreted the results of Franklin et al (2000) in terms of relevant use of lambda(h), and of the modeling approach used. I have detailed my concerns below under each of those categories.

Misinterpretation of Scale: The draft recovery plan misinterpreted the scale to which the results in Franklin et al. (2000) apply. Franklin et al. (2000) explicitly stated that their results related to the territory scale only in statements such as:

1. page 542 of Franklin et al. (2000): "*This study focuses on the **territory scale**, specifically in terms of macrohabitat (Block and Brennan 1993): the extent and configuration of vegetation stands within territories.*"
2. page 543 of Franklin et al. (2000): "*First, we address whether Northern Spotted Owl survival and reproductive output vary with respect to landscape habitat covariates at the individual **territory scale**.*"
3. Page 578 of Franklin et al. (2000): "*These results are scale dependent in both habitat and landscape extent. In terms of habitat within a **territory scale**, scale is relevant only to discrete habitat patches and not to within-patch variation. In addition, landscape extent in this study is limited to the **territory scale** and not to larger or smaller scales. Therefore, differences (or lack thereof) can only be attributed to the **territory scale**. Other scales such as a home range scale or cluster of territories may produce different results and should be appropriately analyzed.*"

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4. Page 582 of Franklin et al. (2000): *"Here, we use the term habitat in reference to the landscape configurations of mature and old-growth forests at the **territory scale**, which collectively defined the life history traits and habitat fitness potential."*

Thus, the scale to which the results of Franklin et al. (2000) apply are very clearly stated throughout the paper. However, the draft recovery plan did not make this distinction at several levels. First, they introduce Franklin et al. (2000) as having *"shifted the paradigm from considering spotted owl habitat at the stand level to the landscape level"* [pages 36 and 51]. However, in introducing landscape scale in relation to spotted owl habitat, they do not explicitly state at which scale (i.e., the territory scale) Franklin et al (2000) measured habitat metrics. In addition, the draft plan states that *"Recent landscape-level analyses in portions of the Oregon Coast and California Klamath provinces suggest that a mosaic of late-successional habitat interspersed with other seral conditions may benefit spotted owls more than large homogeneous expanses of older forests (Zabel et al. 2003; Franklin et al. 2000; Meyer et al. 1998)"* [page 113 in draft recovery plan]. This statement implies that these conditions can be applied across entire landscapes within the provinces, rather than at a territory scale, on which all of the cited studies in this statement were based.

This misinterpretation of scale subsequently led to inappropriate application of the results of Franklin et al. (2000) for land management options in the draft recovery plan. The draft recovery plan uses as it's foundation under Option 1 *"a network of Managed Owl Conservation Areas located in Washington, Oregon, and California"* [page 15] at 2 levels, MOCA 1 supporting 20 or more pairs and MOCA 2 supporting 1-19 pairs [page 16]. In Option 2, only "habitat blocks" are identified [page 23] with two categories supporting the same numbers as the MOCAs. The draft recovery plan then uses results based on Franklin et al. (2000) and other studies (Appendix D of draft recovery plan) to set the percentage of habitat-capable acres in suitable habitat for the MOCAs [page 32-33] under Option 1 and the habitat blocks [pages 48-49] under Option 2. The application of the results from studies on a territory scale to a cluster of territories in the MOCAs and habitat blocks is a clear misapplication of the scale used in the original studies. The primary reasons why this is inappropriate are detailed further below.

Misinterpretation of Lambda(h):

Franklin et al. (2000) defined λ_H (referred to as lambda(h) in the plan) as the *"fitness conferred on an individual occupying a territory of certain habitat characteristics"* and *"the potential fitness that an individual can achieve if it occupies a particular territory with certain habitat characteristics"* [page 558 in Franklin et al. 2000]. Thus, lambda(h) is an individual measure relative to a defined scale (the territory in the case of Franklin et al. 2000). However, lambda(h) is not necessarily directly comparable to lambda (λ) used as a metric to measure overall population change. As Franklin et al (2000) point out, *"For λ_H and λ to be roughly equivalent, all territories need to be occupied. Therefore, to understand the relationship between λ_H and λ, some measure of occupancy on territories needs to be included in some function that also includes λ_H"* [page 581 in Franklin et al. 2000]. To date, this has not been done and the relationship between lambda(h) and lambda still remains unresolved. However, the draft

recovery plan misinterprets this distinction between lambda(h) and lambda in assigning habitat thresholds to provinces by stating "...*Franklin et al. (2000) and Olson et al. (2003)...found that landscape fitness (lambda$_{(h)}$) fell below 1.0 (a stable population) with greater than 80 percent nesting habitat and adult spotted owl survival rates were decreasing in landscapes with greater than 80 percent nesting habitat*" [pages 36 and 52 in draft recovery plan]. In this statement, lambda(h) is inappropriately used as a population, rather than a territory-scale individual, metric and, again, scale was misused (see previous comments). An additional reason why lambda(h) does not apply beyond a territory scale is because immigration and emigration are not included in the computation of lambda(h). At scales of multiple territories (e.g., in the MOCAs and habitat blocks proposed in the draft recovery plan), additional landscape considerations need to be included to account for dispersal of young of the year and movements by territory holders. This landscape matrix between territories was not included in the territory-scale lambda(h) estimated by Franklin et al. (2000) and was the reason why we stated on page 578 of Franklin et al. (2000): "*In addition, landscape extent in this study is limited to the territory scale and not to larger or smaller scales. Therefore, differences (or lack thereof) can only be attributed to the territory scale. Other scales such as a home range scale or cluster of territories may produce different results and should be appropriately analyzed*". To date, these analyses have not been conducted and, therefore, application of lambda(h) from the territory scale to blocks containing multiple territories is not appropriate.

Misinterpretation of Modeling Approach used in Franklin et al (2000):

In Appendix D, there are number of problems with the approach used to develop habitat-capable acres used in Recovery Criterion 4 under both Options 1 and 2. First, the regression of adult survival and lambda(h) against percent nesting habitat is not a correct analysis because:

1. The territory-specific adult survival was not strongly associated with just the amounts of older forest within territories but by the amounts of *interior* (emphasis mine), or core, older forest in addition to the amount of edge between older forest and other vegetation types (see Table 7 in Franklin et al. 2000). Interior older forest was the amount of older forest 100 meters from an edge and is very different than just the total amount of older forest within a territory. The model estimating survival based on just amounts of older forest was not well-supported and had only 3% of the weight in the model set (as opposed to 42.7% for the best-supported model described above; see Table 7 in Franklin et al. 2000).

2. The survival values used in the draft recovery plan were originally estimated based on the best-supported model in Franklin et al. (2000). In the draft recovery plan, these estimates were then regressed again on amounts of older forest in the draft recovery plan to develop optimal percentages of habitat capable acres. This was inappropriate because it ignores the model selection approach used in Franklin et al. (2000), which found that just amounts of older forest alone within spotted owl territories did not explain variation in survival nearly as well as amounts of interior older forest and edges.

These same problems carry through in the analysis of lambda(h) in the draft recovery plan because lambda(h) is a function of the survival estimates from the analysis described above. As noted in Franklin et al. (2000), territory-specific lambda(h) for northern spotted owls can be explained as follows: "*Survival seems positively associated with some level of interior mature and old-growth coniferous forest and the edge between those forests and other vegetation types, whereas reproductive output is enhanced by convoluted edge with little interior habitat. Thus, there is evidently a trade-off in potential need for interior habitat and potential need for ecotones within a territory. This trade-off was expressed in estimates of habitat fitness potential in Northern Spotted Owls, where high fitness balanced having both core owl habitat for maintaining high survival and having some mosaic of older forest and other vegetation types for maximizing reproduction and maintaining high survival. This mosaic was expressed as small patches of other vegetation types with convoluted edges, dispersed within and around a main patch of mature and old-growth forest*" [page 579 in Franklin et al. 2000]. Thus, the analysis provided in the draft recovery plan ignores this relationship between interior older forest and edges, and the subsequent stand configurations within territories, that defined high estimates of lambda(h). Instead, the draft recovery plan ~~was~~ focused on an incorrect relationship with older forest alone. Although I initially provided some of these analyses to the recovery team at their request, I noted both verbally and in writing that these analyses were flawed for some of the above reasons.

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).

I have a number of concerns in the use of the habitat fitness percentages in the draft recovery plan. First, the draft recovery plan misinterpreted a good deal of the information in Franklin et al. (2000) and the other studies in trying to apply these results to recovery criteria. I detailed these concerns in the first section of this letter. Thus, the targets provided in the draft recovery plan have serious flaws. Second, the study detailed in Franklin et al. (2000) was an observational study and the results should be considered more as explicit hypothesis that should have been subsequently tested by large-scale experiments. As noted in Franklin et al. (2000), "*Although these levels of uncertainty do not negate the results of this study, our results should be considered more as working hypotheses from an observational study that require further experimental verification. Clearly, part of the value of this work is in reducing the number of potential landscape configurations that might affect Northern Spotted Owls in this area to a small subset, which then can form the basis of field experiments*" [page 578 in Franklin et al. 2000]. To date these experiments have not been conducted, although they could have been on private lands.

I think that the use of habitat fitness theory is an integral part of managing any wildlife populations. Habitat is critical for maintaining wildlife populations and viable wildlife populations are dependant on habitat quality. One measure of habitat quality is habitat fitness, as used with northern spotted owls in the studies by Franklin et al. (2000),

Olsen et al. (2004), and Dugger et al. (2005). However, these studies encompass only a few ecological provinces within the owl's range and data is specifically lacking for most of the provinces. As pointed out above, the results of these studies are preliminary until further experimentation is conducted. Although there was an attempt to include this in the draft recovery plan under Recovery Action 32 (*Conduct experiments on forest management outside of MOCAs to better understand the relationship between habitat and spotted owl fitness, including the effects of fire and silviculture on suitable habitat and spatial pattern*), this action was given the lowest priority classification (priority 3 – all other actions deemed necessary to meet the recovery objectives) [pages 78 and 90 in the draft recovery plan]. Given the importance of habitat to wildlife populations, such experiments should receive a priority classification of 1, especially because of the long time frames ($\geq$10 years) to obtain meaningful results.

Thus, I have concerns that the results on habitat fitness from studies on northern spotted owls are incorrectly used in the draft recovery plan, and even if they were used correctly, there is no strong mechanism for validating their use in management in the near future.
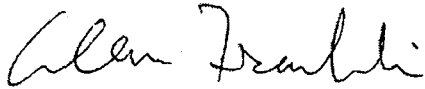
The draft recovery plan specifically proposes habitat fitness percentage targets for each province. Given the proposed percentages (see pages 33 and 49) to what degree do you expect the recovery criteria to be met, specifically the population-related Recovery Criteria (Recovery Criteria #2 and #3)?

Based on my concerns outlined previously in this letter, I don't think that the habitat fitness percentage targets for each province are correct. Given this, it is impossible to say what degree the recovery criteria will be met because spotted owl population trends are implicitly linked with habitat conditions. One underlying factor that can affect spotted owl populations is weather, especially in combination with habitat quality as defined by habitat fitness. Franklin et al. (2000) found a relationship between habitat quality and climate where apparent survival declined 7.1% in good habitat as the climate conditions worsened, but decreased 17.5% and 26.3% in medium and poor habitats, respectively (see pages 575-576 and Figure 11 in Franklin et al. 2000). As noted in Franklin et al (2000), "*These results indicate that individuals in good habitat had a much slower decline in survival as climatic conditions deteriorated than did individuals in poorer habitats. Thus, high habitat quality, as defined in this study, buffered the survival of territory occupants from the negative effects of climate*" [page 576] and "*This also suggests that habitat maintenance is essential at landscape scales because excessive loss of key landscape habitat components, such as mature and old-growth forest, can exacerbate the effects of unfavorable climatic conditions on survival*" [page 582]. Thus, if poor measures are used to define and manage spotted owl habitat (i.e., are inadvertently managing for poor habitat quality), the underlying and uncontrollable effects of climatic variation could have severely detrimental effects on the population. Thus, habitat management for spotted owl populations proposed in the draft recovery plan is largely uncertain because, at the least, the measures of habitat fitness (and hence habitat quality) proposed in Franklin et al. (2000) were incorrectly applied to province-scale measures.

In conclusion, I think the recovery team should re-evaluate the use of habitat fitness in developing provincial targets and consider more rigorous modeling approaches to develop habitat fitness maps, similar to those developed by Zabel et al. (2003) for spotted owl occupancy, rather than simple threshold targets. In addition, a more prudent approach would include a stronger section on adaptive management experiments that tests the empirical models for habitat fitness in spotted owls from the observational studies. Although the effects of barred owls received considerable attention in the draft recovery plan, consideration of habitat is still a primary and necessary requisite to recovering spotted owl populations and should be treated more rigorously in the final recovery plan.

Please let me know if you have any additional questions.

Sincerely,

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