

Review of 2007 Northern Spotted Owl Draft Recovery Plan

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A. Requested Comments on specific sections:

1). Recovery Criterion #4 (pp 32-36 and 48-51)

I don't believe Recovery Criterion #4 sets standards for habitat quality and quantity that if met, will aid in the recovery of the Northern Spotted Owl. This is because the current goals for the percentage of suitable owl habitat in each physiographic province were based on faulty assumptions, and inappropriate use of statistical methods and published scientific data.

First, a sentence on Pg. 36, paragraph #1 suggests Franklin et al. 2000 and Olson et al. 2004 are justification of the province level management being proposed by Recovery Criteria #4 for both options 1 and 2. However, this is an incorrect use of science, as all the recent modeling work done by Franklin, Olson and myself (Dugger et al. 2005) model demographics in relation to habitat characteristics on a territory level – NOT a province level. Franklin et al. (2000) and Olson et al. (2004) in particular, caution against extrapolating these studies to larger landscape scales. Therefore, extrapolating these studies to management on a province level is NOT supported by recent modeling efforts. The spatial juxtaposition of habitat types within territories is not necessarily consistent across stands or provinces and likely has important effects on NSO distribution and demographics within these larger spatial scales. Thus, just requiring a particular percentage of a MOCA or "habitat block" be in "high quality owl habitat" does not necessarily ensure the spatial arrangement of owl habitat such that 20 pairs can be supported. We currently don't know how the territory-level models we (Franklin et al. 2000, Olson et al. 2004, Dugger et al. 2005) have generated reflect overall province level habitat configurations.

In addition, λ_h developed by Franklin et al. 2000 and used by Olson et al. (2003) and Dugger et al. (2005) is NOT landscape or territory-specific fitness, but rather "habitat fitness potential" as defined by Franklin et al. (2000) "can be viewed as the potential fitness than an individual can achieve if it occupies a particular territory with certain habitat characteristics". Habitat fitness potential (λ_h) is NOT an absolute or inherent demographic parameter associated with a specific territory, but rather a theoretic measure of the kind of "fitness potential" a particular individual with survival and reproductive parameters estimated from specific habitat covariates might experience on a territory with these characteristics. The issue here is that it's survival and reproductive success that are modeled for individuals on specific territories – these are the parameters that are being modeled and estimated. Then these estimates are

used to generate theoretical habitat fitness potentials for these same individuals on territories of specific habitat configurations. There is NO direct link between λ_n and habitat – but rather survival and reproduction are the demographic parameters that have been linked directly to habitat characteristics on the territory level and habitat fitness potential is a reflection of these estimates of survival and reproduction. The development of the graphs on pg. 134-136 of Appendix D were completely inappropriate uses of published data (see additional comments below #3) and should not have been developed or used to set Recovery Criterion #4.

In addition, it seems clear the recovery team chose to pick and chose the scientific literature that most strongly supported the role “edge” habitat or “non-old/mature” forest plays in owl demographics, while ignoring my work (Dugger et al. 2005) from a similar area in southern Oregon. Dugger et al. (2005) suggests “edge” and “intermediate” habitat types are not a factor, but rather there is a linear and log-linear increase between reproduction and survival respectively, in response to increased older (mature) forest; yet this research was not considered when developing Recovery Criterion #4. See additional comments regarding this issue below (#2) – but this kind of selective use of the scientific literature is inappropriate and puts the legitimacy of the entire plan in question.

The end result of these faulty assumptions is that the recovery criterion associated with conserving NSO habitat has quite possibly been set much too low. Federal lands will play a primary role in achieving recovery of the NSO throughout its range, so the MOCAs or “habitat blocks” will be the primary source of suitable habitat available over the long-term to NSO populations. Therefore, these pieces of habitat should be managed to maximize NSO nesting and roosting habitat. If only 80% of MOCAs in the California Coast are required to have only 50% of their area in “high quality habitat”, then functionally, the **minimum** amount of habitat required is actually less than what is suggested in Table 33 and 49 (see # B3 below).

In addition, I don't even think you can apply this kind of criterion to option 2; an option that does not map specific protected, management areas for NSO. The idea that you can manage the kinds of percentages outlined above on a shifting landscape and not end up with net habitat loss is an unreasonable expectation. Mistakes, miscommunications, and the application of different “rule-sets” by different federal agencies regarding the management and delineation of “habitat blocks” will not facilitate NSO habitat conservation or recovery, but rather result in a net reduction of high-quality owl habitat. The whole notion that a recovery plan includes multiple “options” for a species recovery is ludicrous, particularly when one of the options does not actually define specific criteria for habitat conservation. Referencing the use of some unknown “rule-set” to manage unmapped, un-delineated blocks of owl habitat is inappropriate and will not facilitate recovery of the species.

2). Pages 112-115 of Appendix A

The following comments are in regards to pg. 113, paragraph 2. First, the work of myself and my coauthors (Dugger et al. 2005) is a “recent analyses” that does **not** support the idea that a “mosaic of late-successional habitat interspersed with other seral conditions may benefit spotted owls more than

large, homogeneous expanses of older forest” and this should be noted clearly here in contrast to the other work cited.

In addition, in reference to Dugger et al. (2005) the Recovery Plan states “The authors concluded that they found no support for either positive or negative effects of intermediate-aged forest” (pg. 113) on NSO demographics. The current wording of this section suggests we may have concluded something that wasn’t supported by our data, which is not true. The lack of support for any direct positive effect of edge habitats on owl demographics were as strong as those we noted for the effect of old-forest on survival and reproductive success and non-habitat on survival; yet the recovery team opted to ignore our findings.

Demographic parameters (survival and reproduction) for our Medford study area **are** low compared to many other NSO study areas in a comparable part of the species’ range - that is true and correctly presented. However, “habitat fitness potential” is NOT an intrinsic demographic parameter of NSO populations, but rather a theoretical interpretation of the effects of habitat characteristics on survival and reproductive success at a territory-scale level. Thus, “low habitat fitness potential” did not have any effect on the “results” of this study, but rather the results of our demographic analysis (estimates of survival and reproductive success in relation to climate and habitat) resulted in estimates of “low habitat fitness potential”. Habitat fitness potential is developed from estimates of survival and reproductive success generated from models linking demographics to habitat characteristics – λ_h is NOT a stand-alone demographic parameter and was developed solely as a way to theorize how habitat characteristics within territories can interact via demographic rates to affect NSO populations.

The low demographic rates observed in our study may be due to the incredibly fragmented, landscape mosaic present in this study area. These landscape patches are comprised of a variety of ownerships and this area has endured higher levels of harvest than areas studied by Franklin et al. (2000) and Olson et al. (2003). This may be why “edge” habitats as measured in a way similar to Franklin et al. (2000) and Olson et al. (2003), were not important in our models – the landscape is full of “edge”, it isn’t limiting and in fact contiguous amounts of old forest were a far greater need for breeding owls. This work taken in conjunction with Franklin et al. (2000) and Olson et al. (2004) suggests variation in the importance of edge habitats to NSO and it’s likely our results (Dugger et al. 2005) will be most applicable to highly fragmented, habitats with checkerboard ownership configurations, and possibly for more northern provinces where wood rats are not as important to the diet as they are in southern Oregon and Northern CA.

3). Appendix D (pp 134-136)

This section is particularly troubling because data from both Franklin et al. (2000) and Olson et al. (2003) have been seriously misused. The problems with Appendix D are severe, and this section can not be used to justify Recovery Criterion #4.

I find the following items most troubling about this section:

- a. How were figures D1 and D2 developed? I know that neither Franklin et al. (2000) nor Olson et al. (2003) contained the raw data that would allow the authors of this recovery plan to generate these kinds of figures. Were these figures (D1 and D2) somehow “guesstimated” by visually converting landscape habitat characteristics from Figure 10 (pg. 573) in Franklin et al. (2000) and Figure in Olson et al. (2003)? The “equation” lines on this graph suggest some sort of analysis has been done developing a relationship between habitat characteristics and λ_h , but that just isn't the case. Where did these “lines” come from? Were they based on some sort of simple regression of λ_h values vs. guesstimated habitat percentages? If so, this is not a valid statistical approach and a completely inappropriate use of someone else's data. Even if it were appropriate to use habitat fitness potentials to set stand management goals (see below), these adapted graphs do not represent the true relationship between territories with specific λ_h and the habitat characteristics of those territories. These graphs represent “guesstimates” of the relationship between habitat characteristics and habitat fitness potentials of a very small subsample of the entire data set used in these analyses. In addition, it is survival and reproductive success that are directly linked to habitat characteristics at a territory level – NOT λ_h . These figures are misleading and an incredible misuse of data.
- b. Habitat fitness potentials (λ_h) are not appropriate parameters on which to base province level management actions. This precludes them from being useful for directly guiding recovery goals throughout the NSO's range. As noted above, habitat fitness potentials are developed for each territory from estimates of survival and reproductive success generated from models linking demographics to habitat characteristics. This is NOT a stand-alone demographic parameter, but was developed solely as a way to theorize how habitat characteristics within territories can interact via demographic rates to affect NSO populations. Habitat fitness potentials are appropriate only at the scale of the territory and NOT at the landscape level. Proportions of appropriate habitat within NSO territories that result in high reproduction and survival, can not necessarily be scaled up to work at the landscape level. The demographic models created by Franklin et al. (2000), Olson et al. (2003) and Dugger et al. (2005) relate survival and reproductive success to habitat characteristics at the territory scale, not to province or even stand level characteristics. The same proportion of specific habitat types within territories can be reflected in many different spatial configurations as evidenced by the Figures in Franklin et al. (2000), Olson et al. (2003) and Dugger et al. (2005). We currently have no quantitative evidence that simply scaling those percentages and even those spatial patterns up to the level of the stand or landscape results in the same relationships observed at the territory level – thus the use of relationships developed at one scale for managing at another scale is not appropriate at this time.
- c. Franklin et al. (2000), Olson et al. (2003) and Dugger et al. (2005) used habitat characteristic data developed from digitized aerial or satellite photo data which can't be compared directly to habitat as characterized using the Biomapper approach. The approach of Franklin et al. (2000),

Olson et al. (2003), and Dugger et al. (2005) characterize habitats directly from remotely sensed data. Biomapper involves a different process, where suitable or “high quality” habitat is characterized using the pattern of NSO use in a landscape. Both techniques may provide useful information for modeling habitat characteristics of NSO in relation to demography, however, using the results of modeling efforts associated with the work of Franklin et al. (2000), Olson et al. (2003) or Dugger et al. (2005) and applying those results to a landscape characterized by a Biomapper definition of owl habitat has not been shown to be appropriate. Recovery Criterion #4 uses the results of Franklin et al. (2000) and Olson et al. (2003) to justify the amounts of high quality habitat required in MOCAs or “habitat blocks” as delineated by Biomapper, but there is currently no support for this kind of generalization.

- d. Since both Franklin et al. (2000) and Olson et al. (2003) and Dugger et al. (2005) for that matter all describe relationships between NSO demography and habitat characteristics of NSO territories in relatively similar landscapes, it’s unclear how these results informed habitat threshold percentages for some of the other provinces with very, different habitat characteristics, like the Olympic peninsula or the eastern cascades (WA and OR). There is currently no evidence that “edge” habitats (non-forest, young forest) are important to the demographics of owls in Washington or the eastern Cascades of Oregon and making assumptions about the positive effect of “edge” based on current diet studies in some of these provinces is unwarranted. Without an understanding of whether these prey sources are actually “preferred” and represent a high quality foraging habitat with high demographic rewards (i.e., positive effects on survival and reproduction) in these provinces must be answered before you can generalize habitat relationships from a couple provinces to all others. In other words, the assumption that edge is preferred or desirable in some of these provinces just because wood rats are included in NSO diets, isn’t necessarily a valid conclusion.
- e. In general, here and throughout this document, habitat definitions are not defined. What is “nesting habitat” in Figure D1? What is “percent nesting” in figure D2? How do the two types of habitat from these different studies compare? Are habitat characteristics across studies comparable? There is nothing in this entire document that tries to standardize or define what is considered NSO habitat. Statements are made referring to “high quality” or “nesting or foraging” habitat, but how is this characterized at the stand level?

B. Requested comments on specific questions:

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

The exceptions I have to the use and interpretation of the data and conclusions presented in Dugger et al. (2005) are clearly outlined above in response #2.

However, a bigger issue isn't how the recovery team used my work, but rather how they ignored it in a variety of places (Recovery criteria #4, Appendix D, etc.). Generalizations from two studies in similar province types (Franklin et al. 2000, Olson et al. 2003) to all provinces in the range of the NSO are made throughout this document. However, a 3rd study, conducted using identical methods and in a province similar in habitat characteristics to those studied by Franklin et al. 2000 and Olson et al. 2003, which contradicts the importance of edge habitat to NSO demographics, was largely ignored by the recovery team. It is clear the recovery team was aware of our study (pg. 113), so a deliberate decision was made to overlook our findings. Given our methods were identical to those used by Franklin et al. (2000) and Olson et al. (2003), there is no justifiable reason for excluding our work. I have to conclude it was the fact that we observed a strong relationship between increased amounts of old forest and demographic parameters and no support for edge, or intermediate habitat types that caused the recovery team to exclude our work. The Recovery Team's approach which does not include ALL the best available science puts the entire recovery plan process and the resulting document into question.

This recovery plan presents the positive effects of edge habitat on NSO reported by Franklin et al. (2000) and Olson et al. (2003) as if this is a general relationship that can be supported throughout the species' range. Our work (Dugger et al. 2005) provides evidence that this is not true, and that information must be considered when setting recovery goals and criteria. In addition, Dugger et al. (2005) is the only study to date that has specifically investigated the effects of intermediate forest types on NSO demography, yet these findings were not used to develop recovery criteria. This is a serious misuse of the best available science and detracts from the overall credibility of the entire recovery plan.

2). What do you see as the risks or advantages of using the habitat fitness theory to establish habitat targets in relation to achieving the recovery criteria.....?

In my opinion, there are no advantages to using habitat fitness theory to establish province-level habitat targets in relation to NSO recovery theory. Please see my summary of Appendix D above (#3d in particular) for my concerns regarding the use of habitat fitness potentials to manage NSO landscapes. I do not believe λ_h should be used to establish habitat targets for NSO recovery.

3). Given the proposed percentages (pg. 33&49) to what degree do you expect the recovery criteria to be met.....?

Given the proposed habitat percentages are based on flawed use of scientific data, it's unlikely these percentages are adequate to achieve recovery criteria. Habitat fitness potentials generated at a territory scale are not applicable to management at the province level (see #3b above). In addition, the percentages proposed by the Recovery Plan were based on the potentially flawed conclusion that "edge" or non-old forest foraging areas are important throughout the NSO range; however, the scientific evidence (i.e., Dugger et al. 2005) suggests this effect may be variable, even in the southern portion of the species' range (see #3d above). Finally, since only 80% of either MOCA's or "habitat blocks" are required to meet the listed percentages, the minimum total amount of high quality habitat (i.e., old or

mature forest) for owls can actually be substantially lower than what is suggested in the Table on pg. 33 and 39. Thus, the plan is misleading in the actual, functional amount of high-quality habitat that will be conserved (at a minimum) within MOCA's or habitat blocks. Since these values are not supported by science, it's difficult to be sure what the overall effect will be on populations, but less habitat will not aid in NSO recovery.