

March 11, 2005

To: Whom It May Concern

From: Joseph Lint, Spotted Owl Module Lead, Interagency Regional Monitoring Team

Subject: Response to comments from three blind peer reviewers and one federal agency management reviewer for the monitoring report entitled “The Northwest Forest Plan- the first ten years (1994-2003): Status and trend of spotted owl populations and habitat.”

Three blind peer reviews of the draft ten-year spotted owl monitoring report were conducted through a contractual agreement with the Sustainable Ecosystems Institute, Portland, Oregon. Also, review comments were received from Region 5 of the Forest Service too late to be included in the November, 2004 reconciliation response to other federal agency management review comments. Responses to these four sets of comments are included in this reconciliation letter.

I have reviewed the comments that were submitted, and made changes to the manuscript, as needed. The comments resulted in primarily clarification edits and text additions to the methods and discussion sections of several of the chapters. Several specific comments did result in the expansion of the discussion in the Monitoring Program Needs chapter and revision of portions of the methods section in the Habitat Status and Trend chapter and the Predictive Model portion of the Related Research Chapter. None of the comments suggested the need for or prompted any re-analyses of the data presented in the report.

The reviewer comments were extracted verbatim from the reviewers’ comment letters and assigned a comment number for response purposes. Our responses (in bold print) follow each comment.

Federal agency management review

John C. Robinson, USFS-R5 (comments were received 11/22/04)

Comment #1.

For the more lengthier chapters (i.e., the first two chapters), an **Abstract** and (optionally) a “**Summary of Key Findings**” would add significant value to the reports, especially for those readers who want to cut to the chase.

Response to #1

An Abstract and a Summary chapter were added to the report.

Comment #2

Figure 2: I noticed that Figure 3 from the “Status and Trends of Late-Successional and Old Growth Forests” report had a color version of the physiographic provinces. I would recommend using that same figure here in the owl report as part of Figure 2 (which currently only appears in gray-scale black-and-white text).

Response to #2

See response to comment #3

Comment #3

Which brings me to my next comment: to the extent possible, all of these various monitoring reports should share resources, information, and visual formats to the maximum extent possible such that:

- Figures or Tables that display the same information are created once and then used in each report, as appropriate;
- Information that expresses a given fact is consistent, both throughout a given report; and between reports; and
- Tables and Figures should be formatted similarly throughout all reports so that a reader who reads multiple reports quickly becomes used to the format and style that is used thus making the format and style of the report essentially transparent for your audiences.

Response to #2 and #3

Tables and figures that were common among the modules were standardized. common facts were used consistently among the modules and tables and figures will be formatted similarly among the modules in the final publication.

Population Status and Trend

Comment #4

Cover Page: I’m sorry, but I know that Joeseph Lint does not look like a Spotted Owl. Please place a superscript 1 by Joe’s name and footnote his credentials at the bottom of the page. Then place a caption under the photograph to identify the date and photographer who took this picture.

Response to #4

The cover page was changed, there is no picture on the page and Mr. Lint’s name and job description information are in the correct location on the page.

Comment #5

There are times when “Anthony et al. (2004)” are cited multiple times within a paragraph. Rather than repeating the entire reference, you may find the use of “op. cit.” a more reasonable alternative.

Response to #5

No changes were made to the citation format used.

Comment #6

Bottom of page 10: It is stated, “The survival effects noted were due to geographic region and time.” This is not very clear. Should we explain this in more detail now; or wait for better explanations on the subsequent pages of the report?

Response to #6

The text was edited to provide examples of the geographic regions used in the analysis.

Comment #7

Page 17: it is stated, “Habitat is a key element in the management of Spotted Owls, but it may not be the primary factor affecting population stability in either the short or long term. The Plan is maintaining and restoring habitat, the complication is that owl population response is likely being driven by a combination of factors, not just habitat.” One argument that I believe has been omitted in this paragraph is that it may simply be too soon to detect the benefits of implementing the Plan using population demographic data – a concept that expresses the idea that the Plan is performing better than we think but that it is too early to see the results of that performance.

Response to #7

The text was edited to reflect this additional concept from the reviewer.

Comment #8

Page 18: “New partners, old partners with new roles, new discussions and new initiatives may be needed to rekindle the certainty of a decade ago that habitat maintenance and restoration will lead to successful recovery of the Spotted Owl.” I would argue here that we simply don’t want to rekindle the same mind-frame we had a decade ago as much as we want to **discover the mix of habitat maintenance & restoration activities in conjunction with how we alter and adapt our management approach as we identify new (or learn more about existing) stressors on the ecology of the Spotted Owl.** Bottom line: I think we all agree that the successful recovery of the owl will not rest solely on implementing habitat maintenance and restoration as originally envisioned by the Plan. Instead, identifying the other important ecological stressors (and determining how to best adapt to those stressors) will be the process that will likely emerge as a key component of the successful conservation and recovery of the Northern Spotted Owl.

Response to #8

The text was edited to reflect the thought expressed by the reviewer.

Habitat Status and Trend

Comment #9

On page 57, the reference to the other report is listed as “Moeur et al. (2004)” but described more fully on page 64 as a 2005 publication. These cross-references need to be consistent.

Response to #9

Text was edited to provide the correct citation of Moeur et al. (in press).

Comment #10

Page 56: reference is made to Lint et al. (1999) when describing the concept of self-sustaining owl populations on land areas that were at least 60 percent owl habitat. Was this a finding attributed to Lint et al. (1999), or to some other researcher’s work that was cited by Lint et al. (1999)? If the latter, then the underlying researcher/publication should be cited here.

Response to #10

The statement was an expectation stated by Lint et al.(1999). The text was edited to clarify that it was an expectation and not a finding.

Comment #11

Last paragraph in report on pages 58-59: this entire section could be better cross-referenced to the Moeur et al. (2004) report, which has a more thorough discussion about the effects of fire on habitat.

Response to #11

Section was cross-referenced as suggested by the reviewer.

Related Research – Predictive Models and Barred Owls

Comment #12

General comment: There are many references to the effect of Barred Owls made in these various reports – yet I don't think any of these references make comparisons to the Mallard – American Black Duck interactions in the eastern United States. Thus the real impact of the Barred Owl I don't think is clearly explained to the reader. For example, in the case of the Mallard – Black Duck example, a phenomenon called “genetic swamping” was used to describe the impact of the more numerous Mallard cross breeding with American Black Ducks and thereby diluting the genetic purity of the latter species. Could (or is) the same thing happening to the Spotted Owl as a result of the Barred Owl's recent invasion of the Pacific Northwest? Or is the primary impact of Barred Owl limited to the physical displacement of Spotted Owls? In my opinion, the displacement of Spotted Owls combined with “genetic swamping” by Barred Owls is a far more serious impact that displacement alone and should be more explicitly explained to the reader if indeed both effects are taking place.

Response to #12

This suggested discussion was beyond the scope and depth of the discussion the author intended for the barred owl in this report. A more extensive discussion of barred owl/spotted owl interaction is offered in the chapter on the Assessment of the Potential Threat of the Northern Barred Owl in the scientific evaluation prepared for the Fish and Wildlife Service by the Sustainable Ecosystems Institute. We used information from that report to frame the discussion section of our report.

Comment #13

Page 1 (Predictive Model Development): In the first paragraph of the Introduction, reference is made to the vision that Lint et al. (1999) originally had for shifting toward a reliance on habitat monitoring using predictive models to estimate occurrence and demographic performance of Spotted Owls. Are there any dissenting or opposing viewpoints to this vision (either then in 1999 or at the present time) that are worth mentioning here? Or is there unanimous support for this vision among the various Spotted Owl scientists?

Response to #13

See Comment # 1 from blind reviewer #2 and the response in this letter.

Comment #14

Pages 11: I find it interesting that many of the owl researchers have recently begun to talk about the owls having greater productivity in even rather than in odd years. Would one use this factoid, then, to predict that 2014 will be a better year for owl production than 2015? I would hope not! I would like to see owl researchers, beginning with this report, to begin expressing this concept more along the lines that: “Age, in general, had a positive effect on productivity; and productivity of all owls tends to follow a 2-year cycle wherein it is greater in one year, lower the next year, and followed by an increase again in the third year.” (For me, it is the cycle that is most important; not whether a good productivity outcome occurred in a year with an even rather than an odd number - jcr).

Response to #14

The term “odd-even” was used by the authors of the demographic report to describe the cycle of productivity they noted and we are merely citing their results. It is not a method to predict productivity.

Comment #15

Page 15: the development of reliable predictive habitat models [for] the demographic parameters discussed in this paper is described as “bleak”. And then no further discussion is offered! We owe our readers more information on what this statement means. For example, are we going to spend another 10 years attempting to develop these predictive models (even though the outlook is bleak)? Or are we going to focus more on using habitat information to predict occupancy and abandon the idea of using habitat to predict the rate of population change? Or are we going to focus on something entirely different than the two possible outcomes I just mentioned? We need to be clear about where our immediate future will lead us. To sum up millions of research dollars by saying that the possibility of achieving our ultimate objective is “bleak” and then not telling your stakeholders what you plan to do about that would be the ultimate insult to your readers, some of whom may have been responsible for funding this very study.

Response to #15

Text in the Predictive Model chapter was revised and expanded to address the concerns of expressed by the reviewer. See Comment # 1 from blind reviewer #2 and the response.

Emerging Issues

Comment #16

Page 1, third paragraph: when we state that “the first cases of the virus were only recently recorded in the range of the owl”, what cases are we referring to? Cases of the virus in humans; cases of the virus in owls other than the Spotted Owl; cases of the virus in animals other than owls; or all three of these scenarios?

Response to #16

The text was edited to reflect the detection of West Nile virus cases in wild birds other than the spotted owl.

Monitoring Program Needs

Comment #17

Does the chapter by Courtney and Franklin (2004) provide budget estimates for the six monitoring needs that have been identified here? If so, we should mention on this page that costs for the expansion of the monitoring program to meet any or all of these new monitoring needs are tabulated by Courtney and Franklin (2004)

Response to #17

No cost information was provided by Courtney and Franklin (2004). They also stated “we will not make recommendations on whether new or existing research and monitoring should be funded and carried out.”

Comments from Blind Peer Reviewers

Peer Reviewer #1

Comment #1

It is unfortunate that the sources from which vegetation data were obtained in California were different (and coarser scale) than the source used for Oregon and Washington. The differences in modeling approaches that were required seemed to present difficulties and the “coarser nature of the California polygons may have affected model output” (pg 5, line 17). Edge-mapping issues between adjacent administrative units (e.g., between National Parks and National Forests) also need to be resolved. As this program moves forward I encourage the entities involved to develop solutions to these and other standardization issues so that vegetation mapping can be as accurate as possible. I believe that every effort should be made to minimize possible sources of error, since so much depends on the accurate assessment of spotted owl habitat and it change over time.

Response to #1

A chapter on information management issues is included in the ten-year monitoring report to identify the issues reiterated by the reviewer and to recommend solutions so they are not issues in the next monitoring period.

Comment #2

The authors quantified habitat changes due to stand-replacing events (i.e., wildfires and timber harvest), but acknowledge that the effects of partial-cut harvests and lower intensity wildfires could not be measured (pg 48, line 7). Efforts should be made in the future to explore ways to quantify the effects (either positive or negative) of these events.

Response to #2

It is hoped that by the next report, we will have a spatially-explicit tracking system for management activities. Partial harvest will be a part of the information in the system and should provide opportunities for better quantifying effects on owl habitat. The assessment of lower intensity fires may also improve as our tracking of fire events evolves and improves.

Comment #3

Future monitoring efforts both inside and outside of reserves should explicitly track and report the net balance of gains in habitat suitability through “in-growth” and losses to timber harvest, wildfire and other factors. Clearly, it is the net gain in the amount and spatial array of suitable

habitat in and out of reserves that is of importance in monitoring the recovery of suitable habitat for Northern Spotted Owls.

Response to #3

We concur and will make a concerted effort to track and report gains through in-growth of forest. Our reporting of baseline conditions across all habitat-capable lands sets the stage for being able to detect shifts in conditions over time.

Comment #4

I was confused by a statement made at the beginning of paragraph 3 on page 4 (line 12), “Non-juvenile movements were similar, but the instances of movement between reserve blocks was lower; only 2.9% of all non-juvenile records.” It is not clear to me what comparison is being made here. From Table 1, it appears that the movement paths of non-juveniles and juveniles differed both in terms of the origin-destination of habitat block and the mean distances traveled, and that non-juveniles consistently traveled shorter distances than juveniles (which was confirmed in paragraph 4 (page 4)). I recommend that you clarify your statement regarding non-juvenile movements at the start of paragraph 3 (page 4).

Response to #4

The statement was edited to clarify the statement on movement of non-juvenile owls.

Comment #5

The author suggests that Barred Owls are “not an issue best considered within the confines of the habitat-based Northwest Forest Plan” (page 23, line 2). While this may be true from a bureaucratic perspective, I believe the population status and trend of Barred Owls within the historic range of the Northern Spotted Owl may have a significant impact on spotted owls. Therefore, it is critical that their status be monitored to the extent necessary to document their impact. Without these types of data, it may prove very difficult to develop models that reliably predict spotted owl occupancy and/or productivity.

Response to #5

Discussions on future monitoring under the Plan will include barred owls. The section on monitoring program needs in our report was edited to emphasize the need for barred owl information.

Comment #6

This brief section reviews some monitoring needs and refers readers to an “Information Needs” chapter in Courtney and Franklin (2004). I would have preferred to see a more developed section that focused on the current and any projected change(s) in monitoring priorities in the future. From my perspective, this document has done an excellent job of detailing the activities that have been initiated and conducted since the development of the Effectiveness Monitoring Plan (Lint et al. 1999); however, I would have liked to see this chapter provide some detail regarding priorities and strategies. For example, is there an expectation that the program will transition from Phase I to Phase II, as envisioned in Lint et al. (1999)? Some aspects of this program are well-developed and ongoing; others are a work in progress; still

others are just getting underway. Some strategic thought regarding the needs of the program in the short- and long-term would be helpful.

Response to #6

The section on monitoring program needs in the monitoring report was expanded to provide detail on the topic areas that should be included in discussions by the managers as they decide on the composition of the owl monitoring program for the second decade of Plan implementation.

Peer Reviewer #2

MAJOR ISSUES

I believe there are 3 issues in need of more thought and consideration before a final report is prepared and distributed.

Comment #1 - Phase I and II Monitoring Methodologies

My concerns here stem primarily from the section **Predictive Model Development** near the end of the draft report by Lint (in review). My strong view is that Phase II is at least 1-2 decades away; even then it seems questionable if a predictive modeling approach to monitoring is likely to be useful in terms of valid inferences. Consider the following information:

The draft report (Lint, in review, page v) states,

“Research on predicting owl survival and productivity in relation to habitat amount and arrangement has given us insight and understanding, but not the capacity to predict demographic performance for a given set of habitat characteristics at the landscape scale.”

This statement is a clear admission that a switch to Phase II is quite premature.

The monograph by Franklin et al. (2000) succeeded in trying to establish various relationships affecting variation in parameters such as apparent survival probabilities, recruitment rates, and finite rates of population change. This research focused on *a priori* science hypotheses to try to establish relationships, associations, and correlations among variables. However, several important limitations were exposed regarding these research results and several of these limitations have a direct bearing on modeling for *prediction and management application*, including,

Substantial uncertainty concerning the structural form of the models -- this is most apparent in modeling recruitment and finite rates of population change (using the logistic function for modeling apparent survival probabilities seems quite reasonable, although the log-log or complimentary log-log might also warrant consideration as these forms allow

asymmetry). This uncertainty will make predictions for management inaccurate and imprecise.

Lack of evidence for causation -- the results at this point represent only relationships, associations, and correlations among a few variables. Prediction would ideally be based on variables (and estimated model parameters) that are actually causal. This lack of causality will hamper accurate prediction for management.

Variable selection uncertainty -- this is most important as shown in Table 4 (Lint in review). Even the best model for apparent survival probability has a model weight of only 0.426 and relates only to 4 climatic variables (variables for which management lacks control or influence).

Franklin et al. (2000):576) note, “We expect that future parsimonious models will support additional climatic covariates with additional years of study. Therefore, the climatic models that we developed to describe environmental variation here should be considered first approximations.”

Franklin et al. (2000:577) continue, “However, we have the least confidence in the ability of the selected climate model to explain variation in reproductive output, because ...”.

At a different level, consider Table 12 in Franklin et al. (2000) where 8 *a priori* models are reasonably plausible for predicting reproductive output and 5 such models were reasonably plausible for predicting apparent survival probabilities. While important relationships were found and quantified, their use in prediction for monitoring and management was neither intended nor at all adequate.

The matter of scale -- this plagues most landscape-level modeling, has important consequences, and cannot be either ignored or arbitrarily specified in valid monitoring efforts where prediction is an important goal. When trying to understand the process, one can study the system at several arbitrary scales; however, in prediction for management purposes (including monitoring) this is an important issue to resolve.

Precision -- The existing, range-wide monitoring program typically provides estimates of key parameters with very high precision. This level of precision is excellent by nearly any standard. Let me illustrate this issue with the following information taken from the draft report by Lint (in review).

The coefficient of variation (cv) for lambda from the meta-analysis is 1.0%. This is the most important single parameter in the entire report and it's high precision is astounding.

The cv for lambda ranges from only 1.4% to 3.6% on an area by area basis (computed from Table 4).

Similarly, the cv for apparent survival probability of 3+ year old owls ranged from 1.1% to 3.8% (computed from Table 2).

Modeling these parameters using subsets of environmental covariates for *prediction* is much more difficult and uncertain and estimates of precision are quite poorer. For a few examples consider,

Franklin et al.'s (2000:575) Table 13 where the cv for each beta parameter ranges from 35 to 69%; this provides predictions with very poor precision.

The precision on predictions is more than an order of magnitude worse than the present approach (Phase I).

Examples illustrating the fact that precision is poor can be found in a large set of cases given in Franklin et al. (2000). Consider, for one example, their Table 10, showing estimated relationships between landscape habitat features and reproductive output.

The first (best) model has 5 slope parameters; the coefficients of variation for these parameters are 29%, 48%, 27%, 43% and 50%, respectively.

Survival and recruitment parameters ought to be a function of things like stand age, the extent of a layered canopy, and understory characteristics, *given* temperature and precipitation. At present we lack the ability to derive these functions, even if the importance of causation is ignored. We cannot expect accurate (monitoring) predictions from the best relationships now available. This situation will not change to any important degree during the next decade.

Short time history of the empirical data -- At best, we now have approximately 20 years of data for some of the earliest study areas ($n = 20$). This time frame is far too brief to experience much of the natural variation in climate and habitat variables. In addition, a deeper understanding from the use of the random effects models require larger sample sizes to provide good estimates of population variation (σ).

To summarize -- Lint et al. (1999) posed three questions regarding the efficacy of predictive models:

1. Can the status and trends in spotted owl abundance and demographic parameters be inferred from the distribution and abundance of habitat?
2. Can the relation between owl occurrence and demographic performance be reliably predicted given a set of habitat characteristics?
3. How well do habitat-based models predict occurrence and demographic performance in different land allocations?

The clear answer to the first two questions is simply *NO*. The inescapable answer to the third question is *POORLY*, if at all! Relatively little information is given concerning "occurrence" or if this is might be different than "occupancy."

Lint (in review: 13) conclude with, "*So even though we may not have achieved success in predicting rates of survival and productivity for a given landscape mosaic, we have a better understanding of the factors that affect them.*" The information discovered by this team failed to be able to discern relationships, correlations, and associations from causation.

[Let me emphasize that I think the occupancy models hold great promise but need further investigation, refinement, and evaluation. This approach might be less expensive than the capture-recapture surveys; however, it may provide less biologically relevant information (e.g.,

estimates of adult survival probability; a key parameter that has been in decline for, at least, the past 20 years).]

In summary I believe the current monitoring system is quite good (albeit “expensive” in the eyes of a few people). It is not prudent to hope that a valid prediction-based monitoring program is feasible, at least in the 1-2 decades ahead. Efforts directed at Phase II would be far better spent in (1) formal experimentation in unison with the current Phase I (see below) and (2) further exploration and experience with occupancy sampling and analysis theory and application.

Response to #1

The Discussion section for the Predictive Model portion of the Related Research chapter was edited and expanded to address the points raised by the reviewer.

Comment #2 - Barred Owl Results Should be Considered as Confirmatory

In some respects I think the increasing abundance of barred owls can be considered as confirmatory. This interpretation is important for several reasons. Consider the following information from the Interagency Scientific Committee (Thomas et al. 1990):

“Fragmentation may also increase the potential for spotted owl displacement by barred owls and great horned owls.” Page 22.

“The barred owl has invaded the Pacific Northwest and northern California in recent years and appears to be displacing spotted owls in some areas (Grant 1966 in USDA 1988a, Taylor and Forsman 1976).” Page 59.

“Several scientists have hypothesized that predation on spotted owls by great horned owls, and competition from barred owls, may increase with increasing amounts of forest edge associated with the harvest of mature and old-growth timber.” Page 274.

“But even more serious is the fact that the SOHAs are themselves internally fragmented into smaller blocks of suitable habitat that further increase the ratio of edge to area. This condition would increase all detrimental effects of fragmentation, and might increase competition with barred owls and predation by great horned owls ...” Page 293.

“The issues of predation on spotted owls, especially by great horned owls, and interspecific competition with barred owls, have surfaced repeatedly during the past decade or so (review in Hamer 1989).”
Page 294.

These concerns were made 15 years ago by a highly respected science team, including Mr. Joseph Lint, and now the substantial increases of barred owls seem to be very well documented. We should not treat this increase, and its likely cause, as a ‘surprise’ or as merely ‘descriptive’; rather it was forecast and hypothesized in the 1990 *ISC Report* (Thomas et al. 1990). I suggest

the increase in barred owls be treated as a largely confirmatory result and face the obvious cause (dramatic loss of forest habitat in the past decades).

Kelly et al. (2003) estimated that approximately 60 new barred owl territories have been added per year in Oregon since 1989. A thorough review of our knowledge of barred owls is provided by Courtney et al. (2004).

The draft report seems to imply that the competitive pressure that barred owls exert on the northern spotted owl is almost independent and a separate problem that cannot be controlled (at least my impression as I read the draft report). Rather, the invasion of the barred owl is but one more consequence of the substantial habitat loss during prior decades.

Page 12. There is, indeed, some *weak* evidence of an effect of barred owls on productivity (Anthony et al. in review, Table 8). Here 10 of 14 study areas had a negative slope in the relationship between productivity and the crude index to barred owls. Regardless, more effort should be expended to better define the variable “barred owl” and explore its importance.

Response to #2

Although we agree with the reviewer that continued attention to the barred owl is an important component of spotted owl conservation, we had no cause and effect results to confirm the effects of the barred owl on spotted owls. In addition, we had no information to confirm the invasion of barred owls was a “consequence of the substantial habitat loss during prior decades.” The following conclusion by Gutierrez et al. (2004), in our opinion, summarizes the barred owl situation well – that is why we included it in the discussion section, originally.

“While the evidence for a negative effect of Barred Owls is clearly correlational, we believe the Barred Owl is having a substantial effect on the Spotted Owl in some areas because of the preponderance of circumstantial and anecdotal information. In our evaluation, the Barred Owl currently constitutes a significantly greater threat to the Northern Spotted Owl than originally envisaged at the time of listing.”

Comment #3. Manipulative Experiments Essential

Monitoring alone is inadequate as the Plan moves ahead over the next several years. There are several areas where deliberate, formal experimentation is needed. This was a subject of much discussion among scientists at the NSO workshop in Corvallis, OR in January, 2004.

1. Individual territory-level barred owl removal experiments designed to learn of negative impacts of barred owls on spotted owl detection, survival, and recruitment.
2. Forest habitat manipulation on a territory level to make stronger, causal inferences. This is perhaps the most central need in terms of understanding and management options.
3. Floater population dynamics.

In each case, I am thinking of carefully designed experiments with proper randomization, replication, and control. The design and conduct of these experiments would represent a substantial challenge but can be done within and in conjunction with the existing (Phase I) monitoring program. Without the causal results from these experiments, the goals of the NWFP and the monitoring program will fail to be fully realized.

Response to #3

We concur that the exploration of causal factors is an important element to consider in future monitoring and data analyses efforts. Formal experiments on several fronts were identified in the discussion portion of the monitoring program needs section of our report.

OTHER SUBSTANTIVE COMMENTS

Comment #4

Page v, middle of page. The habitat variance is low because habitat itself has not varied much on an annual basis since the early 1990s. It is a mistake to believe that habitat cannot vary drastically (e.g., drastic declines in suitable habitat during the 1970 and 1980s). Since ESA Listing and the NW Forest Plan the changes (i.e., declines) in habitat have seemed small on an annual basis.

Response to #4

The statement on page v recapped what was found during predictive model work and was not intended to address the variation in amount of habitat over time.

Comment #5

[Several numbered comments below relate to the “Population Status and Trends” section of Lint (in review). There are no chapter numbers in some cases and the Table of Contents does not use a numbering system for major headings and subheadings. The ms. pages are not numbered sequentially. These issues make it difficult to reference material in the ms. in terms of this review. The final report should have a single pagination and all chapters should be numbered and referenced to the Table of Contents.]

Response to #5

Sequential pagination will be used in the published report and all chapters have been referenced to the Table of Contents.

Comment #6

Page 13. I strongly suggest taking Figure 7 showing the estimated lambda value and their confidence intervals from Anthony et al. (in review) and adding it to this report. This shows the primary results on the finite rate of population change clearly, by area and with associated confidence intervals. Reading the text in this case is not substitute for a “picture” and this is perhaps the most important issue addressed in this draft report.

Response to #6

As suggested, figure 7 from Anthony et al. (2004) was added to the final report and referenced in the text.

Comment #7

Page 16, 5-6 lines from bottom of page. "... population declines may have been due to the lag effect from loss of habitat in earlier decades due to timber harvest and wildfire."

This should not be 'hidden' as this is surely the main issue associated with the still-declining populations of spotted owl.

Response to #7

We do not consider this statement to be hidden. However, it remains one of several issues likely associated with continuing declines, thus we did not choose to single it out as the "main issue associated with the still-declining populations of spotted owl."

Comment #8

Page 17, 5-6 lines from the bottom. It is a real stretch to suggest that "Three of the ten demography study areas were *at least* stable during the monitoring period" (italics mine). From Table 4 only 1 of the 10 study areas has a point estimate >1 (Tyee). The other two (South Cascades and Klamath) have point estimates <1 and the point estimate for South Cascades (0.974) has a confidence interval supporting the notion that the population is probably declining (0.906 to 1.042). This material should be rewritten to be less misleading, given the quantitative evidence.

Response to #8

The text was edited to clarify the information presented.

Comment #9

Page 19, very top. Again, I do not think the barred owl issue should be treated as if it were an independent stressor on spotted owl populations. Perhaps I am reading too much into this; however, it is my feeling that the barred owl invasion and the associated competition is part of the "package" – part of the consequences of drastic forest habitat removal in earlier decades. This fact makes the Plan more important than ever. As I read this material, I get the feeling that people feel the Plan's success is "uncertain" and is becoming passé because of uncontrollable stressors such as barred owls and climatic variation. This is certainly not my view. I think some subtle rewording might lead to a more accurate portrayal of the status of the NWFP or its monitoring program.

Response to #9

The text was edited to reflect that the success of the Plan in maintaining and restoring habitat and the need for this to occur were not uncertain in our minds, but also to point out that future conservation efforts may need to address more than habitat management.

Comment #10

Page 19, very bottom. As my comment, just above, this wording is poor, I think. The recovery of the spotted owl is not “masked” by the barred owl invasion; such factors are part of the “price.” White (2001:384) notes “*Pay me now or pay me more later.*”

Virtually all populations respond to variation in climatic variables; no one thought otherwise at the time of listing or deliberations by the ISC. Some rewording here could yield improvements.

Response to #10

The text was edited and the word ‘masked’ was removed to clarify the discussion. The second part of this comment was not page-referenced so we were unable to respond.

Comment #11

Chapter 3, Habitat Status and Trend is generally outside my field. I scanned this material and felt the maps and figures were quite interesting. I did note that no estimates of precision were given in the extensive tabular material presented. Cannot some notion of repeatability be provided?

Response to #11

Estimates of map accuracy were provided in Appendix F. There were no precision estimates for individual summary statistics derived from the various map queries. We envision that the next round of monitoring will reexamine the vegetation conditions on the same landscape. The BioMapper output displayed histograms would allow assessment of change from the last period. We recognize that issues of map bases and new technologies will need to be accounted for.

Comment #12

Summary in the final chapter, bottom of page 2 and top of page 3. Again, this portrayal of declining vs. stable populations lacks accuracy (see comment above relating to Page 17, 5-6 lines from the bottom). One must not think that a confidence interval that includes 1.0 is an accept/reject “test” result. The evidence for South Cascades does not imply that this population is stable (i.e., non-declining). Some rewording here could be easily done to more accurately reflect the evidence.

Response to #12

The Summary was edited to reflect changes resulting from the response to Comment #8 above.

Comment #13

I was surprised that there was virtually no mention of “floaters” in the entire draft report. Is this an omission or has this segment of the population diminished? Or What? This population segment has impacts, probably both positive and negative. Why no mention of this issue?

Response to #13

The floater population was a direct not part of the population studies. The demographic studies focus on territorial owls and their reproductive output. Fledgling owls are banded, but once they enter the floater population when they disperse from the natal area they are not usually resighted until they resurface (usually 1-5 years later) in the territorial portion of the population.

Comment #14

Page 14, 3 lines from the bottom. The comment, "*It appears that we may have models that can reliably predict owl occupancy for a given year under a given habitat mosaic*" was a surprise to me. Perhaps this is in the report by Olson (in review; I found no citation to this ms.)? I saw no evidence of this predictive ability in the draft report under review (Lint in review). In fact, on Page 12 of **Predictive Model Development** one finds the statement

"There were no peer-reviewed results available from the occupancy modeling efforts."

Then, one finds the following statement,

"Progress to date has shown success in relating annual occupancy to landscape habitat variables."

I would like to see several examples of this ability, including coefficients of variation and estimates of predictive mean squared error (PMSE) before I am willing to think that this statement about predictive reliability is justified. Finding a relationship is one thing; the ability to expect good predictive ability is quite another matter. This is an important issue and bears directly on issues surrounding Phase II monitoring (see extensive comments above under **Major Issues**).

In reading and studying the entire report, I find no justification of this statement (above, underlined). I think this issue deserves some considerable thought yet. In the end, I would be surprised if this statement can be justifiably included here. Important management implications exist here and I cannot be impressed with a fleeting reference to a yet to be seen paper still in review.

Response to #14

The text was edited to eliminate speculative statements relating to the occupancy modeling work and possible outcomes of that research in advance of peer review.

LESS IMPORTANT ISSUES

Comment #15

Consider adding the word “disease” to the first paragraph of the *Abstract*.

Response to #15

The list of possible contributors to the decline of spotted owl populations came from Anthony et al. (2004). They did not list “disease” as a contributing factor.

Comment #16

Much of the text fails to distinguish between parameters and estimates of parameters (e.g., “Survival rates declined ...”, when it is more accurate to write “Estimates of survival rates declined ...”).

Response to #16

The text was edited to reflect reviewer comment.

Comment #17

Chapter 2, page 11, 4 lines from bottom. Consider changing the text to “... failed to show ... “ rather than the more assertive (and less technically correct) “...showed no effect.”

Response to #17

The text was edited to reflect reviewer comment.

Comment #18

Consider adding the words “stable” and “stationary” to the *Glossary*.

Response to #18

The word stationary was added to the Glossary.

Comment #19

Chapter 2, page 16, first full paragraph. The two parenthetical expressions are incorrect and should be removed. The associated values in the text (34% and 77%) are correct.

Response to #19

The text was edited to reflect reviewer comment.

Comment #20

Chapter 2, page 13, first sentence in the final paragraph. The parenthetical expression here is perhaps confusing and seems unclear to the reader.

Response to #20

The text was edited to reflect reviewer comment.

Comment #21

Chapter 2, page 15, line 3 of final paragraph. Again, the parenthetic expression seems unclear and confusing.

Response to #21

We could not find the parenthetic expression the reviewer was referring to, so no response was possible.

Comment #22

Chapter 2, page 19, first line. ...may be an additional stressor on the *population caused by the extensive habitat changes in decades past*. I do not want the reader to think that these are new, unrelated stressors that now confuse and complicate the issues.

Response to #22

The text was edited to reflect reviewer comment.

Comment #23

Chapter 3, Fig. 10 seems mis-drawn. This figure attempts to show conceptual probability distributions by province and by owl. However, the scales have been mixed as the area under each curve should be the same (if each is a pdf the area should integrate to 1).

Response to #23

The figure is correct as drawn.

Comment #24

Chapter 4, Table 1. Three figures might show these differences and the age-specificity much more clearly.

Response to #24

The information in table 1 was displayed in a bar graph.

Comment #25

Chapter 4, Fig. 1 seems hard to understand. What is one to infer from this figure? I could not find anything on this in the text (also, the figures are not referenced in numerical order in the text).

Response to #25

The text was edited to explain Figure 1 and it was referenced in the document.

Comment #26

Fig. 1 in the final section (% barred owls) is of poor quality and barely readable.

Response to #26

This figure was removed from the report. The information it conveyed is included in the text.

Comment #27

There are more than a few cases in the text, particularly in Chapter 3, where one finds "... data is ..." or "... the data was ..." Of course, data is a plural form.

Response to #27

Text was corrected to express "data" as the plural form.

Comment #28

Final chapter, page 2, very bottom. Again, the parenthetical material is confusing. I suggest rewording such as "...Tyee, were under 1.0, indicating a declining population." That is, explain the meaning, without the confusing parenthetical expression.

Response to #27

Text was edited as suggested by the reviewer.

OVERALL REVIEW SUMMARY

I believe there are 3 important areas that need substantive additional attention:

Comment #28

Phase II predictive monitoring should be redirected toward (1) experimentation to learn important causal results and (2) further understand the benefits and advantages of occupancy sampling, modeling, and estimation. There must be a major effort to exhaustively compare and contrast the existing capture-recapture monitoring methods with the new occupancy approaches before jumping from one time-honored, fairly well understood approach to a relatively new and untried (but promising) approach.

Response to #28

The points expressed by the reviewer were incorporated in the discussion section of the predictive model portion of Chapter 5 on related research.

Comment #29

The substantial increases in barred owls should be viewed more as a confirmatory result. This differing view has important management implications and calls for a revised perspective.

Response to #29

At this point in time we consider the information on barred owls to be correlational and circumstantial. We do not feel as comfortable using the term “confirmatory” as the reviewer does. This does not, however, translate to a low concern for barred owls and their possible impacts to spotted owl conservation and recovery. We recognize the barred owl as an important stressor and raise the need to consider the implications.

Comment #30

Formal experimentation is now essential to begin to understand some important causal relationships. Some examples are given (e.g., effects of barred owls on survival and recruitment, effects of forest habitat manipulation, and the dynamics of floater populations). Monitoring, by itself is inadequate and additional attention needs to be placed on the need for some critical experimentation.

Response to #30

We concur with the reviewer that understanding of causal relations is a next logical step and have highlighted it in the monitoring program needs section of the monitoring report.

Comment #31

Lint (in review: 4) notes the “**build it [habitat] and they will come**” approach. I think it is better to consider the “**repair it and do not let it deteriorate further and they will come eventually**” approach. As noted in the ISC Report (Thomas et al. 1990), forest habitat will not be replaced for decades and we can expect owl population decline to continue for some years (decades?) yet. Hopefully, owl populations will stabilize at a new (but lower) level in the future. We must not get over-anxious to see stable populations of northern spotted owls return to the Pacific Northwest or to prejudge any ‘lack of success’ of the NWFP or the monitoring program.

Response to #31

The text has been edited to reflect the habitat maintenance and restoration objectives of the Plan. The discussion sections at the end of chapters 2 and 3 reflect the reviewers concern for not getting “over-anxious” to see stable populations or prejudge “lack of success” of the Plan or monitoring program.

Peer Reviewer #3

CHAPTER 2 - POPULATION STATUS AND TRENDS

GENERAL COMMENTS

Comment #1

I found that there were a number of inconsistencies between the description of the 2004 northern spotted owl meta-analysis and the descriptions in this chapter. I think this was primarily a problem in para-phrasing what was reported in Anthony et al. (2004). I have described these inconsistencies in the Specific Comments below. While some of these comments may seem trivial, I think it is important that this section and the meta-analysis be consistent in the approaches used and terminology. In particular, the section on developing data for fecundity analysis contains statements that are incorrect in how the data files were developed. This chapter needs to be evaluated carefully and re-written accordingly.

Response to #1

Text was edited to reflect specific comments and the section on developing data for the fecundity analysis was reviewed and edited as needed.

Comment #2

The term “stable” is used throughout to describe rates of population change (i.e., where $X_i = 1$). The correct term is “stationary” which should be used instead of “stable” throughout the chapter.

Response to #2

The text was edited to change “stable” to “stationary”.

Comment #3

The writing in this chapter was rough in a number of places. I suggest that revising the wording and grammar would make the writing clearer and match the terminology with previous descriptions of the meta-analyses.

Response to #3

The chapter was reviewed and some changes made, but the general nature of this comment did not provide specific places or even examples where the reviewer thought wording or grammar changes were needed or terminology did not match the meta-analyses. This chapter was reviewed by an editor prior to the peer review and changes were made based on the editor’s review.

SPECIFIC COMMENTS

Comment #4

Page 3, para. 3 – I would eliminate the sentence about 165 years of survey data; the years between studies are not necessarily independent and 165 years does not represent any meaningful sample size. Thus, there is no real utility in this statement and I think it is misleading.

Response to #4

The text was edited to reflect the reviewer's comment.

Comment #5

Page 3, para 4 - Use the term “researchers” or “biologists” rather than “owl surveyors”.
The demographic studies are more complex than just going out and surveying owls.

Response to #5

The text was edited to reflect the reviewer's comment.

Comment #6

Page 4, para. 5 – “0” in the column indicates that the owl was not identified, not just seen (owls can be seen but not identified).

Response to #6

The text was edited to reflect the reviewer's comment.

Comment #7

Page 5, para. 2 & 3 – The section on developing fecundity data needs to be rewritten. In the first sentence, the database provided “annual information” and I would replace “reproductive success” with the “number of young fledged”. In paragraph 3, “Sites where owl pairs were nesting” should be replaced with “Spotted owl sites” because sites were checked for number of young fledged, regardless of whether the owls had nested. That is, a site could have values of 0, 1, 2 or (rarely) 3 young fledged and was checked twice in the field (when possible), regardless of whether the owls nested. Thus the second sentence needs to be re-written to include that fact. The authors need to check Franklin et al. (1996) and Anthony et al. (2004) for the specific details on how fecundity data was collected and avoid deviating from the descriptions in those documents.

Response to #7

The text was edited to reflect the reviewer's comment and the fecundity data collection description was aligned with the references.

Comment #8

Page 7, para. 1 – Survey effort for a_{RJS} should be relatively constant across “space” within years, not “time”. That is, recapture probabilities should be relatively constant across the study within years but can vary by year for the whole study area.

Response to #8

The text was edited to reflect the reviewer's comment.

Comment #9

Page 7, para. 3 – Should be “lambda analysis” or “analysis of a_{RJS} ” rather than “population change analysis”, which doesn't refer to anything mentioned previously (i.e, will be confusing to the reader).

Response to #9

The text was edited to reflect the reviewer's comment.

Comment #10

Page 7, para. 5 – Anthony et al. (2004) did not use a bootstrap goodness-of-fit test but used program RELEASE instead. RELEASE is not a bootstrap GOF test.

Response to #10

The text was edited to reflect the reviewer's comment.

Comment #11

Page 8, para. 1 – The third bulleted item should be deleted; capture probabilities were estimated jointly with survival in the next bulleted step. The fifth bulleted item can be deleted as it is part of the fourth step.

Response to #11

The text was edited to reflect the reviewer's comment.

Comment #12

Page 8, para. 3 – All of the analyses were analyzed using maximum likelihood estimation, not just fecundity.

Response to #12

The discussion on Page 8 para. 3 was specific to the fecundity analysis and does not address other analyses such as apparent survival.

Comment #13

Page 9, para.3 – I would use the term “stationary” instead of “stable” to describe population trends based on X here and throughout this chapter. Technically, stationary is the correct term. I would also introduce the term $\lambda_{X,RJS}$ early on when you talk about the Pradel models in the Methods. It sounds like you are talking about two different types of lambda here when in fact you are talking about λ_{RJS} .

Response to #13

The text was edited to reflect the reviewer's comment.

Comment #14

Page 9, para. 4 – In the last sentence on this page, I would argue that $X > 1$ “may not compensate” rather than “will likely not compensate”. If X is large enough, it can conceivably reverse declining trends.

Response to #14

The text was edited to reflect the reviewer's comment.

Comment #15

Page 12, para. 2 – In the last sentence, the authors’ hypothesis may also have resulted from the poor choice of the barred owl covariate used in the meta-analysis.

Response to #15

The text was edited to state the conclusion of Anthony et al. (2004).

Comment #16

Page 14, para. 2 – The weighted mean X equates to an “average” or “mean” decline, not a “general” decline. A general decline suggests that all of the study areas were declining when in fact not all were declining.

Response to #16

The text was edited to reflect the reviewer’s comment.

Comment #17

Page 14, para. 3 – NW California should be included in the list in the second to last sentence.

Response to #17

Anthony et al. (2004) does not include the NW California study area in the group of study areas with a 20-30 % decline that is discussed in the second to last sentence.

Comment #18

Page 15, para.2 – In the second sentence, population growth was not statistically > 1.0 for any of the populations. I would use “ X ” rather than “ > 1.0 ”. In addition the statement “...an average decline of 3.4 percent indicating the rate of decline were not equal across the range...” is not really correct. The average does not provide any information about variation in X among the study areas.

Response to #18

The text was edited to clarify the second sentence.

Comment #19

Page 18, last para. – I thought the concluding statements here were excellent.

Response to #19

No response needed.

CHAPTER 3 – HABITAT STATUS AND TREND

GENERAL COMMENTS

Comment #20

I thought the authors followed an excellent general strategy in trying to develop a meaningful habitat map for spotted owls. However, I had a number of problems with the details as I have outlined in both the following General Comments and the Specific Comments below.

Response to #20

No response needed.

Comment #21

I think the authors need to be careful about their definitions of habitat and how their mapping algorithm relates to spotted owl habitat. First, I found a lot of inconsistency in definitions of habitat and habitat quality in the report with those in the recent literature. I thought that the definition of habitat used in the report was incomplete with respect to more recently accepted definitions (e.g., Hall et al. 1997. The habitat concept and a plea for standard terminology. *Wildlife Society Bulletin* 25:173-182) and habitat quality was never defined. I would argue that their habitat models really only describe the component of spotted owl habitat used for nesting and roosting (vegetative stands used by spotted owls for nesting and roosting) because (as far as I could tell) the modeling is based solely on roost and nest locations, and not foraging locations. I think this should be made clear in the document. I would also argue that they are not measuring “niche” which can be defined as “the portion of the environment which a species occupies, defined in terms of the conditions under which an organism can survive, and may be affected by the presence of other competing organisms”. The location data for spotted owls associated with particular stand descriptors used in their models do not measure niche.

Response to #21

A new section discussing the term “habitat” and the use of the term “habitat suitability in our report was added to the methods section to address the reviewer’s comment. Text was also added to the discussions of niche in the methods section.

Comment #22

I was ambiguous about the use of BioMapper as an approach to develop an initial predictive model of stands used by roosting and nesting owls. In one respect, I understand that some mapping algorithm had to be used. However, there were a number of problems I had with the algorithm that I detailed below in my specific comments. I think the authors need to be careful in their assessment of BioMapper. In reading the methods sections about BioMapper, I felt I was being sold the method rather than being offered a critical evaluation of its utility. Clearly, there are problems with the program and those should be stated more clearly. I also think that the results from the mapping process of owl habitat should be represented more as a hypothesis to be further tested, rather than a final product. However, this point was not clearly presented in this chapter.

Response to #22

The text of the methods sections was edited to provide a more objective explanation of the BIOMAPPER program.

Comment #23

One problem is that differences in map bases may be confounded with difference in stand use by the owls, as determined from the modeling. In some places, the differences in “habitat suitability” were attributed to differences across the range of the owl (northern versus southern portions of the range). If this was the case then I would have expected the BioMapper habitat model outputs for the Klamath Province of Oregon to be more similar to the Klamath Province of California. However, they were quite different, especially at the tails of the Area-Adjusted Frequency distributions and with respect to marginality, specialization and tolerance values (see Figures D-7 and D-8). Therefore, I suspect that the differences could be attributed to the fact that California had a different map base than Oregon and Washington. This problem needs to be addressed by the authors. I realize the authors alluded to these map differences in several places but I think it needs to be reinforced in several areas to make problems clearer.

Response to #23

The difference in map bases is described in the data sources section, the results section and the discussion section of the habitat chapter. In addition, it is again highlighted in the information management issues section of the 10-year report. We do not think additional discussion is needed.

Comment #24

In future modeling efforts, I think the use of fragmentation indices needs to be considered more deeply. Often, these are *ad hoc* indices that are not examined through simulation to see if they measure what they are supposed to measure (see Li, H., and J. F. Reynolds. 1994. A simulation experiment to quantify spatial heterogeneity in categorical maps. *Ecology* 75:2446-2455).

Response to #24

Future consideration will be given.

SPECIFIC COMMENTS

Comment #25

Page 13, para. 3 – In discussing the utility of habitat models, I think they should include the idea that model development is an iterative process, where models are developed, tested with independent data, refined, and then tested again. Thus, I would argue that the authors have only just started the habitat modeling process, albeit a good start.

Response to #25

The text was edited to incorporate the idea that map development is an iterative process.

Comment #26

Page 14, para. 3 – Based on the arguments here, it seems generalized linear models could be equally effective. There are a number of advantages to using these types of models over the PCA used in BioMapper, such as using information-theoretic approaches for model selection, and more interpretable parameters.

Response to #26

The reviewer expressed valid points regarding generalized linear models. We chose to use the BioMapper model because the available owl data was presence-only information. BioMapper has been shown to be equally effective as GLM as we note in the text.

Comment #27

Page 15, para. 1 – I did not understand the statement that BioMapper is robust to sample sizes. Does that mean a sample size of 5 is as good as 1000? I also would not consider discrimination of 63% as accurate (is not much better than 50%, which is equivalent to flipping a coin).

Response to #27

The text was edited to clarify the discussion of sample size as it relates to BioMapper.

Comment #28

Page 15, para. 2 - In the first sentence, I argue that you are measuring how suitable the habitat may be for species presence. This is different from the original statement that implied you were measuring suitability in a general sense for the species. I also argue that there are more problems with using just presence data to describe species habitat associations and this should be explored more fully in the future (not necessarily in this report). Lack of absence data is not as much a statistical problem (i.e., whether certain models can handle this lack) but more a biological problem (do you have sufficient information to describe the system). For example, I don't believe the second statement in this paragraph that when absence data are lacking, that models based on presence-only data are appropriate. There may be models that can handle presence-only data but you will be more limited in inference when you only have presence data.

Response to #28

Our model output assigns a habitat suitability score to each pixel and we summarize the percentage of the pixels in 0-20, 21-40, 41-60 etc. with no accounting for the size or arrangement of the pixels. Without spatial parameters, our estimate of habitat suitability cannot accurately measure presence. Our objective was to track the maintenance and restoration of owl habitat condition as described in the text.

The statement about appropriate use of models when only presence data is available is supported in the two peer-reviewed journals articles cited in the text on that page.

Comment #29

Page 15, para. 3 – I had two problems with this paragraph. First, reduction of human bias in here. I would eliminate this sentence. Second, principal components analysis has a number of problems that are never presented in the document (see Rexstad et al. 1988. Questionable multivariate statistical inference in wildlife habitat and community studies. Journal of Wildlife Management 52:794-798). One reason the use of PCA diminished in wildlife habitat studies was because of the problem of comparing variables on different scales (e.g., using canopy coverage expressed as % with mean diameters in inches). Factor analysis and PCA were really designed to be used with variables that are on similar scales (e.g., morphological variables all measured in centimeters). I would acknowledge some of the problems in the BioMapper algorithm, rather than just extol its virtues.

Response to #29

The sentence about reduction of human bias was removed. The text was edited to provide an explanation of how BioMapper handles comparison of variables with different units. A section was added to the text to reflect some of the limitations of the BioMapper algorithm we used and provide a more balanced description of the program.

Comment #30

Page 16, para. 1 – I would argue that an animal's niche cannot be described this simply with presence data only.

Response to #30

Additional discussion was added to the text to clarify the description of the animal's niche under BioMapper.

Comment #31

Page 16, para. 2 – I still don't understand what robustness to sample size means.

Response to #31

The word "robust" was removed from the text and additional discussion was provided about the model's performance with different sample sizes of owl presence pixels.

Comment #32

Page 17, para 3 – The authors should also include the idea of validating the models with independent data sets outside of the data set used to develop the model.

Response to #32

Validation analysis using independent data sets was added in Appendix F and referenced in the text.

Comment #33

Page 18, para. 3 – I had a number of problems with this paragraph. First, the maximum sample size should really be the number of owl locations (n = 1560). In actuality, the sample size should be based on owl sites because a number of locations were taken from the same sites. This lack of independence can be dealt with in generalized linear models (i.e., mixed models) but cannot be dealt with the BioMapper algorithm, as far as I can tell. Regardless, the sample size should not be 34,073. This seems to false inflation of the true sample size. Second, having five factors to explain 7 variables does not seem all that informative, even if it does explain a lot of the variation. Models using the untransformed variables might be more informative. Third, I did not understand in Table 2 why QMDCC was negatively loaded on Factor 2 and positively loaded on Factor 3. This should be explained briefly.

Response to #33

We ran sensitivity analyses to assess spatial autocorrelation that may have resulted from using the buffered presence data and the results are discussed in the text.

The rationale for the use of the 5 factors was based on MacArthur’s broken stick method as explained in the text – this is the method recommended by Hirzel for factor selection.

The positive and negative signs for the marginality factor are explained in the text and also table 6.

Comment #34

Page 19, para. 2 – I would refrain from describing the model as explaining 70% of the owl’s niche. As I suggested before, I don’t think your measures and model explain the niche and this is going beyond these data.

Response to #34

The text was edited to clarify the use of the 70% in explaining the owl’s ecological niche space.

Comment #35

Page 19, para. 4 - I think the use of the term “owl use locations” is the appropriate way to describe what is being predicted. I would use this term wherever possible.

Response to #35

No response needed.

Comment #36

Page 25, para. 3 – The use of the term “habitat quality” here is not correct. You are not measuring habitat quality from your data but differences in habitat use. Estimation of habitat quality requires a link with demographic performance, which you have not done. This needs to be corrected in the report and on Table 5.

Response to #36

Table and text were edited to reflect the reviewer’s comments.

Comment #37

Page 26, para. 2 – Why did you choose 68 and 90% confidence intervals? A sentence or two here explaining the reasons for the choices would be helpful.

Response to #37

The text was edited to explain the reason for using the two confidence intervals.

Comment #38

Page 31, para. 4 – In the first sentence, I would clarify that habitat use by the owl applies to roosting and nesting habitat and not habitat in general.

Response to #38

Text was added to the methods section explaining our rationale that the habitat use does include foraging, not just nesting and roosting.

Comment #39

Page 39, para. 3 – Was the loss of the 24% of acres from wildfire in the Oregon Klamath due to a single event (i.e., the Biscuit fire) or to multiple events?

Response to #39

The loss was from multiple events with the Biscuit fire being the largest.

Comment #40

Page 16, para. 2 – I would organize the different hypotheses here a little better and give them equal consideration. In addition, I would explain in one or two sentences why fire suppression might make a difference (i.e., the Klamath Province used to have a very dynamic fire history; see Frost, E. J., and R. Sweeney. 2000. Fire regimes, fire history and forest conditions in the Klamath-Siskiyou region: an overview and synthesis of knowledge. Unpublished Report World Wildlife Fund, Klamath-Siskiyou Ecoregion Program, Ashland, Oregon; Taylor, A. H., and C. N. Skinner. 1998. Fire history and landscape dynamics in a late-successional reserve, Klamath Mountains, California, USA. *Forest Ecology and Management* 111:285-301; and Taylor, A. H., and C. N. Skinner. 2003. Spatial patterns and controls on historical fire regimes and forest structure in the Klamath Mountains. *Ecological Applications* 13:704-719).

Response to #40

The page/paragraph reference does not match the comment, so we were unable to respond to the comment

Comment #41

Page 49, para. 3 – Again, I would not use the term “quality habitat” here but maybe something like “high-use habitat”.

Response to #41

The text was edited to describe that we were referring to habitat-capable lands in the 41-100 range of habitat suitability.

Comment #42

Page 50, para. 3 – I would temper the last sentence here by stating that we need to know the effects of stand treatments on owl habitat and owls before they are applied to reduce risk of loss to wildfire. That is, the treatments could have a greater effect than the wildfires in degrading spotted owl habitat.

Response to #42

The text was edited to reflect this perspective.

CHAPTER 4 – OWL MOVEMENTS

GENERAL COMMENTS

Comment #43

This chapter was short and to the point and provided interesting information. One question I had was whether it would be possible to estimate net movement from reserves to outside, and from outside to reserves. This would provide some information on net flow between the two components as an indication of the importance of lands outside the reserve. In addition, you could just look at reserve juveniles and ask the question “*do reserve juveniles stay within the reserve system?*” under the scenario that areas outside the reserve system no longer support owl populations. In this case, you would use the sample of juveniles born within reserves ($142 + 268 + 232 = 642$), with 58% staying within the reserve system. You might consider doing some analyses with the movement data to examine the above (and other pertinent) questions.

Response to #43

We were not able to estimate net movement from reserves to outside and vice versa. We did include the estimate of the percent of juveniles fledged inside the reserves that stayed in the reserves. These ideas for additional analysis are more appropriate for the future when there is a more definitive difference in habitat conditions inside and outside of the large reserve blocks.

SPECIFIC COMMENTS

Comment #44

Page 2, para. 1 – In the bulleted items, I would briefly define the terms used, such as final dispersal distance, straight-line distance, etc. in parentheses so the reader has a better idea of what you are talking about.

Response to #44

The text was edited to explain the distance statistics presented.

Comment #45

Page 3, para. 3 – The term “recovery” is used in the bird banding literature to indicate a dead bird that has been recovered. However, some of the birds here were re-located alive. I would suggest a different term rather than “recovery”, perhaps “re-location”.

Response to #45

The text was edited to replace “recovery” with “resighting”

CHAPTER 5 – RELATED RESEARCH

GENERAL COMMENTS

Comment #46

The Predictive Model Development section needs considerable work. There were a number of misinterpretations in methodologies and results. I tried to outline these in my Specific Comments below but in some cases, entire paragraphs were incorrect. I would suggest that the authors re-evaluate this section more carefully with respect to the publications cited.

Response to #46

See responses to specific comments below

Comment #47

Contrary to the authors statements, both Franklin et al. (2004) and Olsen et al. came up with similar conclusions (see my Specific Comments below); they differed primarily in how much variation was explained by landscape habitat. I was able to briefly read the published version of Olsen et al. in *Journal of Wildlife Management*, which I received shortly before this review was due. However, there appeared to be important differences in measurement of circles used to estimate territory size (Franklin et al. used a static circle over time whereas Olsen et al. used a circle centered on annual roost and nest sites), and definition of habitat covariates. As far as I could tell (and I could be mistaken), the best model for survival and reproductive output in Franklin et al. was not used in Olsen et al. This could explain the differences in amount of variation in the demographic parameters that were explained by landscape characteristics.

Response to #47

The text was edited to describe the different size circles used in the two papers. Olson et al. noted on page 1051 that comparison of their results with Franklin et al. were limited by different methods, but stated that comparisons of model factors based on model variance [which is the information we included in our review] was appropriate.

Comment #48

I think the statements favoring prediction of habitat based on territory occupancy over prediction of habitat based on measures of habitat quality (i.e. habitat fitness potential) are overly simplistic and lack an understanding of the differences between these two measures.

Response to #48

We were eluding to predicting the probability of occupancy based upon habitat conditions, not as the reviewer states of “favoring prediction of habitat based on territory occupancy”.

SPECIFIC COMMENTS

Comment #49

Page 4, para. 1 – First, the authors use the terms “productivity” and “reproductive output” interchangeably. I would use the term “reproductive output” because productivity usually refers to the number of young fledged per successful female whereas reproductive output refers to the number of young fledged per female. Second, there were differences in the use of circles as surrogates for territories; Franklin et al. used a static circle over time whereas Olsen et al. used a circle centered on annual roost and nest sites.

Response to #49

We changed “productivity” to “reproductive output”. The text was edited to reflect the differences in circles used by Franklin et al. and Olson et al..

Comment #50

Page 4, para. 3 – This paragraph needs to be re-written. First, it should be explained that survival and reproduction were the response variables and climate, landscape habitat, age, and sex were the explanatory variables. The third sentence makes no sense since the response variables were demographic parameters for the owls.

Response to #50

The paragraph was revised drawing upon specific information from Franklin et al. (2000)

Comment #51

Page 5, para. 1 – Cormack-Jolly-Seber models are a type of capture-recapture model and should not be separated here. Again, I would use reproductive output rather than productivity.

Response to #51

The text was edited to clarify that the models used capture-recapture data. The term productivity was changed to reproductive output.

Comment #52

Page 5, para. 2 – Climate was used as a time-varying covariate in models for both reproduction and survival; the differentiation in the first sentence does not make sense. In addition, covariates for precipitation and temperatures were derived from the weather station data; the raw weather station data were not used in its raw format.

Response to #52

The text was edited to clarify the points raised by the reviewer.

Comment #53

Page 5, para. 3 – See my Specific Comment #1 above; there was a difference in how territories were defined between the two studies.

Response to #53

The text was edited to specify the different distance measures used in the two studies.

Comment #54

Page 6 – The description of Methods did not seem to fit completely with what was done in both Franklin et al. and Olsen et al. You should have one or both authors review this section or distill it down further into a more general, conceptual framework used and omit the analytical details. The latter would be preferable.

Response to #54

The methods section was distilled to provide a general description of the methods used.

Comment #55

Page 9, para. 9 – I would use the term “older forest” here rather than “spotted owl habitat”. This was the conclusion reached in Franklin et al. and makes the comparisons clearer between the two studies.

Response to #55

We used the term “spotted owl habitat” because that is what Frankin et al. (2000) used in their results section (eg page 567).

Comment #56

Page 10, para. 1 – It would be clearer in the second bulleted item if you described “core spotted owl habitat” as “interior older forest”. The third bulleted item seems incorrect. Franklin et al. found a positive relation with amount of edge between older forest and other vegetative types. Thus reproduction was positively associated with edge and negatively associated with interior older forest (similar to what Olsen found).

Response to #56

We used the term “core spotted owl habitat” because that is what Frankin et al. (2000) used in their results section (eg page 570). The third bullet was edited to clarify the conclusion.

Comment #57

Page 10, para. 2 – In the second bulleted item, it should be noted that Franklin et al. had the least confidence in the effects of climate on reproduction because the data lacked sufficient environmental variability. In the fourth and fifth bulleted items, I am unsure why this should be unexpected; Franklin et al. found a similar pattern.

Response to #57

The reference to Franklin et al. 2000 was to point out to the reader that there was a difference in results between the two studies not to discuss the confidence in the effects noted by either research team. The term "unexpected" was used by Olson et al. (2004) to characterize their findings.

Comment #58

Page 11, para. 2 – Franklin et al made the same conclusions for all of these items (although not as explicitly in the second item). They also offered some potential mechanisms, which are not discussed here.

Response to #58

The text was edited to discuss the links Franklin et al. mentioned between habitat configuration, survival and reproductive output.

Comment #59

Page 13, para. 1 – I am not sure that the uncertainty of climate reduces our predictive capability. First, the habitat models developed by Franklin et al. explained 67% of the variation (with an additional 9% explained by age) in the absence of climatic variability. In addition, there were interactive effects of climate and habitat in association with survival. This suggested that habitat with high fitness buffered individuals against climate, at least in terms of survival. Thus, habitat quality in the larger mapping effort in Chapter 2 could be examined in good climate years versus poor climate years. This would be a more informative approach than examining habitat in a static environment. Regardless, if climate differentially affects survival and reproduction, it will also affect territory occupancy because territory occupancy is a function of survival and recruitment in territories. I do disagree with the last statement in the paragraph. I think Franklin et al was successful in predicting demographic parameters and Olsen et al may be able to achieve similar predictions given some additional explorations. Both of these efforts should be considered preliminary and more work needs to be conducted in this arena.

Response to #59

Several sentences were edited or deleted to reflect the reviewer's comments.

Comment #60

Page 13, para. 2 –

The authors need to clarify their discussion on variation. They should concentrate more on the effects of spatial variation here because that is directly applicable to the habitat-based variation, keeping in mind that climatic variation is important background variation to be considered.

Response to #60

The text was edited to reflect the nature of climatic variability and its role in model development.

Comment #61 Page 14, para. 2 – This paragraph seems to discount the work by Franklin et al in favor of the findings by Olsen et al. As I argued in my General Comments, the inability to

explain spatial variation based on habitat does not mean that there are not habitat differences. As I noted previously, I could not tell whether the best model found by Franklin et al. was included in the model set examined by Olsen et al. Other differences in the two studies could also be responsible. I would argue these differences need to be resolved before this approach is completely discounted.

Response to #61

The information was recapping the results from the two studies for several similar analyses. No opinion is offered on which conclusions may be better supported-just that they differed. Additional model development studies will likely focus on the same areas and provide better understanding of differences observed between study areas.

Comment #62

Page 14, para. 3 – I disagree with this conclusion because of the reasons I stated earlier. Habitat fitness potential which balances competing influences on survival and reproduction is an important measure of habitat quality, whereas territory occupancy alone is a much poorer and incomplete measure of habitat quality. In addition, both occupancy and fitness are important but describe different levels of population associations with habitat. Occupancy and fitness measures should be considered parts of an overall function describing habitat relationships with spotted owl populations (see page 581 in Franklin et al. 2000 for a discussion of the relationship between habitat fitness potential and occupancy). Thus, relying solely on occupancy may tell an incomplete story and miss some of the competing influences that habitat fitness potential provides.

Response to #62

The paragraph was revised using conclusions from the two papers. Our purpose in examining occupancy is not to discount other information, but to further explore the application of new analysis methods to the spotted owl. Assessing the probability of occupancy for different landscape vegetation mosaics remains an area of interest with respect to monitoring the maintenance and restoration of habitat under the Plan.

Comment #63

Page 19, para. 3 – The barred owl covariate was an annual covariate that applied to the annual population parameters. The reason it was considered a poor covariate in Anthony et al. (2004) was because it was probably better used on a site basis, with inferences to sites rather than entire populations.

Response to #63

The text was edited to focus the discussion on the barred owl data presented by Anthony et al. (2004) not a description of the use of that data as a covariate in the population analyses.