

**MARBLED MURRELET MODULE BLIND PEER REVIEW RECONCILIATION
REPORT OF CHAPTER 4
MAY 13, 2005**

**NORTHWEST FOREST PLAN – THE FIRST 10 YEARS (1994-2003): STATUS
AND TREND OF POPULATIONS AND NESTING HABITAT FOR THE
MARBLED MURRELET**

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This reconciliation report is just for Chapter 4. A reconciliation report for Chapters 1, 2, 3, and 5, and Summaries and Abstract were submitted earlier, on March 18, 2005. The two reviewers who provided comments on Chapter 4 were divided on many issues, thus exemplifying the fine line of breaking new ground with experimental habitat relations techniques, and applying them to a rare, data-limited species such as the Marbled Murrelet.

Before addressing the two reviewer's comments, we reconciled a revised statistical review of Chapter 4. This was triggered when a paper by Keating and Cherry was published in the *Journal of Wildlife Management* (68:774-789; distributed in early January 2005). Keating and Cherry re-examined the use and interpretation of logistic regression in habitat-selection models. They concluded that logistic regression often is being misapplied in the habitat modeling literature. In light of this, Tim Max in consultation with the statistical review team identified the logistic regression issues that in general need to be addressed in case-controlled studies. Below, we reconcile this issue first, followed by reconciliation of the reviewer's comments.

Chapter 4

Second Statistical Review

After examining a paper by Keating and Cherry (J. Wildlife Management 68 (4): 774-789), Tim Max and the statistical review team concluded the following for "case-control" logistic regression approaches, an approach used in Chapter 4:

1. While the estimators for the coefficients associated with the independent variables have the expected properties (roughly unbiased for example), the estimator for the intercept does not.

2. The intercept is biased under this sampling scheme. For equal sample sizes of presence and absence sites the resulting bias in the odds that a site has presence over absence given a particular set of values of the independent variables is roughly a multiplicative factor of $(1-P)/P$ where P is the proportion of sites in the population that have presence. When $P < 0.5$, then the odds are overestimated ($(1-P)/P > 1$) and when $P > 0.5$ ($(1-P)/P < 1$) the odds are underestimated.

3. We can appropriately adjust the odds if we know P . But P is one of the things we want to estimate.

In summary, the estimate of the proportion of sites with presence is biased if one uses the usual estimates of the logistic regression coefficients based on the kind of data that was collected (a case-control approach) and the bias depends on the very parameter we are trying to estimate.

Reply:

Our use of logistic regression (LR) had two objectives: (1) select a set of variables to define murrelet habitat and (2) use LR to predict the amount of murrelet habitat. Keating and Cherry suggested estimating odds ratios as way of approximating unbiased estimates for case-controlled studies. In response, we changed our prediction model to estimate odds ratios relative to a reference condition (average habitat conditions of murrelet occupied sites). Steps in our revised methods are (1) adjusted prediction equation to output odd ratios, (2) estimated odds ratios of the grid inventory plots, (3) transformed the odds ratio using Yule's Q , which placed the odds ratios on a scale of -1 to 1, (4) used the transformed scale as a habitat suitability index, (5) plotted frequency diagrams of the transformed odds ratios, and (6) summed the area expansion factors of all the grid inventory plots within each frequency "bin". All tables and figures were revised accordingly.

Blind Peer Review

Reviewer #1

Comment: This chapter is great because it used both field and remotely-sensed data to assess habitat quality across a very large study area. That is not easy to do and use of the CVS/FIA plots was innovative. However, the chapter should clearly explain that the nesting habitat estimation was only for USFS lands in the plan area and BLM lands in Washington and Oregon. The National Park Service

and California BLM lands contain some important murrelet habitat that is not included in this chapter. Otherwise, the title and abstract are misleading. In the future, can FIA plots be set up on National Park lands or BLM lands in California to overcome this limitation?

Reply: The title was changed to include Federal land: "Estimating the amount of Marbled Murrelet nesting habitat using a systematic grid sampling strategy on Federal lands in Washington and Oregon". Also the abstract was changed to indicate ownership: "Our monitoring objectives are (1) determine habitat attributes that are associated with nesting of Marbled Murrelets and (2) estimate the amount of nesting habitat on U.S. Forest Service and Bureau of Land Management lands in the Plan area in Washington and Oregon." Similar additional emphasis was incorporated into the text.

Comment: Make it clear that this analysis is a site-scale analysis (except for distance to ocean, which is measured on a large scale) and therefore probably the resultant models will not be as predictive as an analysis that includes multiple spatial and temporal scales (see Meyer et al. 2004--citation below).

Reply: Emphasis on site scale added to text.

Comment: I recommend that future modeling efforts combine remotely-sensed landscape and regional variables (including fragmentation variables) with site-scale variables. The different spatial scales could be combined using hierarchical models (e.g., using Monte Carlo Markov Chain Bayesian methods-Gamerman 1997--citation below).

Meyer, C.B., S.L. Miller, and C.J. Ralph. 2004. Logistic regression accuracy across different scales for a wide-ranging species, the marbled murrelet. Pages 94-106 in S. Huzurbazar, editor. Resource Selection Methods and Applications. Omipress, Madison, Wisconsin.

Gamerman, D. 1997. Markov chain Monte Carlo: stochastic simulation for Bayesian inferences. Chapman and Hall, London.

Reply: Our study design for murrelet habitat modeling included landscape-scale/fragmentation variables which have been shown to be important habitat predictors in studies elsewhere. Because of deadlines and because we were directed to simplify our habitat modeling of murrelets, we reluctantly removed all potential fragmentation and large-scale analysis variables (except distance to coast) from our habitat modeling—keenly aware that it would weaken our models.

Comment: I like the authors' use of logistic regressions on occupied and absent sites to predict probability of occupancy. I also like the innovative use of the solar radiation index, although maximum temperature during the breeding season and

annual precipitation modeled from PRISM models could have been useful for more directly assessing the microclimate (easy to access at the Spatial Climate Analysis Service at www.ocs.orst.edu/prism).

I noticed that unoccupied sites were included in areas that are for the most part out of the marbled murrelet nesting range (too far inland--see Dillingham et al. 1995, Meyer et al. 2002, Hunter et al. 1998), at least in California and Oregon. (I do not agree with using the FEMAT 1993 definition of nesting range because that was an out of date estimate). Almost all observations of murrelet occupancy in California and southern Oregon have been in the fog-influenced vegetation zones (which are equivalent to the redwood-dominated vegetation zone in California and the coastal Douglas-fir-dominated western hemlock zone and Sitka spruce zone of Oregon--see map in Meyer et al. 2002). The structural characteristics of stands outside this zone are probably not what is precluding murrelet use of the stand. Instead, it is the warm, relatively dry climate that exists far from the ocean. For example, almost all the unoccupied plots in the Siskiyou National Forest and Six Rivers National Forest in Fig. 1 are outside this fog zone. Similarly, the majority of the unoccupied plots in central and northern Oregon are east of the occupied plots, suggesting they also might be outside the main nesting range. Use of these plots outside the apparent nesting range weakens the models and predictions unless a variable is included in the model that accounts for the fog effect (distance to coast does not quite do it). Future modeling work should account for these climatic constraints, which probably become less important in the more northern, cooler regions of the plan area (e.g., in Washington, occupied sites occur farther inland because there are more days of fog in the summer in contrast to the lower amounts of fog in Oregon and California--see Hardwick 1973 in Monthly Weather Review 101(10): 763-766 for fog frequency estimates in the US).

Reply: We developed a Zone-1 only model, and reported these results almost exclusively instead of using Zones 1 and 2. We agree that there are places where even Zone 1 needs to be adjusted, however it was not our role for this paper to make such changes. In the discussion section, we suggested that an evaluation of the range should be undertaken.

Comment: p. 3. Line 20. It says "80% of the Plan area had <50%....." Again, the entire plan area was not assessed, so this is not true (the plan area includes all land considered part of the Northwest Forest Plan, right?). Be more specific as to what area was assessed to give the 80%.

p. 3. Last sentence. Give percentages associated with the acres.

Reply: These statements were not included in the revision.

Comment: p. 7. Last sentence of first partial paragraph. Reference Moeur et al. in review as a report that tried to overcome that compatibility problem. The mapped classifications in that report can and have been used for a plan-wide murrelet habitat analysis (in Chapter 5). Do the authors think their approach for modeling suitable habitat in Chapter 4 has advantages or disadvantages over the approach in Chapter 5?

Reply: This was addressed in the revised Discussion.

Comment: p.7. Last sentence. See my comments about high contrast edge above for Chapter 2.

Reply: Reference added.

Comment: p. 10. Line 6. Explain the design of research studies with fewer visits and stations. How many fewer and how many stations per 120 acre area?

Reply: In our prediction models, “the probability of detecting Marbled Murrelet occupied behavior, if it occurs, is less than one. We accounted for this by modifying standard logistic regression equations for (1) number of visits (inequitable detection effort), (2) number of visits with occupied detections (inequitable detections), and (3) site size (inequitable detection rate due to scale), according methods in MacKenzie et al. (2002).” Because the reviewer’s comment is addressed in our model, there little need to add more detail here.

Comment: p. 10, Line 13. The pool of available surveyed sites is much higher than 2,000. Note that there were 9,362 stations in California and southern Oregon alone for 1991-1997 that were compiled by the USFS and used in Meyer et al. 2002, 2004.

Reply: The reviewer is mixing site and stations. A site is a collection of stations. Stations can be visited multiple times within and among years. Because of this, a collection of station visits over many years can give the impression that many surveys have been done, however only a small fraction of theses station visits truly represent independence at the site scale.

Comment: p. 10 Line 14. state "5 of the 10 physiographic provinces with the murrelet zones 1 and 2".

Reply: Correction was made.

Comment: p. 10 Line 20. What was considered an incomplete occupancy survey? It appears surveys with fewer visits and stations per site than the protocol were accepted, but the number considered acceptable is not stated. Define "site" in relation to the inland survey protocol so the reader understands that terminology as soon as it is used.

Reply: Below, as a reference, we provide survey criteria information from the Marbled Murrelet Inland Survey Protocol (Evans Mack, D., W.P. Ritchie, S.K. Nelson, E. Kuo-Harrison, and T.E. Hamer. 2003. Methods for surveying marbled murrelets in forests: a revised protocol for land management and research. 76 p. Pacific Seabird Group unpublished document available at <http://www.pacificseabirdgroup.org>). Our text was re-worded as follows:

A site consisted of ≥ 1 survey stations that are laid out together and which collectively are surveyed to determine the status of the site for Marbled Murrelets following this protocol.

.....
In developing our prediction model (see **Predicting Nesting Habitat Variables**), we removed 31 sites from the study because certain data attributes were not available from these sites to correct for inequitable detection effort among survey sites. Of the 169 sites used in our prediction model, 87 were occupied and 82 were absent (Table 1 and Figure 1).

“Survey Site. A **survey site** is the unit by which survey visits are designed and carried out, and the unit to which the requisite number of visits applies. We recommend limiting the size of the site to 61 ha (150 acres). The survey site boundary should not be confused with the management project or survey area boundaries. When the survey area is small ($< \sim 61$ ha), the site encompasses the entire survey area. In this case, the terms ‘survey site’ and ‘survey area’ are interchangeable, and the protocol applies equally. More typically, survey areas are large (> 61 ha), and should be divided into sites (Figure 3). Some flexibility is allowed in exceeding the 61-ha (150-acre) site guideline, but experience has shown that sampling intensity and coverage are compromised when the site exceeds 69-71 ha (170-175 acres).

A survey site contains ≥ 1 survey stations which are laid out together and which collectively are surveyed to determine the status of the site, which influences the ultimate status of the survey area. For the site, every station must be visited at least once and the requisite number of total survey visits to achieve the desired likelihood of classification must be planned per year to determine occupancy. For example, using the approach of at least 5, and up to 9, total survey visits per year to achieve 95% likelihood of correct classification, if a site contains less than 5 stations, more than one visit must be made to one or more of the stations (see ‘Distribution of Visits among Survey Stations’, p.16). If the site contains more than 5 stations, the site will receive more than the minimum 5 visits per year. Individual survey sites within the same survey area may be visited on the same or consecutive days, but survey visits within a survey site generally should be separated by a minimum of 6 and a maximum of 30 days (but see ‘Distribution of Visits Throughout the Season’, p. 17, for exceptions).

It is critical that each site be identified by a unique name or number and legal description or UTM or lat/long location that will identify that particular site over the years. Furthermore, the boundary of the site must be clearly delineated on a topographic map or aerial photo. Stations within sites also must have unique identifiers, but in addition, all stations within a site must share the same site name. It must be

unquestionably clear which stations belong to a site, as there is no other way of determining if the site was surveyed with the requisite number of visits. Multiple sites within a survey area should share the same area name. Figure 3 illustrates one example of a naming convention, which uses alpha-numeric codes in a hierarchical fashion to identify stations, sites, and areas.

Survey Stations and their Placement

Survey station placement is one of the most crucial aspects of survey implementation. Marbled Murrelets can be difficult to detect in and around their breeding areas, in part due to their small size, rapid flight, cryptic plumage and crepuscular behaviors. Where the likelihood of detecting murrelet activity is low, such as where a small number of birds are nesting due to small stand size or extreme distance to marine waters, good station placement is imperative if murrelet use of the stand is to be correctly classified. O'Donnell (1995) reviewed the effects of station placement on the number of murrelet detections and found that the number of visual sightings of murrelets is strongly influenced by the location of the observer. The use of radar in recent studies also has demonstrated that observers could miss a large number of murrelets in some areas. Concurrent radar and audio-visual surveys in the Santa Cruz Mountains and on the Olympic Peninsula found that ground observers missed 71-100% and 77-90%, respectively, of the murrelets detected on radar, even when provided with the birds' bearing and travel direction by the radar operator in the California study (Cooper and Blaha 2002; Singer and Hamer 1999). Thus, sensible placement of survey stations can help overcome site characteristics that may limit the observer's ability to hear or see murrelets.

There are three steps involved in station layout. The first step is to determine adequate coverage and establish preliminary station locations. This can be accomplished by overlaying circular mylar disks on aerial photos and topographic maps. This is detailed in 'Number of Survey Stations' and 'A Simple Technique for Delineating Site Boundaries and Determining Station Location' (p. 12). The maps and photos are used to identify topography; openings or gaps in the canopy; patchiness of habitat; and natural and artificially-created flight corridors such as streams, lakes, rivers, meadows, avalanche chutes, landslides, paths, and roads. Local knowledge of the area is helpful, but not essential, at the initial design stage.

The next step is to locate the stations on the ground and refine their placement based on site-specific factors. This may help to identify openings that were not evident on aerial photographs, or identify potential sources of localized noise disturbance. Because of the high proportion of audio detections during most surveys, placing stations near sources of loud noises, such as busy roads, is less optimum than a quieter location covering the same area. The ground visit also could identify patches with the most suitable murrelet nesting habitat, such as areas with the highest density of potential nest structures. On-site review allows these locations to be factored into the survey design. Other considerations when placing stations include the growth and foliation of adjacent vegetation, increase in snow melt runoff when locating stations early in the spring, and the viewing window. Openings in the forest canopy and along the perimeter of forest stands offer the best opportunities for viewing murrelets. Chances of detecting murrelets

flying silently are increased dramatically if the birds are viewed against a light or bright sky as a background, which silhouettes the birds in the early dawn light.

A third step is not always necessary, but often overlooked. This involves the addition of new or supplementary stations that may or may not conform to the minimum requirements stated in the protocol. These additional stations may improve the surveyor's opportunity to detect murrelets in a difficult setting. Additional stations also can be added after surveys have begun, where detections indicate potential activity in a portion of the survey area receiving minimal coverage under the existing survey design. For example, once presence has been detected and the objective is to determine occupancy, supplementary stations can be added to augment the data previously collected. Such a station could be one that affords a good view of the target stand but is greater than 50 m from its edge. Stations could also be surveyed in tandem, with one observer placed adjacent to a stream that has good visibility but limited hearing, and a second observer at a station with quiet conditions. Note that two stations surveyed in tandem counts as only one protocol visit for the site.

Guidelines on station placement are intended primarily for management scenarios. Surveys designed for research purposes may follow the general principles outlined herein, but likely would deviate somewhat to meet the research objectives."

Comment: p. 12. Under Site habitat data- Indicate here how the platform data were used in this study (it is not used in the models). It seems that future FIA/ CVS monitoring should include platform density estimates and observer error expected with those estimates.

Reply: The platform tree data that the reviewer refers to was not useful at this stage of the monitoring because this type of data was available from only a small subset of the CVS plots. Hence, we could not include such variables in our prediction equation. It makes no sense to begin explaining in the text why we did not use certain variable when the reader has no reason to expect the use of such variables.

Comment: p. 13. Distance to coastline may not be as predictive as distance to nearshore marine areas with high average chlorophyll concentrations ($>10 \text{ mg/m}^3$, see Meyer et al. 2002), which is an indicator of marine primary productivity. The chlorophyll data can be obtained from Coastal Zone Color Scanner and SEAWIFS web sites and averaged over all the years collected to map out the relative differences across the coastline. Future modeling efforts could include this variable.

Reply: We appreciate the suggestion for future monitoring.

Comment: p. 15. Last sentence of middle paragraph. What was the pixel size? Could one resample the occupied and unoccupied plots at the same small scale as was used for the FIA and CVS plots to see if changing the scale makes a difference?

Reply: Pixel size was added. The field vegetation plots at the occupied/absent were placed randomly across the site, and were assumed to be a representative sample of the entire site. It's not clear what would be achieved by taking a small and incomplete sample of a site just to match area sampled over the much smaller CVS grid plot locations (1 ha). A difference, if it occurred, would be from too small of a sample from a site.

Comment: p. 16. p. Line 4. What was the average spacing of the plots for wilderness vs. non-wilderness areas? Also, the paragraph needs work (has typos) and is confusing as to what was done with subplots vs. PSUs to calculate area expansion factors.

Reply: In our paper, we referred to Moeur et al. concerning the detailed methods on expansion factors, which are not necessary to repeat in this paper. The reworked the paragraph to address the confusion.

Comment: p. 18. Under surrogate attributes of tree platforms. Figure 3 should also show how the dbh-nesting platform relationship differed among tree species.

Reply: Tree platform relationships can vary by tree species, however these data will be covered in a separate paper in the future. Because these data are not used directly in this study, there is little need to get side-tracked into analyses of tree-platform data.

Comment: p. 19. Under Habitat variables. Table 2 should include BA30low and BA30high if they were also variables used in the candidate models. Did these two variables substitute for BA30 or was BA30 still used in some candidate models? Also, Table 8 has other variables like BA25, BA76Low, CDEN25, that are not in Table 2. Did the authors forget to convert to inches in the variable name? Also, why not have mutually exclusive variables, which will be MUCH easier to interpret and less correlated, such as BA10-30 inches and BA>30 inches, rather than BA>10 and BA>30? In the abstract for this chapter, it says sites had more basal area of larger-diameter trees (>30 in dbh), but BA76Low (which I assume is really BA30Low) is the variable in the selected model. Please explain how the interpretation of the results differs with BA76Low in the model rather than BA30. Also, were BA>10 and BA>30 in the same candidate models and did that cause multicollinearity? Were measures taken to ensure candidate models did not have high multicollinearity?

Reply: The recommended additions to Table 2 were done. BA"76" was a typo left over from converting the variable names from metric to English units. Only BA30High and BA30Low were used. As only the predictions from the models were used, no interpretation was performed on the coefficients in the models. Therefore, multicollinearity is not a concern. (Had there been interpretations of the coefficients, then multicollinearity would certainly be of concern.)

Comment: p. 20. Top of the page. It seems that the dominant tree species should have been included as a variable, rather than, or in addition to, BA30low or BA30high because the relationship between platforms and dbh varied by species (e.g., murrelet habitat in old-growth redwoods has a larger dbh compared to old-growth Douglas-fir). Or maybe just a binary variable for redwood vs. non-redwood is needed.

Reply: Certainly there are a few plausible approaches we could have taken, the one take was recommended by the biometrician assigned to assist with our analyses.

Comment: p. 21. First sentence. Note, MacKenzie's model is PRESENCE, not PRESENSE.

Reply: corrected

Comment: p. 21. I suggest showing the 30 candidate models in a Table. The recommended procedure used by many ecologists these days (see Anderson et al. 2001 in J. Wildl. Manage. 65:373-378) is to give the AIC_c and delta AIC_c and Akaike weights for each *a priori* model in a Table, so the reader can compare them. Anderson and Burnham don't like all possible subsets because they consider it data dredging that may lead to spurious results. But if the models have good measures of global fit (see Eberhardt 2003 in J. Wildl. Manage. 67:241-247) or prediction accuracy on independent data, I think the approach in this chapter would be OK. Unfortunately, the authors don't use independent data, either, except in a K-fold crossvalidation (see comments about that below). Couldn't the model be applied to known occupied sites or nest sites not in the calibration dataset to test the prediction accuracy on independent data? Perhaps there are some occupied or unoccupied sites on FIA/CVS sites that could be used. That would be a truer test of the model's performance. Or else, rerun and develop the regressions on 80% of the data and test it on 20% a number of times and see how it performs (randomization method in Fielding and Bell 1997 in Environmental Conservation 24:38-49). Did any models assess variable interactions or non-linear relationships?

Reply: A new table with 30 candidate models was provided. Because of the limited amount of data, no interactions were considered. We did examine potential non-linear relationships with generalized additive models (GAM's) with the top model and found no evidence of non-linear relationships.

Comment: p. 22. No. 3. Although many ecologists use 0.5 as the cutpoint in logistic regressions, it probably is not the optimum cutpoint when there are unequal sample sizes for occupied and unoccupied sites. Also, note that prevalence (proportion of all sites that are occupied) affects prediction accuracy (see Manel et al. 2001 in Journal of Applied Ecology 38:921-931 or see Figure 1

in Meyer et al. 2004). Use of either concordance, Somers'd, Kappa statistic, or AUC of ROC plots does not depend on prevalence or cutpoints and would be a better measure of predictability of the models than prediction success using a confusion matrix (see Fielding and Bell 1997).

Reply: In addressing the statistical issues of logistic regression for case-controlled studies and switching from probabilistic outcomes to odds ratios, this comment does not apply anymore.

Comment: The authors use the k-fold crossvalidation recommended in Boyce et al. 2002 but the method is recommended for use sites vs. random sites (or available sites), not use vs. non-use. Boyce et al. 2002 also recommend using a Spearman rank correlation to show the fit, which is not given in this chapter for some reason. Is that because the authors do not believe r_s the best measure of the goodness of fit (following Hirzel et al. 2000--in his Biomapper manual)? Please explain why this crossvalidation technique and the chi-square goodness of fit statistic was chosen over other methods.

The 72% accuracy in CA, 78.8% in OR, 75.9% in WA, and 76.5% for the entire area in Table 9 is not as good as it seems. Note that one could get 70% accuracy for CA, 59% in OR, 52% in WA, and 59% for all states with just a constant (no variables) in a logistic regression using these data because of the ratio of occupied to unoccupied sites for each of those. With just a constant, the logistic regression classifies all of the sites as occupied or unoccupied, depending on which class is larger, so the larger class is 100% correctly classified. Therefore, the model is weak for California, which I think is because over half of the unoccupied sites and ALL of the FIA sites appear to be outside the main nesting range of the murrelet.

Reply: Including k-fold cross validation in our methods was an oversight: at one time this method was proposed but later it was dropped. Step 4, where the method was identified, was removed from the methods.

We revised the Discussion section to address the uncertainty of “accuracy”, as follows:

Our models correctly predicted occupancy of the actual survey location data at ~75% (Table 11 B; Zone 1-Only model), a modest improvement over chance alone. Our prediction models, however, were focused on distinguishing habitat differences between sites (occupied and absent) that probably were more similar than dissimilar. According to protocol (Evans-Mack et al. 2002), only sites potentially suitable for nesting were surveyed, thus increasing the likelihood that habitat characteristics overlapped broadly between occupied and absent sites. Whereas, if the occupied and absent survey sites had been selected from a much broader pool of forest conditions, including

improbable nesting sites, the level of prediction agreement certainly would have been much higher. Potential false absences in the survey data, that is, an absent site is suitable for nesting but a Marbled Murrelet is not detected, also may have affected the prediction agreement. An “absent” site may not be occupied for nesting because of factors other than habitat conditions, including limited food resources near nesting habitat.

Comment: p. 25, no. 1,2,3. I like this approach of using bootstrapping to estimate standard errors. However, it is unclear where the results of this bootstrapping are presented--just Table 12? Were the standard errors in Table 8A bootstrapped? Can information be given as to the amount of variability attributed to 2 vs. 3? As for no. 3, were not all the grid FIA/CVS inventory sites available used to predict potential nesting sites, rather than a random subsample of the sites? If so, I don't understand where the variability in 3 comes from.

Reply: The only bootstrapping results were presented in table 12. In reference to table 8A, we estimated the sampling error associated with the random selection of grid inventory plots. We are unable to split out the relative proportion of the standard error estimates between sources 2 and 3.

Comment: p. 26. End of first partial paragraph. My version of the report only has Table 7, not Table 7A and 7B. (it is also missing 7C, which is mentioned on p. 27). It was unclear in the description of the methods that the detection rates would be variables in the model. If that is true, I would add those variables to Table 2. Did accounting for detectability with the PRESENCE model change your results much from not accounting for it?

Reply: The table numbers were typos, not missing information; they were corrected.

Comment: p. 38. Last sentence of second paragraph. I agree. Fig. 1 shows zone 2 only has unoccupied sites. This is a good reason not to include zone 2 because it is probably beyond the bird's main nesting range if occupied sites have not occurred there (there may be exceptions, but probably not important enough for modeling the major nesting habitat). As said earlier, I'd suggest even removing the eastern part of Zone 1 as not suitable habitat in California and Oregon (based on the fog zone), and then build the model to get a good, predictive model for within that nesting zone.

Reply: Future modeling

Comment: p. 29. Top. Where are the k-fold crossvalidation results?

Reply: (Same as above)--Including k-fold cross validation in our methods was an oversight: at one time this method was proposed but later it was dropped. Step 4, where the method was identified, was removed from the methods.

Comment: p. 29. Under estimating amount of potential nesting habitat. Can a map showing probability classes for the areas evaluated be created? That would help the reader assess how reasonable the model might be spatially. For example, does the model predict suitable habitat (>0.5) in California and southern Oregon outside the fog zone? (it shouldn't) Very little good murrelet habitat in California has FIA plots because most of the birds occur on national and state park lands and private lands in California. General statements comparing habitat amounts between states are misleading without considering other lands than those with FIA/CVS plots.

Reply: We developed a non-spatial model that analytically is not capable of producing maps. We are capable of making area estimations at varying geographic scales (e.g, physiographic province, zone, and state), but are not able to pinpoint where the potential habitat occurs within these geographic areas.

Comment: The discussion seems light on comparing results with other studies. Zharikov et al. (in press) suggests warmer sites are better for nest sites in British Columbia, but Meyer et al. (2004)(in Wilson Bulletin 116:197-286) found cooler temperatures were important for occupied sites in California and Oregon. The cooler temperatures and foggy environments in the relatively warmer CA and OR areas may improve epiphyte growth, but as one moves north to BC, cooler temperatures may limit epiphyte growth due to a shorter growing season (Zharikov et al. in press). Also, there is little discussion about the structural variables and comparing those results to other studies. I'd also like to see more discussion on how the model can and should be improved per my comments above.

Reply: The Discussion section was expanded, including how the model could be improved.

Comment: Table 3 gives predicted probability of being suitable. Was this calculated individually for each variable using univariate logistic regressions?

Reply: Table 3, during the revision process, shifted to odds ratio-based information. The logistic regression models were multivariate.

Reviewer #2

Comment: Data reported – Sample sizes for some analyses were inexplicably small. The number of habitat parameters considered was quite small, with a high degree of intercorrelation, which appears to produce some strange results. Most obviously, the model results strongly depended on solar radiation as a predictor, which was not supported by raw data or the results from Chapter 5. It was difficult to assess how well the models fit the habitat data, and the models had modest success (75%) in predicting occupancy of sites. The discussion focused too much on the broader Plan Area rather than on Zone 1 in which all the occupied sites were located.

Reply: Although the above comments are covered later and more specifically, we addressed the above concerns: the value of solar radiation variable as a predictor variable was defended; intercorrelation of predictor variables do not cause analysis problems when making estimations as we did in this study, whereas intercorrelation of predictor variables will cause serious problems when making cause and effect predictions; we explained the circumstance for the level of success of the modeling; and we removed most of the Plan Area results from federal lands, focusing the results just on Zone 1.

Comment: Conclusions – The report makes several conclusions about habitat associations which are clearly contradicted by their own data. There is little attempt to compare the results of this analysis with the many other studies of landscape-level habitat association. The report does not adequately address the shortcomings of the analysis, or suggest how the analysis might be modified to work as a monitoring tool for all murrelet habitat in the NWFP.

Reply: All of the above were addressed in the revisions.

Comments: Changes to the program – I think this was a valid exercise to test a new method, but the results and the report interpretation give no confidence that this method should be pursued any further, unless the major problems that have been identified can be rectified. Serious consideration should be given to removing this chapter from the overall report. Alternatively, address the problems and limitations of the analysis and include it to show that alternative approaches have been tried.

Reply: Most of the above comment is likely related to the use of the solar radiation index variable which we defend below.

General comments on Chapter 4: I found this chapter disappointing. The method of analysis is new and potentially valuable, but the analysis, results and interpretation are unconvincing and in some cases erroneous. Overall, the chapter is likely to add confusion to understanding murrelet habitat rather than

clarify it. There is a lot of overlap between the goals and products of this chapter and Chapter 5 and the latter is clearly a much better analysis.

Reply: Unfortunately, this reviewer was confused about the multivariate aspect of the prediction model, and we were to some degree responsible for the confusion. In the comments, this reviewer made univariate interpretations of the models, even though the models were multivariate. Because of this confusion, we inserted additional emphasis throughout the paper that we used a multivariate approach.

Agreed that there is overlap between Chapters 4 and 5, and their objectives; the intent of Chapter 4 is more experimental, taking on the objectives using untried methods. During our revision, the experimental nature of our study was heightened.

Comment: In a nutshell the problems with Chapter 4 are:

- it samples only a portion (never explicitly estimated) of available habitat within the NWFP, does not include all federal lands and excludes all non-federal lands;
- it uses small samples once the data are broken down into states and provinces (Table 1), despite the apparent wealth of available data (>2000 sample points);
- it uses habitat variables which are frequently intercorrelated (Table 4) which might lead to spurious inclusion of variables (see point 14);
- much of the focus is on the Plan Area as a whole, including the inland Zone 2 in which there were no sites with occupancy. This gives undue prominence to some potentially spurious features (distance from sea) perhaps at the expense of more predictive measures (e.g. size of trees);
- one can't assess the overall strength of the model from AICc values and it is therefore difficult to assess how well the models fit the data;
- the model selects about ¾ of occupied sites (but samples are small Table 9), and it is not clear whether misidentifying 25% of possible occupied sites is acceptable within the NWFP;
- the model results strongly indicate the importance of solar radiation, which is not supported by raw data or the results from Chapter 5 (see point 14);
- there is little attempt to compare the results of this analysis with the many other studies of landscape-level habitat association;
- the issues raised above create doubt that this procedure is a reliable and repeatable method for identifying and mapping habitat, or monitoring habitat change in the NWFP.

These points are expanded here and details are given in the numbered points below.

Reply: as the reviewer mentioned these point are covered below.

Comment: Restricted land base. The modeling focuses exclusively on federal lands, but a substantial portion of murrelet nesting habitat in the Forest Plan area is on non-federal lands (see Table 6A and data in Chapter 5). Even on federal lands, it appears that the analysis left out National Parks and some BLM lands (page 14). The analysis and hence interpretation is thus severely limited in its spatial scope. This seems to be a serious weakness in this modeling approach and in its possible application for monitoring. Some interpretation or at least discussion is needed on how the model or results might incorporate or be applied to non-federal lands. We at least need to be told what % of estimated total murrelet habitat in the NWFP was included in this modeling exercise.

Reply: It was not our charge to test our experimental model on all lands in the range of the murrelet. We clearly stated that inventory plot were not installed on National Park Service lands and BLM lands in California, hence we could not make inferences to these areas. We do not agree that inferences on 6 million acres of federal land are “limited in scope”; it’s quite rare in the wildlife literature to be making inferences that are as broad in scope as our study. There is no reason to make speculations to nonfederal land beyond our model’s capabilities, as suggested. Clearly shown in Table 6 are the estimated total lands broken down by various attributes (state, province, zone, land use allocation) inside the Plan’s range of the murrelet.

Comment: Small samples. There were apparently >2000 sites available but only 200 were used (see point 3). The result is very small samples within some states and physiognomic provinces. This puts doubt on the results, especially in those states or physiognomic provinces with inadequate samples.

Reply: Although >2000 sites were available, it did not mean that they were immediately available for use in a prediction model. Ground-based vegetation sampling had to be done at each site that was selected. Because of budgetary constraints, we could only sample 200 sites, which is a reasonably large undertaking dispersed over three states, and often in very remote locations. We agree that the sample size in any one province might seem small, but our models were developed using data from all provinces together. Even with all provinces combined, we agree that the sample size as a whole is small. We discuss this point in the Discussion section in the revised manuscript, and make suggestions to improve the largely experimental model by increasing the sample size.

Comment: Intercorrelations. The number of variables used is relatively small and a large proportion are significantly intercorrelated (over half of all possible combinations – see Table 4). This seems to be glossed over, but is a serious issue. The models presented (Table 8A and B) seem to involve a rather simplistic, step-wise inclusion of variables, with no attempt to compare competing models which include or exclude highly correlated habitat variables. The totally unconvincing prominence of solar radiation in the models seems likely to result from its intercorrelations with more meaningful habitat variables.

Reply: Intercorrelations are not considered in estimation modeling, although knowledge of them can be useful in interpreting results.

Comment: Assessing model strengths. The AICc is a valuable measure for comparing competing models applied to the same database, but the AICc values themselves give no indication of the overall strength of the model, relative to other models using other data and done in other studies. There is therefore no way to assess how well these models fit the data or perform as predictors, except for the comparisons with occupied and absent sites (Table 9). Here the models appear to perform moderately well (overall about 75% accuracy in predicting occupancy), but there is no comparison with other studies or other models to assess whether this is good or poor.

Reply: No reply needed.

Comment: Solar radiation. Both models relied strongly on solar radiation (Table 8B). This is potentially an exciting and novel parameter, but when one looks at the raw data, the predictions of the model, and the alternative analysis in Chapter 5, it is clear that the inclusion of solar radiation is a statistical flaw, and that this parameter really tells us nothing about the probability of occupancy by murrelets. This then casts doubt on the entire analysis and in my view invalidates the entire exercise. See points 10 and 14 below.

Reply: Apparently, the reviewer assumed that solar radiation's contribution was univariate rather multivariate. In the Discussion, we provide separate test result with and without solar radiation in the prediction model that demonstrate the importance of the solar radiation to our multivariate models.

Comparison with other studies. One of the stated goals of this analysis (p. 6) was to determine what habitat attributes are associated with nesting Marbled Murrelets. In my view this analysis fails in this goal, because the results are inconsistent and there is little attempt to compare these results with the many other landscape-level habitat analyses that have been done for Marbled Murrelets. There is very little discussion comparing these results with other studies.

Reply: The inconsistency issues have been discussed above (solar radiation). Our primary goal was to determine key habitat attributes so that we could estimate the amount of suitable habitat. Chapter 5 used mostly the same habitat attributes as Chapter 4, and Chapters 5 and 2 discussed the habitat attributes of other studies.

Applicability to monitoring. The problems arising from this analysis, plus the existence of a more robust alternative (Chapter 5), suggest that this modeling

procedure is not likely to find acceptance among other murrelet researchers or the informed public as a reliable and repeatable method for monitoring availability of murrelet nesting habitat in the NWFP. It seems unnecessary complex, restrictive in application, based on too few variables, and produces results which are not consistent or defensible.

Reply: We believe that the experimental method has more potential than this reviewer believes. That potential is covered in our revision to the Discussion section.

Other general points. The terminology applied to Zones is confusing. Within the NWFP there are six Zones 1-6. Here Zone 1 and 2 mean something completely different and it takes a little reading to sort this out. I suggest giving these inland and coastal areas a different name – Regions perhaps? Or call them Coastal and Inland zones.

Reply: Unfortunately, we are stuck for now with the terminology that was given in the Plan. We suggest in the Discussion that the dual Zone concept should be phased out.

Specific comments on Chapter 4

Comment: 1. Undue focus on entire Plan area including inland Zone 2. In the abstract (p. 3 bottom) and throughout the text you provide details from the entire Plan area, but it is clear from your results that the model and management are best focused on the coastal Zone 1 for which only sketchy details are provided in the abstract. I suggest stating something like this in the abstract, and providing more detail on how the model applied to this zone.

I think including the inland Zone 2 data add little to understanding murrelet habitat because your own data show that murrelets were not routinely found in this zone (Fig. 1) and all the predicted habitat falls within Zone 1 (Table 11), and you therefore artificially raise the importance of variables like distance from the ocean. This is addressed in the discussion (p. 33 para 2) but somehow too much focus still seems to be on the total Plan area, including the inland Zone 2.

Reply: Zone 2 was removed from the analyses, except to demonstrate why we should remove Zone 2 from our models.

Comment: 2. p. 8 near bottom and p. 9 near bottom. It is not clear here whether the analysis used presence (all detections of murrelets) or occupancy (sub-set of detections showing likely occupied behavior). On p. 8 the text indicates all detections (i.e., presence) but later in the paper it is clear that only occupied sites were considered. On p. 9 presence and occupancy are incorrectly treated as

synonyms. These terms have been clearly defined (Paton 1995, Evans Mack et al. 2003) and need to be consistently applied to avoid confusion.

Reply: Where the word “presence” was used we replaced it with the word occupancy.

Comment: On p. 10 line 7 it would be useful to provide a short summary of what behaviors indicated occupancy. Be sure to indicate whether circling above the canopy was included as a measure of occupancy since this is sometimes but not always included. Ralph et al. (1994) is somewhat outdated in this context.

It would also be useful to include some assessment of the limitations and potential errors in determining occupancy and absence. This is briefly discussed in Chapter 5 (p. 20) but not in this chapter which relies more strongly on these designations.

Reply: The inland survey protocol is readily available on-line, as indicated in our literature cited. We prefer not to repeat the details of that protocol in our document, nor it necessary.

Comment: 3. p. 10 line 13. It is not clear why such a small sample (200 sites) was selected from the large data set available (>2000 sites). This sample is further reduced by taking out incomplete surveys (leaving 87 occupied and 82 absent), and once these are divided up into various zones and provinces the sample sizes become small and inadequate (see Table 1). Why not use sufficient samples to give the analysis of zones and provinces some rigor?

Reply: We addressed this comment earlier. The reviewer didn't understand why we only used a small fraction of these sites (reasons explained earlier); we hope that did not taint the remainder of the review.

Comment: 4. p. 11 line 7. “nonfunctional nesting habitat” is an oxymoron. Better as “habitat unsuitable for nesting” or something like that.

Reply: Good point. We fixed it.

Comment: 5. p. 12 line 2-3. Give the % represented by the 27 sites >150 ac and 12 sites >175 ac.

Reply: Added percents.

Comment: 6. p. 12 lines 11-13. I suggest deleting this section. I don't see this breakdown of platform size anywhere in your analysis, and I doubt whether ground observers could accurately detect 2 inch differences at the top of a 40 m tree. Just stick to the basic definition of a platform.

Reply: We summarized the methods in the field protocol.

Comment: 7. p. 15 bottom and p 16 top. This explanation of how PSU data were extrapolated to surrounding areas is inadequate and confusing. What is meant by “contribute”? Why were areas surrounding the PSUs different - were they isolated stands surrounded by clearcuts? Give some examples to make this clear.

Reply: We added text to clarify. It is evident from this comment that this part of our modeling process (using the prediction model to estimate acres of habitat from the inventory grid) was not understood very well. We re-organized the manuscript into two steps, (1) developing the model and (2) making predictions, to improve clarity.

Comment: 8. p. 18 lines 3-4. Best to call these potential nesting platforms. In most cases platforms are defined on diameter plus height above ground (see Chap 2 p. 11), and without assessing cover or epiphyte cover (which is impossible to correctly assess from the ground).

Reply: done.

Comment 9. p. 19 top para. Nice explanation of the application of data in the analysis.

Reply: thanks.

Comments 10.: p. 19 middle para. I think the failure to statistically test differences in habitat features between occupied and absent sites was a mistake. Surely this is an essential step in verifying the likely importance of these features as univariate measures of murrelet habitat. Such an analysis would also have revealed the inconsistent and confusing way in which solar radiation varies among these sites. See point 14 below.

I find the argument in the last sentence of this paragraph unacceptable. I agree that combinations of habitat features are more likely to provide reliable indicators of habitats than single variables, but that is not the way the chapter treats these data. In the very next paragraph you make a univariate comparison between occupied and absent sites. In the abstract (p. 3 lower) and later in the text (e.g. p. 27 bottom, and explicitly on p. 29 top), the way you describe the various measures strongly suggests that each variable influences murrelets on its own. Most readers would interpret the text in that way. That is clearly not true for all the variables suggested, especially for solar radiation. On p. 34 (middle) you explicitly state that “murrelets are more likely to nest in cooler environments”, which is a direct contradiction of some of the raw data presented (see point 14).

Reply: The reviewer, indeed, found some places where the information was presented such that the reader would be interpret the results from a univariate perspective. We have corrected this, and are thankful for the reviewer catching this. The solar radiation variable issues are reconciled elsewhere in this document.

Comment: 11. Tables 3 and 4. The data for Basal area >30 in (BA30) are missing from both tables. This is unfortunate because it appears to be the most important variable driving the analysis, making it difficult for a reviewer to assess the raw data. The discussion and conclusions center on BA30 and not the rather confusing separation into BA30Low and BA30High, so we need to see the actual data and correlations for this measure.

Reply: Data on BA30High and BA30Low were added.

Comment: 12. p. 20 top paragraph. This breakdown of the BA30 data is confusing and poorly explained. Why are these dummy variables necessary? Does zero mean “not considered” or was the numerical value zero entered into the data? I’m always suspicious of models which enter the same set of data in several different ways – they are clearly not independent measures. Any improvement to models in doing this seems to result from statistical serendipity and has little to do with biological reality.

Reply: Explanation was improved. As far zero goes, it means zero and not means “not considered”. This has nothing to do with independence but rather fitting a functional form with as few parameters as possible.

Comment: 13. p. 21 line 3 PRESENSE or PRESENCE ?
p. 22 line 4: performed not preformed.
p. 22 point 4. Confusing grammar, long sentence, and mixed tenses within sentences. Fitted is better than fit.
p. 24 end of top paragraph – nice to see this background explanation to habitat use.

Reply: edits done. The original point 4 was removed because we did not do the “k-fold”.

Comment: 14. Solar radiation (p. 27 bottom and elsewhere). I simply don’t buy the argument that occupancy is more likely at sites with lower solar radiation (even if it acts as a proxy for aspect and slope), and the prominence of this variable in the models makes me skeptical of the value of the entire modeling process. Consider these points:

- For the total plan area, the difference between occupied (26612.7) and absent (27108.0) sites amounts to a difference of 0.35% (Table 3);

- Mean solar radiation for occupied sites is higher than absent sites in Oregon (Table 3);
- The radiation for sites with >50% probability of being occupied is higher than for the sites with <50% probability (absent sites) for the Plan as a whole and for each state (Table 3).
- In the analysis done in Chapter 5, solar radiation was included but failed to contribute significantly to the models;
- In Table 3 of Chapter 5 we see the same marginal differences in solar radiation between occupied and global sites, plus the same inconsistencies (radiation at occupied sites is higher in CA, and slightly lower in OR and WA).
- At occupied sites, solar radiation shows a positive significant correlation with other habitat features, notably BA30 (Table 4), suggesting that the inclusion of solar radiation might be as a spurious proxy for more meaningful habitat measures.

How can one have confidence in models based so strongly on such inconsistent, trivial and almost certainly non-significant differences?

Reply: In the Discussion, we provide separate test result with and without solar radiation in the prediction model that demonstrates the importance of the solar radiation to our multivariate models.

Comment: 15. p. 28 lines 1-2. Where do these AICc values come from? They don't seem to relate to the model data presented in Table 8A or 8B.

Reply: We added a new table, Appendix Table 9 with AICc values for the first 30 models selected.

Comment : 16. The AICc values are useful for comparing competing models applied to the same data, but they give no information on how well the models are able to predict occupancy or how they might compete with different models using completely different parameters. Is there some universal objective rating (like R-squared) that can be applied to indicate how well the models are performing?

Reply: The confusion matrices (accuracy of predictions) show how well the models work.

Comment: 17. p. 29 lines 4-6. Here you compare occupied vs. absent probabilities using selected parameters (distance to sea and BA30), but don't include solar radiation despite its importance in both Plan and Zone 1 models. Obviously such comparisons would be embarrassing, because the trend in solar radiation is the opposite of what was predicted – see point 14 above.

Reply: The paragraph was not part of the revision so the comment is no longer applicable.

Comment 18. p. 29-30. Many of these results are relatively trivial and well known before this modeling exercise, for example the broad-scale distribution of likely murrelet habitat relative to conservation zones and proximity to ocean.

Reply: The results section was completely revised.

Comment 19. p. 33 line 2. It would be useful to give more details on the “original estimates” of habitat in the Plan area, either a map or table comparing previous estimates with these new estimates by conservation Zone etc.

Reply: Our estimates using odds ratios relative to known nesting habitat in suitability classes are not comparable to the Plan estimates.

Comment: 20. p. 33 last 4 lines. The effect of distance from the sea is strongly influenced by the spatial scale involved. At fine scales, murrelets are known to avoid shoreline forests (see review Burger 2002), and nests and habitats do not show simple linear relationships with distance within the likely commuting distance from the sea (approx. 30 km) – see nest distributions in Hamer and Nelson (1995: Fig 1). It is more likely that there is a distance threshold beyond which murrelets seldom venture, but within which there is a relatively uniform distribution of nests, depending on habitat availability. Your model, showing a negative relationship with no definite predictive curve or threshold, does not really contribute much to our understanding of how distance inland affects the probability of murrelets nesting.

Reply: What the reviewer suggests on uniform distribution is logical; however, it has never been proven to our knowledge. The GAM that was fit with the top model did not indicate anything beyond a straight-line fit.

Comment: 21. Literature Cited

These papers cited in the text were not included in the reference list:

- FEIS 1994
- Palmer et al. in press
- Connelly 2004
- SAS Inc.

p. 37 include the acronym FEMAT in the citation for Forest Ecosystem Management Assessment Team 1993 to make the connection with FEMAT (1993) given in the text. Other acronyms used in text citations also need to be included in the reference list (e.g., USDIFWS 1992).

Remove first names for Max et al. 1996.

Reply: done.

Comment: 22. Table 2. Change Mean number of conifer stems to Mean density of conifer stems.

Reply: done

Comment 23. Table 3. Caption – last line. I think this should read “All variables have the same sample size”

It would be useful to add the codes for each habitat variable (from Table 2).
Data for Basal area conifers >30 in are missing.

Reply: fixed.

Comment 24. Table 4. Why put parentheses around the asterisks?

Reply: fixed

Comment: 25. The labeling of Tables is confusing. Tables labeled A and B often seem to have little in common (e.g. Table 8A and 8B) – why not just call the second one Table 9?

Labeling Table 10 as an Appendix is also confusing – call it Table 10 or Appendix 1.

Reply: We will work with PNW editors on this.

Comment: 26. Table 8A & 8B. The habitat codes used here (metric) don't match those used in the rest of the report.

Reply: Fixed

Comment 27. Table 11. Is it possible to put confidence limits around these estimates of habitat area?

Reply: Table is not in the revised version.

Comment 28. Table 12. Caption - Replace Primary with Plan Area.

It is puzzling that Table 11 shows no predicted habitat for Zone 2, but this table shows 52,000 acres.

Reply: Table is not in the revised version.

Comment 29. Table 10. I think you could save paper by deleting all the Plan Area models.

Reply: Plan Area results were removed.

Comment 30. Figure 3. Add units to the x-axis label.

Reply: done.

Comment 31. Figures 5-10. These figures need more descriptive captions explaining what is shown in each graph. Again I think the Plan Area graphs are redundant.

Reply: These figures are not part of the revised version.

Reviewer #3

Reviewer #3 did not have comments on this chapter.