Science Consistency Review for Empire Project Draft Environmental Impact Statement, Mt. Hough Ranger District, Plumas National Forest

On March 15th, 2005 the Forest Supervisor from the Plumas National Forest requested that the Pacific Southwest Research Station of the Forest Service assist the Plumas National Forest by leading a Science Consistency Review (SCR) of the Draft Environmental Impact Statement (DEIS) for the "Empire Project." The SCR process is briefly defined below and more thoroughly discussed in the Forest Service document FS-771, "Science Consistency Reviews, A Primer for Applications."

Concern over Federal land management decisions has grown in recent years. Public debates over activities on Federal land have been contentious, especially regarding management of national forests. Decisions on the management and use of national forest lands are based on many different considerations and values. Although Federal land managers can make choices concerning how to balance the various risks and tradeoffs involved, their decisions face questions from the public about whether the management direction and its associated effects, outcomes, and outputs are appropriate.

The responsible official can use a number of methods to evaluate the scientific information supporting those decisions. Examples include scientific peer review, science advisory boards, science consistency reviews, or expert opinion. The SCR is defined as *the process used to determine whether an analysis or decision document is consistent with the best available science*. That review is accomplished by judging whether scientific information of appropriate content, rigor, and applicability has been considered, evaluated, and synthesized in the documents that underlie and that implement land management decisions.

The request from the Plumas National Forest specifically called for the SCR team to evaluate whether the draft Environmental Impact Statement and attending documents have considered and correctly interpreted applicable and available scientific information. The request also sought an evaluation to assess whether the expected risks and consequences from the proposed action and alternative actions are consistent with what would be inferred from the scientific literature, and whether they are correctly interpreted and disclosed in the draft document.

The specific concerns identified by the Forest with regards to the Empire Project addressed the following four Issues/Questions:

a) Assessment of the response of the California spotted owls to proposed treatments;
b) Projected effectiveness of the proposed fuel treatments relative to meeting our on-the-ground objectives;
c) Have we displayed and supported with suitable evidence how proposed treatments are anticipated to move the Forest landscape toward fire resilient conditions; and
d) Are the described/anticipated cumulative effects supported adequately by scientific reason and evidence.

This review process is especially focused on four criteria with which to evaluate the specific questions raised by the Forest. These evaluation criteria are:

1. Is the relevant scientific information considered?
2. Is the scientific information reasonably interpreted and accurately presented?
3. Are the uncertainties associated with the relevant scientific information acknowledged and documented?
4. Are the relevant management consequences identified and documented, including associated risks and uncertainties?

These evaluation criteria are the central purpose of the SCR process. The evaluation team is not intended to judge the decision or otherwise make recommendations on the merits of one potential alternative decision vs. another. It is important that all parties understand the appropriate role of the SCR team.

Upon receiving this request Forest Supervisor, PSW assembled a six person SCR team that included a specific focus on expertise needed to address the above four questions. Both the Forest and PSW agreed that the questions dictated certain areas of expertise, specifically a) fire behavior, effects and fuels management, b) forest ecology and silviculture, and c) old forest dependent wildlife species. Two people were identified for each of the three areas of expertise needed, thus a six person team. Dr. Peter Stine was requested to be the team leader and manage the SCR process. The team members include:

- Dr. Dave Peterson, Research Forester, Pacific Northwest Research Station, Seattle, WA
- Dr. Kevin O'Hara, Professor, University of California at Berkeley, Berkeley, CA
- Dr. Don McKenzie, Research Ecologist, Pacific Northwest Research Station, Seattle, WA
- Dr. Eric Knapp, Research Ecologist, Pacific Southwest Research Station, Redding, CA
- Jeffrey Dunk, Research Wildlife Ecologist, Humboldt State University, Arcata, CA
- Brian Woodbridge, Wildlife Biologist, U.S. Fish and Wildlife Service, Yreka, CA

The team was given the task of reviewing the relevant (to their areas of experience and expertise) portions of the DEIS, Biological Assessment and Biological Evaluation, and other attending written documents. We then met as a group in Quincy, CA on June 1st and 2nd to see portions of the project area in the field and then discuss in more detail the project and the documents with the Interdisciplinary Team and managers from the Plumas National Forest.

Our report has two main components. We first present a summary of the collective views of the team; distilling all the relevant issues identified into one set of issues. Each issue is addressed by stating the issue and providing a brief description of the issue and some clarification on how it might be addressed/clarified, if necessary. Then each individual report from each team member is appended for the record.

First we would like to state that we believe that the Interdisciplinary Team (ID Team) has done a noteworthy job of presenting the scientific information that bears on the decision. Clearly the ID Team is knowledgeable and capable and they have done a very competent job of documenting their work and use of scientific literature and tools. They should be commended for their work. To a great extent, vegetation and fuel treatment planning amounts to an adaptive management experiment. The careful assembly of data and planning information here provide the basis for informed decision making on the Plumas National Forest. The Forests should approach these projects from the point of view that each iteration of treatments represents an opportunity to learn more and revise and refine subsequent treatments to more effectively address the array of land management objectives. With some adjustments that are recommended by the team the final decision documents should provide a good model for future project development in the area.

Summary of Issues, by Category:

General Issues

1) Ecological models are an important component of effects analysis and consideration of alternative land management options. Each model used, whether one that is in standard use or one that is experimental, carries with it a degree of uncertainty and a number of assumptions. It is therefore helpful to directly acknowledge the uncertainty and assumptions and be clear how this information is used appropriately to weigh in on decisions.

2) Adequate consideration/treatment of uncertainty is lacking from the EIS – this is a point that is required within the guidelines for science consistency reviews. Uncertainty is particularly important for vegetation and fuel treatments, because empirical data and replicated studies are relatively uncommon for evaluating the long-term effects of those treatments. While there may not be much that can be done to reduce the uncertainty, it should be acknowledged to a greater extent throughout the EIS. Uncertainty is not a flaw, per se, it is the general rule for a large portion of our understanding of natural processes. However, it should be bluntly acknowledged and reported in the decision document.

3) This purposed and need for this project is highly dependent on previous direction, notably from the HFQLG Act. Therefore the flexibility of the ID Team and management for applying different kinds of management prescriptions is limited from what otherwise could be exercised by the team. This is not necessarily good or bad, it is simply important to make this clear. There is a law that prescribes that these kinds of approaches be taken in this area. Clear representation of this fact will address some questions that could be raised (see more on this below under silvicultural options).

4) Explain the relationship of any monitoring requirements for this project with the overall monitoring strategy for the HFQLG pilot program. Ideally there should be a monitoring strategy for the entire program that efficiently utilizes information obtained from individual project monitoring.

5) Like most EISs, the document contains many comprehensive lists and discussions, that is, a high level of detail complexity. It would benefit from an effort to illustrate the connections between different biophysical components of the Empire area, that is, the dynamic complexity of the system.

6) The rationale for the use of Equivalent Roaded Acres should be more thoroughly discussed. We understand that is the methodology of choice in Region 5 and thus relied upon to analyze cumulative watershed effects. There are many other hydrologic modeling approaches available in the Forest Service and elsewhere that have been used to predict postfire water supply and sedimentation. Why is the ERA approach better than other models?

7) It appears that very few trees in the planning area have a diameter greater than 30 inches. Since most trees are therefore available for cutting in the thinning treatments, it is unclear exactly what size class trees are being selected for removal. According to the principles of fire resilient forests outlined by Agee and Skinner (2005), maintaining the largest trees should be emphasized if fire resiliency is a goal. We gathered from discussions during the review that retaining and encouraging the development of larger trees where they can be sustained is an objective of the treatments so there is some incongruity (i.e. between size classes targeted for removal and retention of larger size classes of trees) without further explanation. Some figures showing simulated stands before and after treatment would be useful to illustrate the intention of retaining and/or restoring the larger tree component while concurrently removing some of the mid-size trees.

Fire behavior, effects and fuels management

1) The landscape context of fuel treatments and other treatments that affect fuel loadings and distribution needs to be addressed more explicitly. A discussion of how spatial patterns of vegetation are affected, and how these might affect other resources of concern, would be a valuable addition. The anticipated adequacy of a fuels management strategy is inherently tied to spatial (and temporal) context of the treatments.

2) It might be useful to reiterate the fire management goals of the project prior to discussing the results of the fire modeling efforts, and to state how the results from fire models indicate how such goals will be achieved.

3) The use of "fuel models", though just about the only option for linking to fire behavior computations, collapses all natural variability into a few possibilities. Some discussion of this problem is needed so as not to portray a false sense of accuracy.

4) The EIS should have much better documentation of how fuel models were "assigned" for future scenarios. Right now, it seems totally subjective and thus unclear (i.e., one could choose based on the desired outcome).

5) Weather conditions seem to have been assumed the same for all stand structures, both for surface fire behavior and crown fire potential. Perhaps a better explanation is needed to clarify if this was indeed done, and if so why.

6) There seems to be some uncertainty about what the forest will look like in treated areas. What future vegetation structure was assumed in the fire modeling efforts? It might be useful to acknowledge the expected tradeoff between extent of stand thinning and subsequent understory development (i.e. Keyes and O'Hara 2002). Addressing the maintenance requirements and outlining a strategy for attaining durable fire-resilience will more thoroughly address concerns around uncertainty of future stand structure.

7) The key variables of interest (with respect to evaluation of successful treatments) in the fire modeling are torching index, crowning index, and resistance to control. Be clear about this and emphasize what your simulation effects tell you about these variables.

8) Be careful to document all the steps that are taken to ultimately create fire behavior simulations and thus projected effects. We believe your work is sound and scientifically defensible however some of the work could be made clearer to the reader. Do not assume that the reader will grasp all the ramifications of each step and assumptions that you have to make in order to make projections of fire effects under different forest conditions.

9) When providing some quantitative estimates of outcomes, as presented in some of the graphs, it is desirable to also include "error bars." We realize that some of these models used do not provide for estimating error however it is better to provide some indication of error than to create the impression that there is no uncertainty or variability. This is especially true for estimates as they proceed further into the future.

10) It still does not seem clear what was going to be left in areas within the group selection and individual tree selection sites. What sort of fuels conditions will there be within these treatment sites where silviculture and not fuels are the driving management concern?

11) We do not see it demonstrated (just stated) that alternatives A and C will reduce wildfire risk in PACs (table 2.19). This assertion should be supported with more concrete evidence.

12) Explain why the fire modeling relied upon a 90^{th} percentile weather condition and how this will be an adequate standard for a fire resilient forest.

13) Discuss more thoroughly the status of surface fuels after thinning and the consequences for post treatment conditions.

14) Effects on air quality are not presented in much detail. Smoke management plans are mentioned, but it appeared as if air-quality differences from the different alternatives

were not estimated. Some discussion of this is desirable, in the context of regional-scale air quality issues and concerns.

15) While the Sierran Mixed Conifer Forest and Red and White Fir Forest sections provide information on fire history, the section on montane chaparral is entirely about fire severity. It would be helpful if both fire history and historical and current fire severity patterns were consistently described for all three vegetation types.

16) Since the argument that opening the stand decreases fuel moisture and increases flammability has been used in the past to argue against thinning treatments, it might be useful to acknowledge this possibility, but to also state alternative views; e.g. in the words of Weatherspoon (1996), "the negative effects on microclimate of opening the stand are outweighed by the reduction in live and dead fuel loading and continuity." A similar quote is given in Agee and Skinner 2005. Acknowledge there are various points of view on this topic within the scientific community and it depends on a variety of factors.

Forest ecology and silviculture

1) Several proposed units for group selection exceed the 11.4 percent threshold for group selection harvest density (Table E3). The best group selection systems are those which are established with an eye towards location, access, and orientation of future group openings. This is largely an operational issue, but it might be useful to address it in the Empire EIS to demonstrate that managers are thinking ahead.

2) Individual tree selection (ITS) – This term has a particular meaning in the forestry profession. Single tree selection (synonymous with individual tree selection) is defined as an uneven-aged regeneration method where "individual trees of all size classes are removed more or less uniformly throughout the stand, to promote growth of remaining trees and to provide space for regeneration" (Helms, 1998). It would appear as though the ITS treatment described in the Empire EIS is more of a crown thinning than a regeneration treatment. The Herger-Feinstein QLG Act (HF-QLG) stipulates "individual tree selection" so the treatment described in the Empire EIS may meet this requirement in name only. Clarification is needed to differentiate between individual tree selection as used for the Empire Project and the accepted term used in the forestry profession.

3) The project could benefit from clearly articulated target stand objectives that can be met by the proposed treatments. These target stands should demonstrate how development over time either meets objectives or moves the stand toward meeting objectives. Diagrams of stand structures would be useful for this purpose.

4) It is not clear what the longer term, landscape objectives for the ITS silviculture are. The silvicultural strategy for all treatments should be grounded in some clearly stated forest management objectives. Lack of a well defined desired future condition makes it more difficult to defend a silvicultural strategy that removes larger trees. Presumably there is such a definition and if so it should be more thoroughly articulated in the

document. Post treatment fuel loading conditions are perhaps one component of desired conditions that needs better articulation.

5) Some DFPZs include stands heavily affected by past timber sales that removed the largest trees. The removal of larger trees has encouraged species compositions with a greater proportion of incense-cedar and white fir that existed in presettlement forests. Perhaps DFPZ prescriptions should consider augmenting species composition with a restoration planting of ponderosa pine, rust resistant sugar pine, or Douglas-fir. This could accomplish the dual long-term objectives of improving long-term fire resistance and restoring a composition closer to presettlement conditions.

6) Long-term stand development and plans for DFPZ maintenance are not developed. There should be some assurances in the Empire EIS that current DFPZ treatments are providing short-term values while also not creating long-term problems.

7) We realize that various sources of policy and guidance have dictated diameter size limits for harvesting trees and we understand the motivation for this. Later in our comments we will address the issue of restoring the large tree component of these forests. However there is little discussion of the scientific rationale or implications of such a policy. Thus if possible it seems prudent to at least be as transparent about this policy as possible, both in terms the habitat, fire and fuels management, and the economic/forest products implications of this potentially widespread practice.

8) Crown competition factor (CCF) was presented during the indoor presentations of the Empire DEIS. There may be some misinterpretations on how this measure is used. A CCF of 140, for example, does not indicate crown cover of 140%. Instead it indicates that the average diameter could support fully crowned trees that covered 140% of the stand. This indicates that given the average stand diameter, the stand is overstocked but it does not indicate crown layering or crown overlap. The original reference on CCF development is Krajicek et al. (1961).

9) Successful silviculture is dependent on understanding the origins of any stand being managed which requires understanding the stand's age structure. Otherwise we risk similar mistakes in stand interpretation like those that contributed to selective removal of the largest trees within stands in the past. This should be considered as part of relevant information for the decision.

10) The Empire EIS relies on canopy cover to represent stand density and potential fire behavior in silvicultural prescriptions. Canopy cover is difficult to measure and results are inconsistent between measurement techniques. Canopy cover may also be poorly correlated with changes in bulk density, stand structure, species composition, and site quality. Rather than using canopy cover guidelines across project areas or the entire QLG, flexible canopy cover guidelines should be considered that integrate changes over gradients in site quality, species composition, and stand age structure. It is key that assertions of canopy cover response post treatment be corroborated with reliable monitoring data. What are the plans to provide for this?

11) The silvicultural aspects of the Empire EIS appear to be prescribed through external direction rather than being the site-specific prescriptions characteristic of US Forest Service forest management since the Forest Management Act in 1976. Management activities are limited to a narrow range of treatments that are assumed to be applicable to a range of sites regardless of site quality, previous management, stand structure, species composition, or, to a lesser degree, landscape position. This may leave the project planning process vulnerable because treatments may be misapplied or inappropriate for the present stand conditions.

Old forest dependent wildlife species

1) Accuracy of the data used to characterize wildlife habitat should be discussed more carefully. The California Wildlife Habitat Relationships (CWHR) habitat types (dbh and canopy cover) were used to evaluate the potential effects of the various plan alternatives. We realize that these categories, while standard practice today, represent rough breakdown of appropriate size classes of habitat. Some important distinctions within some size classes of habitat are lost in the gross categorization of habitats. If this can be addressed it should, if not it should at least be acknowledged as an important uncertainty.

2) Site quality in this area appears to be relatively mediocre compared with some other locations in the northern Sierra. Whatever large tree structure may have once existed in this area is largely gone now. Presumably part of the long term objectives of silviculture in this area is to restore the large tree structure of habitat at an appropriate level (i.e. what the site can support without heroic management efforts). If this is true the project plan should more explicitly state this and be more conscious of what silvicultural practices and prescriptions are necessary to achieve this.

3) Evaluating Risk and Uncertainty with the Data that are Available. One possible way to evaluate risk associated with the uncertainty of habitat typing accuracy and potential impacts to California spotted owls (CSOs), northern goshawks, marten, and fisher is to evaluate only size class 5D or 5D and 4D or 5D and 5M as being suitable habitat. This could be done at the project scale and at a PAC by PAC level (for CSO and goshawk).

4) California Spotted Owl Population Trends and Habitat: The Science, Risk, and Uncertainty. The empirical data on CSO population trends comes from individual demographic studies (Steger et al. 2000, Blakesley et al. 2001, Seamans et al. 2001, and LaHaye et al. 2004) and a meta-analyses (Franklin et al. 2004). Four of the five studies have successfully made it through the scientific peer-review process and represent the most reliable information available at this time. Whether or not the Empire project's impacts would result in a trend toward listing or loss of viability of CSOs cannot be judged with the information presented in the DEIS. Such an evaluation would entail weighing the effects of all other current and proposed projects within the species' range (clearly beyond the scope of this work). Including some more discussion points, acknowledging the uncertainties in amount of habitat, and CSO population trends in the

final document should address the uncertainty in the decision documents better than currently reflected in the DEIS.

5) As with the CSO, however, we need to consider whether the analyses used in the DEIS adequately represents goshawk habitat. A recently published monograph on northern goshawks (McGrath et al. 2003) reported that goshawk nests occurred on the lower 1/3 of slopes and in drainage bottoms more than expected based on availability (and less than expected on the upper 1/3 of slopes and ridge tops). PNF personnel could modify their estimates of northern goshawk nesting habitat within the project area by using some other information, (including McGrath) to further distinguish suitable habitat from unsuitable.

6) As with the CSO and northern goshawk, the DEIS perhaps overestimates the amount of potential fisher and marten habitat by including all CWHR 4M and 5M stands. The appropriateness of all 4M and 5M stands should be discussed more.

7) The DEIS seems to lack a grand "synthesis" of the various project alternatives relative to the project's several stated goals and objectives. Perhaps a series of figures showing the relationship of each of the project's goals and the degree to which goals are met, including how risks increase or decrease by each alternative, would be a helpful addition.

8) The Wildlife Analysis Area appears to include 94,502 acres however the total of National Forest System land within this area is 76,121 acres. The condition and future management anticipated for these non-NFS lands is somewhat lost in the analysis. Casual read of the documents may suggest that the anticipated effects are covered for the entire 94,502 acres. Percentage figures in the document also suggest this. Be more clear about the distinctions.

9) The layout of groups relative to CSO and goshawk home ranges could use some further discussion. This point pertains to giving the decision maker important information relative to evaluation criterion #4, "are the relevant management consequences identified and documented, including associated risks and uncertainties?" Given there is some degree of discretion in where groups are placed it would be helpful to discuss the relevant consequences of different placement strategies on the likelihood of owls remaining in the territory. Granted there is no hard evidence for any thresholds in what they may tolerate in the way of changes in forest structure in the home range but if a manager has flexibility in the location and degree of treatment intensity he or she can potentially use that discretion to reduce risk.

Science Consistency Review

Draft EIS, Empire Vegetation Management Project
Mt. Hough Ranger District, Plumas National Forest

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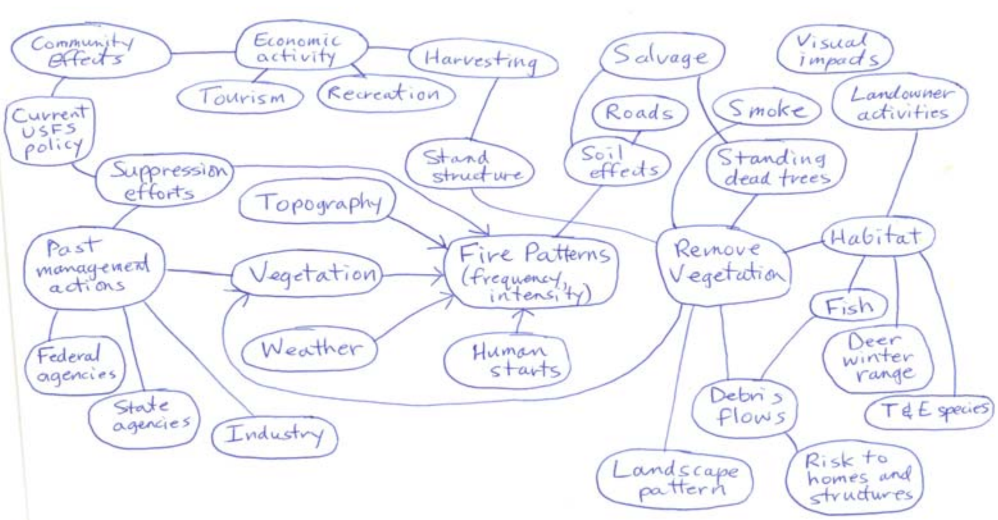
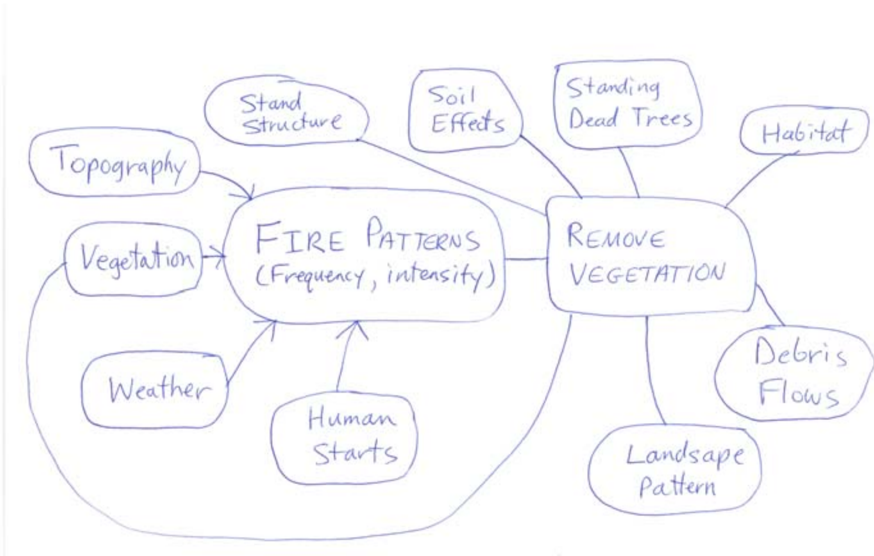
Overall the EIS effectively uses established scientific principles to examine alternatives for vegetation management – the Plumas ID team has done an excellent job! In fact, the use of scientific literature is exceptional compared to most such analyses, and recent data and syntheses are cited to support proposed management actions. While some fine tuning would help to improve the EIS, there are no serious errors or technical shortcomings.

There are two general points that could improve the communication of scientific information:

1. Like most EISs, the document contains many comprehensive lists and discussions, that is, a high level of detail complexity. It would benefit from an effort to illustrate the connections between different biophysical components of the Empire area, that is, the dynamic complexity of the system. This can be accomplished through the use of a concise system diagram. Here is an example of a very detailed, but ineffective, system diagram. It does not allow for straightforward communication and discussion.



Here is an example of a straightforward system diagram. It focuses on the most important components and interactions, and can be effectively communicated within an ID team or to the public.



2. Consideration of uncertainty is lacking almost entirely from the EIS – this is a point that is required within the guidelines for science consistency reviews. Uncertainty is particularly important for vegetation and fuel treatments, because empirical data and replicated studies are relatively uncommon for evaluating the long-term effects of those treatments. In addition, the modeling tools used to generate some of the outputs considered in the EIS have a high degree of uncertainty in their calculations and outputs. While there may not be much that can be done to reduce the uncertainty, it should be acknowledged to a greater extent throughout the EIS.

The following list addresses points mostly in the order they appear in the EIS:

1. Model output should be regarded as "fuzzy" numerical output with unknown but sometimes large variance around the calculations. This is normal for modeling efforts, and the output is probably better evaluated in a relative rather than an absolute sense.

2. Monitoring is mentioned in several sections, but needs to be articulated better with respect to specific monitoring schedules, techniques, locations, duration, and reporting.

3. As noted in the EIS, large trees provide a number of benefits including fire resistance. What is the tradeoff of retaining trees < 30 in dbh vs. 20 in dbh?

4. The 90th percentile weather cutoff is lower than I have seen for most analyses of "severe" fire weather conditions. Was there a specific reason it was chosen? How does it compare to 98th percentile weather?

5. Underburning and broadcast burning are important and commonly used tools for reducing surface fuels and ladder fuels. Unfortunately they are not uniformly reliable for effective fuel reduction due to weather and fuel moisture variability. So a prescribed burn might cover entire treatment unit, but may not reduce the fuels very much or might remove fuels in a patchy pattern. This variability could be noted as a potential impediment to achieving desired conditions in fuels.

6. The use of standard fuel models is a commonly used convention, but it does not quantify real fuels. Fuel models and the fire behavior prediction system used by the Forest Service model one-dimensional, homogeneous fuels, whereas real fuels are three-dimensional and heterogeneous. This is a major tradeoff in accuracy compared to real fuels and fire behavior.

7. Prescribed fire can in some cases increase surface fuels for 5-10 years due to mortality of small trees and lower branches.

8. The use of group selection is an interesting approach for the DFPZ. I do not have any concern about it ecologically, because it can represent small disturbances, although it may be effective in slowing high-energy fires only after several adjacent blocks are cut.

9. In these forest types, which contain fire regimes that vary between low severity to mixed severity, mean fire return interval may not be the best metric for evaluating long-term fire occurrence. Natural fire rotation is probably better.

10. The treatments include strong consideration for protecting spotted owls. Is there anything about the new habitat that will be created through vegetation treatments that might favor barred owls or corvids?

11. The use of Equivalent Roaded Acres requires considerably more justification. There are many other hydrologic modeling approaches available in the Forest Service and elsewhere that have been used to predict postfire water supply and sedimentation. Why is the ERA approach better than other models?

12. I think the proposed DFPZ looks fine as a potential deterrent to fire spread. However, the overall pattern is not discussed much and would benefit from some comment about why this particular pattern is used.

13. Some additional discussion on air quality is needed. What are the effects of the different treatments on air quality? What are the major air quality concerns in this area and how can they be mitigated or avoided? Which tools will be used to evaluate emissions and smoke transport?

The amount of information and disclosure of data, regulatory issues, and processes in the EIS are excellent. To a great extent, vegetation and fuel treatment planning amounts to an adaptive management experiment. The careful assembly of data and planning information here provide the basis for informed decision making on the Plumas National

Forest. With slightly better integration across resource areas (per the dynamic complexity issue above), the EIS will be a model for similar projects on the Plumas National Forest and beyond.

Empire Project

O'Hara comments on silviculture/management issues

The Empire Project develops three general prescriptions for the entire analysis area: 1) DFPZ; 2) ITS; and 3) group selection. My comments focus on these three treatments with some general comments on related issues.

Group Selection

Group selection with group sizes ranging from 0.5 to 2.0 acres is the primary regeneration method on the Empire project. It is intended to simulate a mosaic of even-aged groups of trees over a stand. Within a group, trees greater than a minimum diameter or any sugar pine tree would be uncut. The resulting structure would therefore be group openings with residual trees. The matrix area between groups would be cut with an ITS prescription. Over time, the intention is to periodically establish group openings until the entire stand has been regenerated. At this point the process would begin again. The length of time between entries or establishment of group openings is the cutting cycle. Group selection is justified for the QLG based largely on Weatherspoon's (1996) review that concluded the presettlement landscape structure was approximated by what would be produced with a group selection system.

Several proposed units for group selection exceed the 11.4 percent threshold for group selection harvest density (Table E3).

Page 36 of the Empire EIS indicates a 175-year regeneration cycle (rotation) and cutting cycle lengths averaging 20 years over the project area (I'm not sure why this information is not given until Alt. D). This is consistent with the QLG website describing regeneration silviculture (http://www/qlg.org/pub/agree/regensilv.htm; accessed June 6, 2005) that indicates a 150 year regeneration cycle on better sites and 200 years on poorer sites. Note the cutting cycle length is inversely related to the amount of volume removed for a given regeneration cycle (O'Hara and Valappil 1999). Reducing the length of the cutting cycle implies smaller but more frequent volume removals. Given the range in percent group selection harvest density of 4 – 23 % (Table E3) across all proposed alternatives (and 4-17 % in Alt. A), cutting cycle lengths would range from 7 to 40 years. This implies a range in frequency of entry into these stands and a range of volume removals (not including variations in volume productivity related to site quality or stand structure). The effect of group selection on any given stand in the project area is therefore highly variable. This is not necessarily a problem if it is intentional. In this case, it does not appear to be intentional and represents a potential vulnerability to the Empire EIS. How can such variability in ecosystem treatments and outputs be prescribed other than on a stand-by-stand basis? Since this was apparently not the case for this analysis, it appears arbitrary.

The regeneration cycle also determines how much area is affected on an annual basis. According to the QLG website (http://www/qlg.org/pub/agree/regensilv.htm; accessed June 6, 2005), the number of acres cut annually is "stipulated by the QLG Bill" at 0.57 %

of the pilot project area. Whereas the process outlined in the Empire EIS would average 0.57% annually because of the 175-year regeneration cycle, this amount would fluctuate from year-to-year because of the variation in selection harvest density from year to year. This is a potential area of vulnerability for the Empire EIS because it may not be consistent with the specifications of the QLG program.

With group selection systems, it is common to establish these systems in predominantly even-aged stands as part of a conversion process to uneven-aged or multiaged stands. This is a long-term process that requires nearly a full regeneration cycle to complete the conversion. The first set of groups are relatively easy to establish based on timber maturity, access, and location relative to other ecosystem elements (openings, younger patches, riparian areas, etc.). Subsequent openings are more difficult to establish because, as the conversion process proceeds, shape and location are increasingly predetermined by previous entries. The best group selection systems are those which are established with an eye towards location, access, and orientation of future group openings. This is largely an operation issue, but it might be useful to address it in the Empire EIS to demonstrate that managers are thinking ahead.

Planting of group selection units is mentioned on page 28 of the Empire EIS. This is important as research on group selection indicates that without some assistance, establishment of shade intolerant species is uncertain and planting can be highly successful.

Individual Tree Selection

Individual tree selection (ITS) – this term is used incorrectly in the Empire EIS. The Dictionary of Forestry (Helms 1998) defines single tree selection (synonymous with individual tree selection) as an uneven-aged regeneration method where "*individual trees of all size classes are removed more or less uniformly throughout the stand, to promote growth of remaining trees and to provide space for regeneration.*" As a regeneration method, ITS must have a regeneration objective. The glossary of the Empire EIS (page 209) defines individual tree selection as "*A harvest method resulting in the removal of individual trees from a forest stand or management area.*" It is unclear where this usual of this term originates. The QLG group included it in their October 1998 (http://www/qlg.org/pub/agree/regensilv.htm; accessed June 6, 2005) description of regeneration silviculture. There is no discussion of regeneration in ITS units in the Empire EIS. On page 65 the Empire EIS stated that "*The ITS treatment may be described as a crown (Peterson et al 2005) of "free" thinning (Graham and Jain 2005) where individual trees are selected for removal for forest health objectives while maintain forest composition and structure."* I am not sure either of these two publications are correctly cited. Peterson et al. do not describe ITS, and the "free selection" described by Graham and Jain is a regeneration method. The use of the term "individual tree selection" in the Empire EIS and subsequent descriptions of this treatment are therefore not consistent with the accepted use of the term "individual tree selection". Note that the "Vegetation Report" for the Empire EIS does cite the Helms definition correctly.

It would appear as though the ITS treatment described in the Empire EIS is more of a crown thinning than a regeneration treatment. The Herger-Feinstein QLG Act (HF-QLG) stipulates "individual tree selection" so the treatment described in the Empire EIS may meet this requirement in name only. This is a potential vulnerability to the Empire EIS: an accepted term in the forestry profession is not used properly and the treatment does not conform to the legislative directive.

A related vulnerability in terms of using ITS as a regeneration method is that ITS may make stands less resistant to fire by encouraging the development of a vertically stratified stand. This would seem to create part of the desired future condition (Empire EIS page 8) of "all-age, multistory" forests but not the "fire resilient forests" that are also part of this directive. ITS will likely favor the regeneration of tolerant conifers such as white fir and incense-cedar, and form fuel ladders.

If redefined as a crown thinning treatment, the effect of the treatment may be more effective in reducing canopy cover, crown bulk density, and vertical continuity of fuels. If ITS is maintained as a key Empire Project treatment, then it might be useful to consider other ITS approaches that favor shade intolerant species such as the work I've done with ponderosa pine (O'Hara et al. 2003). If it remains as a regeneration method, then some thought should be put into why a regeneration method is being used in the matrix area of a group selection stand. This is another potential vulnerability of the Empire EIS.

The HF-QLG Pilot Project is to "demonstrate the effectiveness of fuelbreaks, group selection, individual tree selection…". A key question is whether the Empire project should demonstrate the ineffectiveness of a highly questionable treatment, or redesign the treatment to demonstrate its effectiveness.

Much of the ambiguity with ITS in the Empire EIS may originate with how this treatment was defined in the initial QLG process and subsequently interpretations. Evolving interpretations are essentially a moving target that could be avoided with consistent terminology use and clear definitions at the onset of any legislation. This ambiguity is a source of vulnerability for the Empire EIS.

As described in the Empire EIS, the ITS is used only in the matrix area between group selection harvest openings. There is no discussion of a slash treatment plan for this treatment other than the possibility of "ITS with Biomass" described on page 66. In any form, these treatments will generate considerable slash and this may further reduce the fire resistance of these matrix areas beyond the possible encouragement of fuel ladders and shade tolerant species.

There is very little information provided on how trees in ITS units will be selected for harvest/retention. Page 65 if the Empire EIS describes both crown and free thinning treatments that favor fire resistant conifers. But it is also stated that "individual trees of all sizes are removed more or less uniformly" implying a procedure similar to a BDq approach (O'Hara and Gersonde 2004). As these two methods might be read as conflicting with each other, it might be useful to clarify these cutting methods. This is related to previously described problems with terminology. In the paper cited above, we discuss the common procedures for regulating stocking in uneven-aged stands.

DFPZ

Some DFPZs include stands heavily affected by past high grading. This high grading has encouraged species compositions with a greater proportion of incense-cedar and white fir that existed in presettlement forests. Perhaps DFPZ prescriptions should consider augmenting species composition with a restoration planting of ponderosa pine, rust resistant sugar pine, or Douglas-fir. This could accomplish the dual long-term objectives of improving long-term fire resistance and restoring a composition closer to presettlement conditions.

Long-term stand development and plans for DFPZ maintenance are not developed. This is the result of a scarcity of knowledge in this area, but there should be some assurances in the Empire EIS that current DFPZ treatments are providing short-term values while also creating long-term problems.

General Comments

I am not familiar with any discussion of the long-term implications of leaving all trees above a given diameter at breast height (either 20 or 30 inches). Perhaps this is in the Sierra Nevada Record of Decision. What are the implications of this potentially widespread practice for future stand productivity, fire resistance, and economic return to local sawmills and communities?

Crown competition factor (CCF) was presented during the indoor presentations to the Empire EIS. I think there were some misinterpretations on how this measure is used. A CCF of 140, for example, does not indicate crown cover of 140%. Instead it indicates that the average diameter could support fully crowned trees that covered 140% of the stand. This indicates that given the average stand diameter, the stand is overstocked but it does not indicate crown layering or crown overlap. The original reference on CCF development is Krajicek et al. (1961).

At the third stop on the field trip, there was some discussion about understanding the age structure of forest stands to successfully manage them. I am a strong proponent that successful silviculture is dependent on understanding the origins of any stand being managed which requires understanding the stand's age structure. Otherwise we risk similar mistakes in stand interpretation like those that contributed to high grading stands in the past.

I believe I have been incorrectly cited on page 58. The cited paper actually notes that diameter distributions in multiaged stands are highly variable. When diameter distributions are developed for multiple stands or forests, then the distribution takes on the negative exponential form shown in figure 3.1. My professional opinion is that the variation that is masked in forest-level diameter distributions represents an important stand structural feature that should be integrated into the stand-specific prescription. This point is more clearly articulated in O'Hara (1998).

The Empire EIS is relies on canopy cover to represent stand density and potential fire behavior in silvicultural prescriptions. The first stop on our field trip included a pine stand after thinning for a DFPZ that appeared to have a higher canopy cover. However, it had not been measured in any post-treatment monitoring. Canopy cover is difficult to measure and results are inconsistent between measurement techniques. Canopy cover may also be poorly correlated with changes in bulk density, stand structure, species composition, and site quality. Rather than using canopy cover guidelines across project areas or the entire QLG, flexible canopy cover guidelines are needed that integrate changes over gradients in site quality, species composition, and stand age structure.

Apparently, monitoring of canopy cover is being done on some sites by classes at Feather River College. Post-treatment checking and long-term monitoring is necessary and there should be a systematic plan for both. Given the difficulties and inconsistencies with measuring canopy cover, there is a need for more measurement and oversight of these monitoring efforts.

Summary Comments on Silviculture/Forest Management Issues
The silvicultural aspects of the Empire EIS appear to be prescribed from above rather than being the site-specific prescriptions characteristic of US Forest Service forest management since the Forest Management Act in 1976. Management activities are limited to a narrow range of treatments that are assumed to be applicable to a range of sites regardless of site quality, previous management, stand structure, species composition, or, to a lesser degree, landscape position. This may leave the project planning process vulnerable because treatments may be misapplied or inappropriate for the present stand conditions.

I think the project could benefit from clearly articulated target stand objectives that can be met by the proposed treatments. These target stands should demonstrate how development over time either meets objectives or moves the stand toward meeting objectives. Diagrams of stand structures would be useful for this purpose.

Literature not cited in EIS

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Biographical Info

Kevin L. O'Hara is Professor of Silviculture at the University of California - Berkeley. He is a Registered Professional Forester in California (RPF license # 2694) and a Certified Forester by the Society of American Foresters. His education includes a B.S. in Forest Management from Humboldt State University, an MS is Silviculture from Duke University and a Ph.D. in Silviculture and Forest Management from the University of Washington. O'Hara's research work focuses on forest stand dynamics and applications to stand and forest management. He is particularly active in the research of multiaged forest stands. He has authored over 70 scientific publications.

Science Consistency Review

Draft EIS, Empire Vegetation Management Project
Mt. Hough Ranger District, Plumas National Forest

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As requested, I have reviewed the Draft EIS, dated May 2005. Also as requested, I focused on the Fire, Fuels, and Air Quality Report, but have also looked at other topics, particularly the sections on forest vegetation and wildlife and fisheries. In the company of other reviewers, I visited the site for two days, was given a tour of field sites, and heard presentations that provided context and more detail to what is presented in the EIS.

Overall, I was impressed with the documentation, the proposed use of modeling and other tools, and the associated references to scientific literature. This EIS establishes a template for others to follow, as the Data Quality Act and other developments require management actions to be based on scientific understanding. What follows are comments that purport to address both the scientific quality of proposed and completed analyses and to a lesser extent, areas that might be vulnerable to hostile criticism.

Fire, fuels, and air quality

1. Starting broadly – the landscape context of fuel treatments and other treatments that affect fuel loadings and distribution needs to be addressed more explicitly. Total acreage in different categories is a first step, but a discussion of how spatial patterns of vegetation are affected, and how these might affect other resources of concern, would be a valuable addition.
2. The use of "fuel models" collapses all natural variability into a few possibilities. Granted, there are few reasonable alternatives for linking fuels to fire behavior computations, because fuel models "adjust" and homogenize real fuels on the landscape so that fire-behavior models can produce believable results. This homogenizes the landscape in two steps – one within stands to produce realistic fire behavior, and one across the landscape, e.g., every acre depicted as fuel model 10 (or any other) has the same fuel profile. What this does is give a false sense of accuracy to model results, and perhaps more important, a false sense of the precision with which alternative treatments can be compared. What may appear to be significant differences without any associated "error bars" might collapse if the true uncertainties were known. In the absence of a robust method to analyze those uncertainties, more discussion, even if informal, is warranted to mitigate that false sense of precision.
3. Page 16 has a discussion of future scenarios, where simulation model projections, up to 50 years, were assigned to fuel models so that future surface and crown fire potential could be estimated. Because of the limitations and homogeneities associated with the fuel models (see

#2), more detail is needed on the rationale for assigning these fuel models. It would be easy for a critic to argue that fuel models were assigned to ensure the fire-behavior output that the modeler desired, so as to either inflate or downplay the advantages or disadvantages of the different alternatives. Some discussion of how sensitive the output would be to changes in those rationales would give a more realistic picture of the results of the different alternatives, with respect to confidence levels.

4. Weather conditions (microclimate) seem to have been assumed the same for all stand structures, both for surface fire behavior and crown fire potential. The Raymond (2004) thesis was cited, but her key finding – that thinning treatments may have increased fire severity, because of increased fuel loads, altered microclimate, or both, was not mentioned. I may have missed the explanation of this, if so, then perhaps more clarification is needed.
5. In general: Some "error bars" are needed around all the outcomes. Better even to guess than to create the impression that there is no uncertainty or variability.
6. Effects on air quality are not presented in much detail. Smoke management plans are mentioned, but I saw no attempt to estimate air-quality differences from the different alternatives. Some discussion of this is desirable, in the context of regional-scale air quality issues and concerns. In the Northwest, for example, air-quality issues are one of the most frequent causes of cancellation of planned prescribed burns.
7. After visiting the field sites, and asking several times about fuels management in the group selection (GS) and individual tree selection (ITS) areas, I still wasn't clear what was going to be left on site. Maybe it was just me, but I sensed that this had not been fully worked out. That uncertainty gives critics the opportunity to make assumptions that fit their expectations and concerns. Across the West, in my estimation, surface fuels are the key to understanding fire-hazard potential. The tradeoffs between the presumed economic benefits of cutting trees in the GS and ITS units and the potential *increases* in fire severity (partially canceling, over the total acreage, the benefits of the fuel treatment units) need to be explicitly stated. I sensed a reluctance to discuss this.

Other issues

1. The whole idea of cutting (GS and ITS) in spotted owl habitat seems counter-intuitive to me. Although this is apparently not solely a science issue, I think the order not to consider habitat core areas when selecting group-selection sites will be a lightning rod.
2. But on the science side, I don't see it demonstrated (just stated) that alternatives A and C will reduce wildfire risk in PACs (table 2.19). As I mentioned above, increasing surface fuel loadings, especially fine fuels, will do more than anything else to increase fire hazard. In my opinion, further analysis in an explicitly spatial context would be required to support this claim. Given that there are not sufficient resources to carry out such an analysis, I believe the claim should be withdrawn.

United States Department of Agriculture | Forest Service | Pacific Southwest Research Station | 3644 Avtech Parkway
Redding, CA 96002

Date: June 22, 2005

Dr. Peter Stine
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RE: Empire Vegetation Management Project
Science Consistency Review

Dear Peter;

Though I was unfortunately not able to make the field trip due to conflicts with ongoing research prescribed burns on the Plumas National Forest, I read the draft EIS and can offer the following comments/ suggestions:

DRAFT EIS

1. Page 80 – Fire History. While the Sierran Mixed Conifer Forest and Red and White Fir Forest sections provide information on fire history, the section on montane chaparral is entirely about fire severity. It would be helpful if both fire history and historical and current fire severity patterns was consistently described for all three vegetation types. If fire personnel observations are provided about fire severity/ intensity for montane chaparral, provide the same for the forested vegetation types as well so the information does not seem so out of place. Nagel and Taylor (In Press) determined that the montane chaparral in their study area on the west shore of Lake Tahoe burned approximately every 28 years, as opposed to 14 years in the surrounding forest. This indicates that fire spread under historical conditions may have been constrained by the shrub fuels. Instead of stating that differences exist in fuel moisture etc. between shrub fields and the surrounding forest, it would be valuable to indicate what these differences are. For example, litter under shrubs is generally less abundant, more compact etc. Since trees sampled by Nagel and Taylor (In Press) were collected from fire scars on residual conifer trees located within the shrub matrix, it is possible that there was more conifer litter associated with the shrub fields in their study than would be present in a pure stand of shrubs. This means that the fire return interval in shrub fields may be even longer than the 28 years reported by Nagel and Taylor (In Press). It is also important to distinguish between fire spread and fire hazard. While rate of fire spread may typically be less in montane chaparral than the surrounding forest (in the absence of wind), fire hazard may be greater because when it does burn, it is more likely to burn at mixed/high severity (Wilken 1967, Skinner and Chang 1996). For the fir dominated forest vegetation type, another fire history reference would be Pitcher (1987), who reported a fire return interval of 65 years.

2. It might be useful to reiterate the fire management goals of the project prior to discussing the results of the fire modeling efforts, and to state how the results from fire models indicate how such goals will be achieved. On page iii of the summary it is stated that a purpose is "…to implement fuel reduction in the WUI and… to reduce the potential size and intensity of wildfires and provide fire suppression personnel safe locations for taking action against wildfires", and to achieve "an all-aged, multistory, fire-resilient forest". While the torching and crowning indices are helpful for predicting how treatments might influence fire intensity and stand structure when impacted by a wildfire, these statistics say little about the influence of treatments on wildfire size. This might require more of a landscape modeling approach looking at the location of treated areas relative to each other, such as that used with the Fireshed assessment tool (Bahro et al.). In addition, torching and crowning indices do not provide all the information necessary to predict whether treatments would lead to a fire-resilient forest. Enough heat can be generated to cause stand replacement with a surface fire alone, especially with relatively small-medium sized trees. Crown fire is not the only concern.
3. Attempting to model the torching/ crowning indices expected with different treatments into the future is certainly commendable. However, there is a great deal of uncertainty about what the forest will look like in treated areas. What future vegetation structure was assumed in the fire modeling efforts? Were vegetation growth models used to predict tree densities in the different size classes? What about understory vegetation? It might be useful to acknowledge the expected tradeoff between extent of stand thinning and subsequent understory development (i.e. Keyes and O'Hara 2002). Stands thinned the most now may produce more ladder fuels and more rapidly develop higher fire hazard conditions later, unless maintained by thinning, mastication, or prescribed fire. Will there be funds/ incentive to do such work in the future, especially if most of the merchantable material is removed now? Addressing the maintenance requirements and outlining a strategy for attaining durable fire-resilience might help to mitigate such concerns.
4. Fire models provide only a rough approximation of fire hazard parameters due to the many assumptions that such models are based on. The use of fuel models to characterize fuel loading conditions for the fire models, while a valid approach when actual numbers are not known, increases the amount of uncertainty. When error is considered, many of the modeled differences may not be sufficient to adequately contrast treatment alternatives. A discussion of potential error associated with the use of fuel models, and fire models in general, should be included.
5. Page 82. In addition to the lack of precision when using rough fuel models to characterize a variable landscape, additional error is associated with decisions about appropriate fuel model to use for the different treatment x time comparisons. For example, I question whether fuel model 9 is the best representation of post-underburn conditions. A good underburn would be expected to reduce fine fuels to the point that fire spread and chance of crowning/torching become very unlikely (i.e. examples in Agee and Skinner 2005). This condition should persist for at least a few years post-burn. Using fuel model 9 will likely over-estimate the crowning/ torching indices in underburned areas. Van Wagtendonk (1996) approximated post-prescribed burn conditions in evaluating fuel treatments with FARSITE, by reducing initial (control) fuel loads by 50%. Perry et al. (2004) used fuel model 8 for post-underburn conditions, as the fine fuels in this model are half those of model 10. Either of these two approaches may be more

representative of post-burn conditions, and more in-line with the scientific literature. However, both will still likely over estimate crowning/ torching indices, because proportional fuel reduction is assumed over the landscape and fuel discontinuities created by fire are not considered. Other fuel model assumptions (i.e. fuel model 10 after fifty years in group selection openings) are also not well supported… it is simply hard to say with much confidence what will be on a piece of land in the future. When this much uncertainty exists about what rough fuel model to use, it might be worthwhile to conduct a sensitivity analysis, using a range of possible fuel models and custom adjustments of fuel models to evaluate how much the results reflect the choice of the fuel model, and how much the results reflect the change in tree density/ forest structure. Because of the uncertainty, reporting a range of potential outcomes would be preferable to a single value.

6. Outputs only show that fire hazard is projected to decrease in treated areas. Since wildfire is likely to be fought at the landscape scale, how much fire hazard reduction does treating 10% of landscape really buy? Have you considered any landscape-level analyses, such as those conducted using the Fireshed assessment tool to evaluate the optimal placement of fuel treatments and proportion of landscape treated?
7. Page 83. Numbers in columns of Table 3.6 do not add up to "total" in bottom row.
8. Page 85. Small trees within the group selection units may not be especially vulnerable to scorch-induced mortality if fuels are adequately treated prior to replanting/ natural regeneration. Weatherspoon and Skinner (1995) found that plantations with site preparation that included broadcast burning suffered little or no damage when impacted by a wildfire. Small trees do not produce much fuel and plantations treated in this way may actually slow the spread and reduce the intensity of wildfire (Weatherspoon and Skinner 1995). However, fire damage within plantations was positively associated with level of damage on adjacent stands. Thus young trees in group selection units may be more vulnerable to fire damage if fuels outside of the group are not treated.
9. Why is prescribed fire proposed only for areas on steep slopes with low tree densities? Fire on such slopes is more likely to kill residual trees than on less steep slopes. What is the desired future condition in these areas?
10. Page 87, first bullet. Burning montane chaparral will likely perpetuate, not modify, the montane chaparral community type (Wilken 1967, Skinner and Chang 1996, Nagel and Taylor, In Press).
11. Page 88. What is the "threshold" of 25mph torching index? Why was this number chosen? What is "initial condition"? Explain.

DRAFT FUELS, FIRE, AND AIR QUALITY REPORT

12. Page 9. Given the definition of Fire Regime Condition Classes, I don't understand how shrub fields can be classified as FRCC 2 or 3. For example, in the Table F-3 of the Fire and Fuels report, 112 of the 316 acres in fuel treatment unit #1 are in fuel model 5 (shrubs). However, only 18 acres in this fuel treatment unit are considered FRCC 1 (Table F-5), therefore you must be putting the remaining 94 acres in either FRCC2 or FRCC3. What distinguishes a shrub field in FRCC 1 condition vs. FRCC 2 or FRCC 3? [Note: FRCC numbers in table F-5 of the Fire and Fuels report are in conflict with the

numbers in Appendix B of the EIS, which look more reasonable.]

13. Page 13, 3rd paragraph "A study by Perry et al. (2004)…" It is difficult to understand the point as written. In the 4th paragraph – "mean tree diameter in treated areas" is not necessarily comparable to "average diameter of the largest cohort" from FIA data, unless the plan is to leave only this largest cohort behind following treatment.
14. Page 16. What are fuel models 10a, 10z etc.? Either provide a citation describing fuel loading values and adjustments made from the standard fuel models, or provide in a table.
15. Page 16 last paragraph. As indicated in the comment above, it would be more accurate to present a number of different possible fuel models for each treatment by year combination. It seems doubtful that a 50 year old group selection unit starting from bare ground will accumulate enough fuel to be considered fuel model 10.
16. More information on the modeling effort would be useful. Are you modeling a head fire or backing fire? Is this the type of fire most likely to impact a given area? Is modeling done using actual slope of the treatment unit or the average for the planning area?
17. Values in Table F-7 are not all consistent with narrative.
18. Page 22, Table F-10; How do you get individual torching/ crowning indices for each tree size category? Do you assume all trees are that size? How do you then predict torching/ crowning in the more realistic case of multiple different tree size classes growing together? Assuming a single size category of trees will likely overestimate crown bulk density, and therefore the risk of crown fire, if the stand is actually multi-storied (Perry et al. 2004).
19. Table F-9 in the Fire and Fuels Report shows the same number of recommended gallons per acre of retardant for brush, timber or light logging slash… and these numbers also don't generally differ before and after treatment (Table F-15, F-18 etc.)… isn't this in conflict with the assertion that different forest structures within the same fuel type vary in effectiveness of retardant application?
20. Page 42. Why is it difficult to modify crown and ladder fuels of trees larger than 8-10 inches in diameter using prescribed fire? While you may not kill too many trees of this size with prescribed fire, stand thinning is only one component to creating a fire resilient forest. Another important principle involving crown modification is increasing the height to live crown (Agee and Skinner 2005), for which prescribed fire should be quite effective on trees of this size.
21. Page 43. Last paragraph. While shrub stems may be killed, burns in this vegetation type are likely to be patchy (Nagel and Taylor, In Press). In addition, fire causes seeds of many shrub species to germinate (Kauffman and Martin,1991). Stems of young shrubs are not only closer to the ground and therefore available for browse, but also likely to be more nutritious (Huffman and Moore, 2004; Scotter, 1980).
22. Page 70. Appendix 1, the description of methods used to create FRCC maps is indecipherable. Is it really necessary to include such detail?

OTHER COMMENTS

23. Alternative E and F differ from the other action alternatives by having a 20 inch dbh limit, rather than 30 inch. However, the cutoff between tree categories 4 and 5/6 is 24 inches. It is therefore difficult to visualize what the forest will look like from the

vegetation analyses. It would help if the tree categories used the same dbh cutoffs as the alternatives being compared.

24. It appears that very few trees in the planning area have a diameter greater than 30 inches. Since most trees are therefore available for cutting in the thinning treatments, it is left to the reader's imagination exactly what size class trees are being selected for removal. According to the principles of fire resilient forests outlined by Agee and Skinner (2005), maintaining the largest trees should be emphasized if fire resiliency is a goal. Some figures showing simulated stands before and after treatment would be useful.
25. Since the argument that opening the stand decreases fuel moisture and increases flammability has been used in the past to argue against thinning treatments, it might be useful to acknowledge this possibility, but to also state that, in the words of Weatherspoon (1996), "the negative effects on microclimate of opening the stand are outweighed by the reduction in live and dead fuel loading and continuity." A similar quote is given in Agee and Skinner 2005.
26. The Knapp et al. 2005 paper listed under references in the EIS has now been published. The citation is: Knapp, E.E., J.E. Keeley, E.A. Ballenger, and T.J. Brennan. 2005. Fuel reduction and coarse woody debris dynamics with early season and late season prescribed fire in a Sierra Nevada mixed conifer forest. Forest Ecology and Management 208:383-397.
27. In the summary section (Page iii) it is stated that one purpose is to "**test** the effectiveness of an uneven-aged silvicultural system in achieving an all-aged, multistory, fire resilient forest…". However, no monitoring plan is provided, or methods proposed, to actually conduct such a test.
28. Yoder et al (2004) recently published a paper about liability concerns associated with prescribed fire that might be worth citing.

Thanks for inviting me to be part of the Science Consistency Review team for the Empire Vegetation Management Project. It is apparent that a great deal of effort has already gone into producing the EIS and accompanying reports. I hope you will find my comments useful for helping to improve the scientific rigor of the analyses and addressing gaps that could cause confusion on the part of interested parties reading the EIS. Please feel free to contact me if you have any questions about these or other issues.

Sincerely,

Eric E. Knapp

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Science Consistency Review Comments – from Jeff Dunk

Within the DEIS for the Empire Vegetation Management Project all wildlife-related analyses were conducted in an identical fashion, so the general comments I have relate to all species covered. The more specific comments largely relate to the California spotted owl, because so much more is known about this species within the Sierra Nevada than for the other focal (old-forest related) species.

Accuracy of the Data

California Wildlife Habitat Relationships (CWHR) habitat types (dbh and canopy cover) were used to evaluate the potential effects of the various plan alternatives. The GIS layers used by PNF personnel were remotely-sensed (based on aerial photographs I believe). Stand exam data that were presented to the SCR team and used by PNF personnel showed that quadratic mean diameters in CWHR types 4D, 4M, 5D, and 5M were either at the very low end of the diameter spectrum (closer to the 11" dbh for size class 4 stands) or outside of the diameter spectrum for size class 5 stands. This may be due to differences in how CWHR defines these diameter classes relative to how PNF personnel evaluated their stand exam data (e.g., I believe CWHR has a minimum tree size that they use to estimate mean dbh – 5 or 6 inches – whereas PNF personnel apparently used all trees). Re-evaluating the stand exam data by truncating the smallest size classes, consistent with CWHR methodology, would help to rectify this potential problem, and/or to clarify the degree to which remotely-sensed CWHR typing agrees with on-the-ground forest data. It is likely that this re-evaluation would be very simple to conduct and would result in the two sources of information being more similar. That said, if on-the-ground data continue to be at the lower end of the CWHR classification, then estimates of total amount of habitat for the old-forest-related wildlife species will be less than depicted in the DEIS. Furthermore, the majority of the habitat-association research conducted on California spotted owls, northern goshawks, fisher, and marten was based on on-the-ground field sampling of forest structural and compositional variables. Subsequent to these field samples, species' habitat associations were categorized into CWHR types. There exists error in any sampling, but much less so in the on-the-ground measurements relative to remotely-sensed classification of forests. A re-analysis of the stand exam QMD's (as per the above recommendation) would inform us as to whether the current estimates of the acreages presented in table 3.11 (and elsewhere) of the DEIS are likely to be accurate or overestimates of what is on-the-ground (it seems very unlikely that they would be underestimates). The disparity between the current CWHR classification and the stand exam plots represents uncertainty in the "truth" of the amount of each CWHR habitat type in the project area. For the old-forest related wildlife, overestimates of CWHR habitat type 5D (likely the most important habitat type) and the "higher end" (i.e., closer to 24" dbh trees) of 4D would tend to result in underestimating potential impacts to these species.

Evaluating Risk and Uncertainty with the Data that are Available

One possible way to evaluate risk associated with the uncertainty of habitat typing accuracy and potential impacts to California spotted owls (CSOs), northern goshawks, marten, and fisher is to evaluate only size class 5D or 5D and 4D or 5D and 5M as being suitable habitat. This could be done at the project scale and at a PAC by PAC level (for CSO and goshawk), in a fairly short period of time. You could also use all of the known CSO (or goshawk) nest locations on the PNF to get an idea of the abiotic niche characteristics that exist in those areas (e.g., elevation, percent slope, slope position [high, mid, lower], distance to nearest stream). If patterns exist (i.e., they aren't found everywhere) you could screen all areas not meeting those criteria within the Empire Project boundaries. You would have to make a few determinations about how you would use the data (i.e., would you use the mean, meadian, 75^{th} quartile?).

California Spotted Owl Population Trends and Habitat: The Science, Risk, and Uncertainty

The empirical data on CSO population trends comes from individual demographic studies (Steger et al. 2000, Blakesley et al. 2001, Seamans et al. 2001, and LaHaye et al. 2004) and a meta-analyses (Franklin et al. 2004). Of these, Steger et al. (2000) is the only one that has not been published in the peer-reviewed literature. This is not meant to diminish the value or validity of that study, but to point out that the other four have successfully made it through the scientific peer-review process. The demographic study area closest to the Empire Project is on the Lassen National Forest (Blakesley et al. 2001, Blakesley 2003). The findings of individual peer-reviewed demographic studies was a decline in California spotted owl populations over time (Blakesley et al. 2001, Seamans et al. 2001, LaHaye et al. 2004). The meta-analysis of Franklin et al. (2004) concluded that strong inferences about a population decline could not be made because estimates of lambda did not differ significantly from 1.0 (a stationary population), though point estimates in four of the five study areas were <1.0. Thus, the empirical information on CSO population trends is uncertain. However, the uncertainty lies in whether populations are in fact declining or remaining stable, not whether they are increasing. One might also reasonably ask whether the study areas chosen for conducting demographic studies are representative of CSO populations in general or are at the higher end of quality (although possible, it seems unlikely that these study areas are at the lower end of the habitat quality spectrum for the species). If the reality of CSO populations is that they are declining, activities that further remove their habitat are likely to further contribute to their decline. If CSO populations are in reality stationary, activities that remove their habitat may or may not push the population from stationary to declining, depending on the magnitude of habitat loss and how close to declining the population currently is.

Bond et al. (2004) evaluated CSO nesting habitat selection in Eldorado and Placer Counties, California and found that the number of large trees (≥76.1 cm) and canopy cover were the best predictors of owl nesting habitat (more large trees and greater canopy cover resulted in higher probabilities of the stand being used for nesting). The scale of Bond et al.'s (2004) was 0.2 ha. Blakesley (2003) evaluated the relationship between forest characteristics at two spatial scales (203 ha and 814 ha) and CSO site occupancy, apparent survival, and nest success. She found:

> "*The best model in analyses of site occupancy, apparent survival and nest success included positive relationships between the response variables and SELCCG, HABSCORE, or REMNANT. This indicates that although*

owls were found nesting and roosting in a variety of forest stand types, site occupancy, apparent survival, and nesting success all increased with increased amounts of forest cover types known to be selected by the owl at the landscape scale. Reproductive output decreased with increasing amount of non-habitat within the nest area." Italics added by JRD. (note, nest area = 203 ha scale).

Blakesley (2003) defined SELCCG as areas containing trees >61 cm (24 inch) dbh with >70% canopy cover or multi-layered stands with >70% canopy cover. REMNANT was defined as the presence of remnant trees, and HABSCORE was the ratio of medium and large cover types weighted by proportion used to proportion available (see Table 1.1, page 21, in Blakesley 2003).

Even with the most studied organisms, uncertainty will remain. That said, the best scientific evidence suggests that CSO populations are either declining gradually or stable, but perhaps leaning toward decline. Additionally, both small, plot-scale, and larger scale (hundreds of ha) evaluations show that CSOs use a variety of habitat types but preferentially select nesting and roosting areas in forests with large trees and greater canopy cover (in CWHR parlance 5D, and perhaps the upper end of 4D and 5M). Blakesley (2003) provides the link between "simple" evaluations of habitat selection based on used – unused and included an evaluation of the fitness consequences within used areas. Her findings that occupancy, apparent survival, and nesting success all increased with increasing amounts of old-forest characteristics provide potential insights into the impacts that land-management actions might have on CSOs.

Many researchers have evaluated the spatial scale at which spotted owls respond to habitat (Blakesley 2003) for the CSO, and Hunter et al. (1995), Bingham and Noon (1997), Meyer et al. (1998), Franklin et al. (2000), and Zabel et al. (2003) for the northern spotted owl (NSO). Each of those evaluations found that areas within ~800 m (~200 ha) of nests were influential in determining occupancy, and/or fitness (Franklin et al. 2000 used a smaller radius of ~ 710 m). PNF has 300 acre (118 ha) PACs for CSOs in which (if my understanding is correct) no management activities will occur. The published literature on CSO and NSO suggest that effects outside of the PAC (on another ~200 acres or 82 ha) may influence a site's "quality" for spotted owls. Several of the Empire Project's alternatives include management activities within Home Range Core Areas (HRCA; 700 acre areas delineated by PNF wildlife biologists, but without apparent management constraints). Based on the scientific literature, one could argue that management actions that reduce high-quality spotted owl habitat (see above) within a 500 acre (200 ha) area around known CSO nests could present more risk to CSOs than activities occurring outside of this area.

On page 115 of the DEIS, it says

> "*Because the spotted owl population is currently within the 95% confidence limits of a stable population (Franklin et al 2003 in SNFPA SFEIS 2004), the SNFPA FSEIS and BA/BE concluded that these cumulative habitat changes (within the range of the California spotted owl within both the Sierra Nevada and the HFQLG planning area) would not result in a trend toward listing or loss of viability of the California spotted owl.*" (italics mine)

When presenting statistical information, scientists generally choose 95% confidence intervals and test hypotheses with an alpha level of 0.05 (5% chance of committing a type-I error). In part the decision on these "levels" is arbitrary, and it is largely dogmatic (i.e., it's what everyone does and what we've always done). Nonetheless, the *meaning* of the numbers or *their interpretation* may change if we were to choose an 85%, 90%, or 99% confidence interval and/or an alpha level of 0.15, 0.1, or 0.01. As the numerical value in front of the confidence interval (CI) increases, the intervals get increasingly larger (99% CI are larger than 95% CI) because we are requiring more certainty that the difference/effect/trend we are evaluating is "real". Thus, when a 95% CI for lambda (rate of population change) overlaps 1.0 (1.0 = a stable population), we fail to reject the null hypothesis (null = no trend or no difference between observed trend and a lambda of 1.0) using an alpha level of 0.05. We are making an inference about the degree of difference between the observed population trend and a stationary (lambda = 1.0) population. When the 95% confidence intervals around lambda overlap 1.0, we are not 95% sure that the population is stable. Instead, we are NOT 95% sure that the population is declining (or potentially increasing, but that isn't the case with the CSO).

The alpha (or probability of committing a type-I error) sets the standard of how often we wish to say there is an effect/trend/difference, when in reality there is not (at alpha = 0.05, we are saying that we're willing to make this sort of mistake 5% of the time). Thinking about this in medical terms, a type-I error is making a false positive diagnosis (saying one has an illness/disease when in reality they do not). However, the lower the alpha level, the higher the probability of committing a type-II error (i.e., saying there is no effect/trend/difference when in-fact there is). In medicine, a type-II error is making a false negative diagnosis (saying one does not have an illness/disease when in reality they do). Unfortunately, the relationship between type-I and type-II errors is not necessarily linear, but it is a negative relationship. When discussing the status of a sensitive species, or one that might be petitioned to be listed under the Endangered Species Act (I believe the CSO has been so petitioned in the past), weighing these various risks is important. Would you rather make the error of saying there is a negative effect/trend/difference when there really isn't OR would you rather make the error of saying there is no effect/trend/difference when there really is? The main point here is that the 95% CI in Franklin et al. (2004) [I believe it was mis-cited as a 2003 paper in the DEIS] are not sacrosanct, but were simply values chosen by those authors. In four of the five study areas evaluated by Franklin et al. (2004) the point estimates (the value around which the confidence intervals are placed) of lambda were <1.0. Relative to CSOs, the choice for land managers seems to be either: 1) focus on the 95% confidence intervals overlapping 1.0 and assume that in reality the CSO population is stationary, or 2) focus on the point estimates of lambda and assume that the CSO population is slowly decreasing.

The scientists responsible for the field and analytical efforts (Franklin et al. 2004) concluded their monograph on the demography of CSOs with the following sentence

> "*We also believe that all the demographic evidence available – such as estimated vital rates, rates of population change, and differences between paired studies – suggests substantial caution in owl conservation and management efforts*." (italics mine).

Whether or not the Empire project's impacts would result in a trend toward listing or loss of viability of CSOs cannot be judged with the information presented in the DEIS. Such an evaluation would entail weighing the effects of all other current and proposed projects within the species' range (clearly beyond the scope of this work). My opinion is that the Empire project DEIS does not adequately represent the uncertainty and risk associated with the proposed actions in terms of impacts to CSO relative to trends toward listing and/or viability. Including some of these (above) discussion points, acknowledging the uncertainties in amount of habitat, and CSO population trends in the final document should rectify this.

Northern Goshawk Habitat

The 200 acre (~79 ha) PACs around northern goshawk nest sites surely provides some protection to these birds and corresponds closely to the 83 ha scale that McGrath et al. (2003) reported to be important for goshawks. As with the CSO, however, we need to consider whether the analyses used in the DEIS adequately represents goshawk habitat. A recently published monograph on northern goshawks (McGrath et al. 2003) reported that goshawk nests occurred on the lower 1/3 of slopes and in drainage bottoms more than expected based on availability (and less than expected on the upper 1/3 of slopes and ridge tops). PNF personnel could modify their estimates of northern goshawk nesting habitat within the project area by: 1) removing all ridgetops from consideration as habitat, and 2) multiplying by 0.5 the current estimates of nesting habitat that exists within the upper 1/3 of slopes. As stated above, McGrath et al. (2003) found selection against upper 1/3 of slopes, but those areas weren't completely avoided. Instead, in their study, the upper 1/3 of slopes were used half as often as would be expected based on the availability of such areas (thus the recommendation to multiply by 0.5). McGrath et al. (2003) also reported that mean canopy cover at goshawk nests and random sites was 53.1% and 33.2%, respectively. Although goshawks can and do use areas with less than 50% canopy cover, assuming that 4 and 5 M stands are nesting habitat (as is done in the DEIS) might be underestimating the risk and uncertainty of the project alternatives. Dunk and Keane (unpublished analyses) used an information theoretic approach to model selection and compared 36 a priori habitat models at each of two spatial scales (100 and 200 ha) for northern goshawks on the PNF. They used CWHR habitat classes and some abiotic variables (e.g., percent slope, distance to nearest water source). Their best model included CWHR 4D, CWHR 5D, and percent slope; and the 100 ha model performed better than the 200 ha model. The probability of the stand being a nest site increased within increasing amounts of 4D and 5D.

Fisher

The fisher's historic range includes the PNF (Zielinski et al. 2005), but this species has not been found in the forest (or much of its historic range in the Sierra Nevada) for many years. As with the CSO and northern goshawk, the DEIS perhaps overestimates the amount of potential fisher habitat by including CWHR 4M and 5M stands (see rationale below).

Zielinski et al. (2004a) reported that fisher in California (northwestern coastal mountains and southern Sierra Nevada) used trees for resting that averaged 117.3 cm dbh for live conifers, 119.8 cm dbh for conifer snags, and 69.0 cm dbh for hardwoods. Furthermore, they reported that cavity structures were used for rest sites (more so by females) as were platform structures (more so by males). Their best "combined" (coastal and Sierra) fisher rest site model was:

$$W(x) = \exp((0.0618 * \text{average canopy cover}) + (0.0153 * \text{maximum tree dbh}) + (0.0229 * \text{percent slope}))$$

They used a variable-radius plot (20-factor prism) to estimate forest structural attributes (dbh of "in" trees), and spherical densiometers to estimate canopy cover. The positive coefficients in the above equation show that the relative probability (W(x)) of an area being a fisher rest sites increases when each of the variables in the model increases (i.e., with more canopy cover, larger maximum tree dbh, and steeper slope).

Zielinski et al. (2004a) also found that few fisher rest sites were re-used by individuals and suggested that fishers "...require multiple resting structures distributed throughout their home ranges." Of note to the Empire project, perhaps, is the following quote by Zielinski et al. (2004):

> "*In the sierra study area, the presence of nearby water and the contribution of hardwoods were more important model parameters than in the Coastal area, where the presence of large conifer snags was an important predictor. Based on our results, managers can maintain resting habitat for fishers by favoring the retention of large trees and the recruitment of trees that achieve the largest sizes. Maintaining dense canopy in the vicinity of large trees, especially if structural diversity is increased, will improve the attractiveness of these large trees to fishers*." (italics mine)

Zielinski et al. (2004b) reported descriptive measures of fisher home ranges in their two California study areas (same as mentioned above). They found that the greatest proportion of home ranges in their Sierra study area were in CWHR structural type 4D, and primarily in Sierran Mixed Conifer habitats followed by Ponderosa Pine (those two vegetation types represented >72% of all types within home ranges in the Sierra study area). Zielinski et al. (2004b) did not evaluate fisher selection of habitat types at the home range scale (i.e., whether habitat types were used in proportion to their occurrence within the study area). Nonetheless, their descriptive values provide insight into what is used by fishers. Carroll et al. (1999) modeled fisher habitat suitability in northwestern CA and found canopy cover (positive relationship with fisher detections) to be an important variable when evaluated at the scale of 10 km^2 around sample locations – mean canopy cover at fisher detection sites was 66.7% at the 10 km^2 scale.

Marten

My comments on marten are not as extensive as for the other three species, in part owing to my not being as familiar with the marten literature. Nonetheless, many of the similar themes that I've discussed above also hold for marten. In a review of fisher and marten, Powell et al. (2003) stated "In western North America, the need for old growth forests is fairly clear." This was in reference to both fisher and marten. Wilber et al. (2000) found martens to be more associated with forests containing old-growth characteristics in the winter than during other times. Thus, a re-evaluation of including CWHR size classes 4M and 5M (or some portion of those types) as being suitable marten habitat seems warranted.

Maintaining and Enhancing Ecological Health

The Individual Tree Selection (ITS) target trees, as outlined in the DEIS (page 29), would be

> "*High risk, unhealthy, or poor genetic-quality commercial trees (having excessive lean, dead tops, mistletoe infections, blister rust infections, heart and root rots, severe bole damage, forked stems, or fading or* chlorotic *foliage).*"

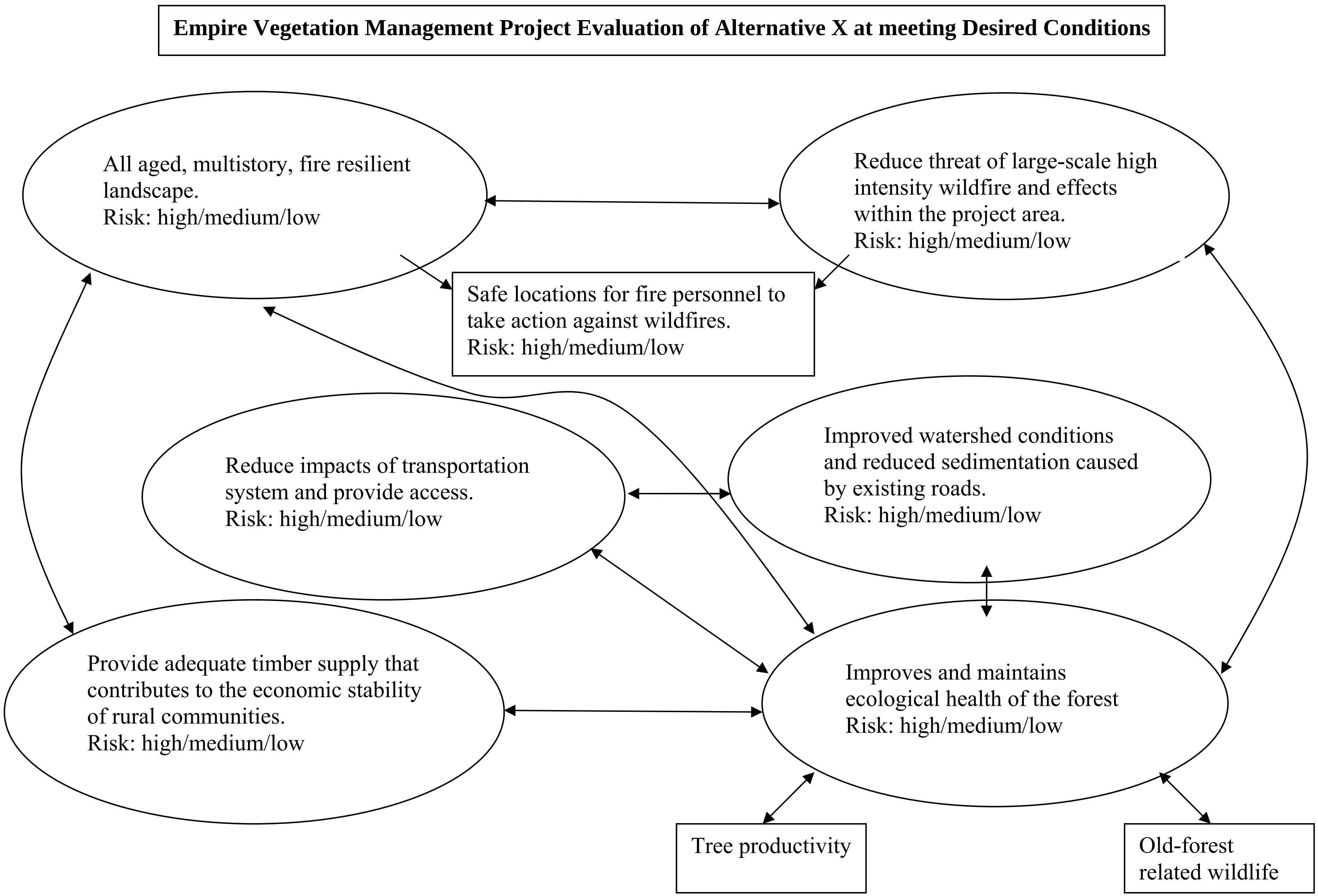
Trees with some of these characteristics are those, when allowed to get large enough, that have much value to both northern goshawks and CSO (and perhaps to fisher and marten). For the old-forest related birds, such trees often provide platforms on which goshawks make nests, and CSO may subsequently use to nest (owls do not build their own nests). Taking all such trees from these units may present a greater risk to these species than leaving some of them. These trees are not necessarily the same as snags, so the snag retention guidelines articulated do not necessarily cover this issue. Leaving some of these trees in ITS units could contribute to the long-term recruitment of large, valuable trees to old-forest related wildlife. "Legacy" trees have been shown to be of value to wildlife even in highly managed forests (Franklin et al. 2000, Hunter and Bond 2001, Mazurek and Zielinski 2004). Leaving a few such trees in ITS units (and leaving them when, over time, ITS units become group selection units) would likely decrease the risk of deleterious effects to old-forest related wildlife over the Empire Project area in the long-term. Maintaining and improving ecological health is one of the project's goals. If one includes old-forest wildlife values in the definition of ecological health (a reasonable position in my opinion), then maintaining or enhancing the value of the area for such species is one way of meeting this project goal.

Figure 3.1 (page 58) of the DEIS is a histogram of tree diameter distributions among ~60 stand exam plots. If we assume that these data are representative of the planning units within the project area, then we have an approximation of the current conditions of the area – relative to diameter distributions. The DEIS states that maintaining and improving ecological health and moving toward an all aged forest are goals. Can another figure be added that shows what the desired future diameter distribution would look like (in 20, 40, or 100 years)?

Figure 3.1 also shows that trees >20" dbh are quite rare in the project area. If trees >30" dbh are to be retained in GS units and current ITS units are likely to become future GS units, how are large trees going to be recruited in the project area over time? (I say this recognizing that no harvesting will occur within PACs, but see above for discussion of why PAC protection alone might increase risks to CSOs). I also recognize that recruitment of large trees is not a project-specific goal. However, maintaining and improving ecological health of the project area is a goal. As stated above, if maintaining and/or improving the project area for old-forest wildlife fits within the "umbrella" of ecological health, then recruiting large trees would be a defacto project objective. It seems that the >30" dbh trees that are left during the first GS harvest are likely to be the only large trees that will remain in these areas over time. If 20" dbh was the upper threshold of harvest for all treatments, the initial difference in number of trees remaining on the landscape would be small; but over time it appears that recruitment of larger trees (defined for this argument as 20" or greater dbh) would be more likely. Perhaps some forest growth models could shed some light on this.

Synthesis

The DEIS seems to lack a grand "synthesis" of the various project alternatives relative to the project's several stated goals and objectives. Perhaps a series of figures showing the relationship of each of the project's goals and the degree to which goals are met and risks increase or decrease by alternative would be a helpful addition. I've drafted such a diagram below – the same could be done for each of the various alternatives. I've attempted to draw linkages with arrows. This is not meant to be exhaustive, but an example – and perhaps a reasonable tool for the various authors of the DEIS to rank the risk in their area(s) of expertise. The "risk" I've envisioned is the risk of NOT MEETING THE PROJECT GOAL(S).


Empire Vegetation Management Project Evaluation of Alternative X at meeting Desired Conditions
All aged, multistory, fire resilient landscape.
Risk: high/medium/low
Reduce threat of large-scale high intensity wildfire and effects within the project area.
Risk: high/medium/low
Safe locations for fire personnel to take action against wildfires.
Risk: high/medium/low
Reduce impacts of transportation system and provide access.
Risk: high/medium/low
Improved watershed conditions and reduced sedimentation caused by existing roads.
Risk: high/medium/low
Provide adequate timber supply that contributes to the economic stability of rural communities.
Risk: high/medium/low
Improves and maintains ecological health of the forest
Risk: high/medium/low
Tree productivity
Old-forest related wildlife

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SCIENCE CONSISTENCY REVIEW COMMENTS – FROM BRIAN WOODBRIDGE

Jeff Dunk and I spent a lot of time discussing the wildlife analyses and conclusions within the Empire Project DEIS both during and after our meeting in Quincy, and many of my comments overlap with his. To avoid simply duplicating Jeff's comments (which are likely better-stated than mine!), I have attempted to focus my comments on data gaps and analytical approaches that have not already been discussed.

California Spotted Owl – Science, Risk and Uncertainty in Determination of Project Effects
Within the Empire Project DEIS, the evaluation of effects to California spotted owls leans heavily on older published literature (i.e Verner et al. 1992) and policy documents relating to the HFQLG. Comparison of the project's expected effects to established management units such as PACs, HRCAs, and SOHAs may be useful to assess whether the project meets established management standards and guidelines, but does not necessarily measure the actual effects to occupancy of CSO territories and fitness of owls in the project area. Results of more recent studies of habitat use and population status of CSO provide a better foundation for analysis of the potential effects of this project.

1) Spatial aspects – A number of studies based on radio telemetry have found that activities of both northern spotted owls and California spotted owls tend to be concentrated within an area of roughly 500 acres. For example, in their Chico study area, Irwin and Rock (2003, 2004) found that the mean 75% fixed-kernel home range areas for both male and female CSO was 530 acres (mean for NSO and CSO at 8 study sites was 525 acres). Results from numerous other studies support the use of a roughly 500-acre analysis area for evaluation of effects to NSO/CSO within their "core use area" where habitat management activities would be most likely to have measurable effects to owls (Blakesley 2003, Zabel et al. 2003, Franklin et al. 2000, Bingham and Noon 1997). An evaluation of the number of acres to be harvested within a 500-acre area of each affected CSO activity center, in comparison with the amounts of suitable habitat (see #2 below) within that area, would allow comparison with these existing studies and highlight sites where negative effects are more likely to occur.

2) Habitat Attributes – There is a great deal of uncertainty inherent in the use of WHR classification of wildlife habitat; this uncertainty is compounded by the error rates of remotely-sensed vegetation data. These data are probably adequate for evaluating landscape-level changes in habitat types, but may not be adequate for evaluation of site-specific impacts to owl core areas. This was highlighted in the presentation of FIA results for various WHR types; the mean diameter of class 4 stands appeared to be well below the 11" minimum for that class – which is already below the size class of stands typically used for foraging by CSO. If these problems result in overestimation of the amount of suitable habitat within PACs/HRCAs, then the potential effects of the Empire Project on CSO will be underestimated. At the least, this potential problem should be expressed within the DEIS; but there are ways to improve the effects determination more directly.

If plot-level data for the CSO areas in question are not available, an assessment of FIA data for the WHR types considered to be suitable habitat (5D/5M/4D/4M) can be conducted and compared with published information of habitat use by CSO. This assessment may lead you to re-evaluate the suitability of lower-quality classes such as 4M, or suggest that only a portion of this type should be considered suitable. This approach is in keeping with studies of habitat use by CSO, which suggest that forest stands with certain attributes are used preferentially, and that 'habitat suitability' forms a curve from lower suitability (used, but less than expected) to high suitability (used more than expected). Two such studies that provide data that can be readily incorporated into your evaluation of habitat suitability are Irwin and Rock (2004) and Bond et al. (2004). Irwin and Rock (2004) found that probability of stand use by CSO increased strongly as basal area rose from 80 to 320 ft^2/acre (optimum range 160-320 ft^2/acre), and was also positively influenced by the number of trees/acre that were >26" dbh. As importantly, abiotic factors such asdistance to nest, distance to water, and elevation were very influential. Blakesley (2003) found similar results, with the amount of habitat dominated by large (>24"), dense (>70% canopy closure) trees within the roughly 500-acre nest area having a positive influence on site occupancy, survival, and nest success.

3) Evaluation of the potential effects of the proposed stand treatments within CSO HRCAs would be improved by a synthesis, whether informal or more formally via modeling, of the spatial and habitat attributes influencing CSOs and how the proposed treatments will likely change those attributes. For example, if treatment units are within roughly ½ mile of CSO nest sites (500 acres), occur within denser stands containing large trees (>26") at low slope position, the effects to CSO would be expected to be much greater than treatments outside of those conditions. This type of analysis greatly reduces the uncertainty of coarse-scale evaluation (WHR) and enables an improved description of risk.

Northern Goshawk:
There is a need to discriminate between information derived from protocol surveys, evaluation of accumulated historical information, and assumptions. The DEIS (p. 101) states that the PNF supports approximately 110 goshawk territories, but then states that the numbers are simply the accumulated records from surveys and incidental sightings; suggesting that the actual population size and density is unknown, and no estimate has been made. This information, along with the 1988 PNF LRMP requirement for a network of 60 territories to provide for a viable population, is ultimately used to support the effects determination for the project; but little information is provided to allow evaluation of population size, density, or viability.

One way to improve this evaluation is to use published information on goshawk territory density and spacing to estimate a range of values for the Empire project area. Data sets from studies in the western US (Woodbridge and Detrich 1994, DeStefano et al. 1994, Reynolds et al. 1994, Reynolds and Joy 1998) establish a range of densities from 1 territory/2,123 ac. to 1 territory/4003 acres; territory centers are roughly 1.9 to 2.3 miles apart (see also table 3.31 and 3.32 in the Status Review for the Northern Goshawk; USFWS 1998). These are crude densities; including both suitable and unsuitable habitat within the study areas; some are from areas very similar to the Empire landscape. Using this range, the expected number of territories within the Empire analysis area might range from 24 – 44 territories. Comparison of the density and spacing of known goshawk territories within the Empire analysis area to a range of expected values would provide a foundation for your subsequent discussion of population size and viability – are goshawks relatively rare in the project area? Common? Or quite unknown? If the area has not been entirely surveyed, what is the pattern of spacing in the surveyed areas?

Habitat Attributes – Table 3.12 in the DEIS provides data on the acreage of forest habitats in 4 WHR classes considered to be highly suitable for nesting by goshawks. The scientific foundation for this classification is not given, and this evaluation could be greatly improved in a manner similar to that described for California spotted owls above. Studies of stand structural attributes of goshawk nest sites in Sierra Nevada forests (Keane 1999, Maurer 2000) and in other similar forest types (McGrath et al. 2003) provide ample information to allow comparison of WHR classes to actual stand attributes typically used by goshawks. At the local level, simple GIS overlay of currently or recently-active nests within the analysis area with your current GIS layer would provide another foundation to the habitat classification scheme used in the DEIS. As with the CSO, these evaluations (I suspect) would suggest that WHR classes 4M and 4D are used, but proportionally less that 5D and 5M. To the extent that treatment units are located within 5M/5D stands, effects to goshawk nesting habitat would be proportionally greater than described in the DEIS; conversely, if treatments occur largely in 4M stands, a lesser effect would occur. Abiotic factors such as slope position and aspect also play a significant role in determining habitat suitability (better expressed as probability of use) for goshawks, and should be incorporated into an evaluation of project effects. Habitats located upslope and on ridges, or on south-facing slopes, has a reduced likelihood of use by nesting goshawks; hence potential effects can be reduced through project design that focuses treatments in areas less likely to be used, or at least the assessment can account for the influence of landscape position on habitat suitability.

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