REVIEW OF: METHODS TO COMPLETE WATERSHED ANALYSIS ON PACIFIC LUMBER LANDS IN NORTHERN CALIFORNIA

Review prepared for the National Marine Fisheries Service

by

Dr. Leslie M. Reid
USDA Forest Service Pacific Southwest Research Station
Redwood Sciences Laboratory, 1700 Bayview Drive, Arcata CA 95521

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Executive summary

The three questions of primary concern for this review are: 1) are the WDNR modules adequately and validly modified to suit local conditions, as required by the HCP/SYP? 2) is there an adequate "distinct cumulative effects assessment" method, as required by the HCP/SYP? 3) will the cumulative effects assessment method and the modified WDNR modules be adequate for providing the information required by the HCP/SYP?

Adequacy and validity of individual modules

Introduction

- The introduction states that each analysis must be completed in 2.7 months so that all 22 may be completed in a 5-year period. This perceived time limit is not valid, since multiple analysis teams can work simultaneously.

Mass wasting

- The method used to calculate "probability of failure" does not do so because it does not account for covariance.
- A standard "epidemiological" approach would provide the necessary information more efficiently by quantifying the information collected during the WDNR Level I analysis.

Surface erosion

- Field observations will be necessary during storms to determine the extent of overland flow and subsurface soil piping and thus allow valid estimates to be made of sediment delivery.
- Predictions of road-surface erosion rates will need to be compared to measured erosion rates to determine whether the equations developed for Washington conditions apply here.
- Predictions of surface erosion rates on logged areas will need to be compared to measured erosion rates to determine whether WEPP and SEDMODL provide accurate estimates.
- Effluent from waterbars will need to be considered in the same category as effluent from road ditches and culverts.

Hydrology

- The method proposed for determining "how well flood history is explained by climatic variables alone" is not capable of discerning the magnitude of change expected, given the variance that will arise from the lack of rainfall and runoff data for the specific watersheds under consideration.
- The model based on data from Caspar Creek will not adequately represent the hydrological effects of tractor yarding, extensive roading, or burning and herbiciding (in the controlled experiment, only half the clearcuts were burned and herbicided).
- The assumption that hydrologic recovery is complete in 12.5 years is not well-founded, being based on an extrapolation from a 5-year record before thinning. Calculations will also need to be carried out for more widely adopted hydrologic recovery rates, such as the 50-year period quoted in the mass-wasting module and the 30-year period used by the Forest Service.

Riparian function

- The assumption that a 40% riparian cover of 11-inch trees is sufficient to sustain woody-debris loadings characteristic of old-growth redwood channels is inappropriate.

- It is not appropriate to restrict analysis to a 170-foot distance from the channel and to exclude Class III channels. Cumulative effects on woody debris loadings cannot be assessed unless the full potential source area under old-growth conditions is assessed.
- It is not appropriate to defer to the prescription team responsibility for analyzing long-term cumulative effects of inadequate riparian stands.
- The proposed method for assessing canopy cover over streams is not valid. The water surface is not likely to be visible on air photos of even those low-order channels without canopy cover since air photos provide oblique views of any object located away from the center-point of the photo. Even trees located some distance from the channel thus would obscure the water surface. Results of the proposed method would be strongly biased toward invalid findings of sufficient cover.
- Factors other than canopy immediately over the channel will need to be considered to adequately evaluate cumulative impacts on stream temperatures.
- It is not possible to evaluate the cumulative effects of altered temperature on anadromous salmonids if potential impacts on thermal refugia are not evaluated.
- A cumulative effects assessment is not possible if tributaries contributing less than 20% of a channel's flow are ignored.

Channel

- A method will need to be described for assessing sediment production from destabilization of Class III channels.

Fish

- Forestry-related impacts to estuaries will need to be evaluated, since these are a very important component of anadromous fish habitat in northwest California.
- Cumulative effects on fish cannot be evaluated unless chemical contamination is evaluated, since chemical effects are synergistic with other effects. Because use of chemicals is a standard and predictable component of the land-use activities regulated by the HCP/SYP, it is not appropriate that this component be excluded from the analysis.
- Redefinition of the size of a key piece of woody debris from the 27" diameter, 80-foot length specified in the WDNR method for a 50-foot wide creek to the 4" diameter, 6-foot length used in this module is not appropriate.
- Turbidity apparently is to be monitored only during field surveys, which are to be carried out during periods when water is clear enough that other habitat attributes can be observed. This is not an appropriate study design. More information is needed about how chronic turbidity impacts will be evaluated.

Amphibians

- A procedure will need to be described for how the currently misclassified Class II channels will be reclassified.
- The method to be used for making "habitat diagnostic calls" and "vulnerability calls" will need to be described.
- A method will need to be developed for identifying potential habitat; the proposed method appears to be capable only of identifying existing habitat.

Cumulative effects

- The proposed method does not identify the cumulative effects that will be considered for evaluation. These will need to be listed.

- There are no methods described for assessing any cumulative effect. Development of field protocols for such analysis appears to be deferred to module leaders and to the prescription team, while analysis procedures apparently are intended to be developed during the synthesis step of each watershed assessment and during the prescription phase. This is not an appropriate approach; assessment methods will need to be developed before assessments are carried out or appropriate information will not be obtained.

Adequacy of cumulative effects assessment

- As presented, the cumulative effects assessment is not adequate: the cumulative effects to be assessed are not identified and no actual methods are described for how each cumulative effect will be assessed.
- The HCP/SYP requires development of "a distinct cumulative effects assessment" method. However, the proposed method effectively redefines "cumulative effects" to mean the very small subset of those effects that were already within the scope of the subset of WDNR modules to be used here. This redefinition is inconsistent with the mandate of the HCP/SYP, since the resulting assessment will be neither "distinct" nor a "cumulative effects assessment." Instead, it will simply be a functional equivalent of the original WDNR synthesis module.

Adequacy for intended decisions

- Because the cumulative effects that must be evaluated according to NEPA, CEQA, and the Forest Practice Rules will not be evaluated, it will not be possible to develop valid prescriptions for land use activities.
- The HCP/SYP requires that prescriptions meet the objectives of the Basin Plan. The proposed method does not provide the information necessary to design prescriptions that meet the objectives of the Basin Plan because impacts on the beneficial uses of water defined by the Basin Plan would not be evaluated.
- Decisions to modify the 100-foot no-cut buffer width cannot be made without a valid assessment of riparian function. The proposed method for assessment of riparian function is not valid.
- Decisions concerning modification of the mass-wasting avoidance strategy require estimates of probability of failure for particular land-use activities on particular landforms. The proposed method for estimating probability of failure is not valid.
- There is no indication of how information provided by the modules will be used to modify the disturbance index.

Conclusions

In its current form, the proposed method is not adequate for its intended applications. For some modules, changes required to produce a valid and useful assessment are relatively minor .

In the case of the riparian module, however, major changes in approach and in scope are required before a valid analysis is possible. Both the woody debris and stream temperature portions of the analysis are not capable of producing valid results. For example, according to the proposed method, a creek the size of Godwood Creek in Redwood National Park—a tributary currently lined by redwood trees with basal diameters of 50 to 200 inches and hosting woody debris of similar sizes—

would be *defined* to have conditions equivalent to those of an old-growth redwood forest if it had one 4-inch-diameter, 6-foot-long piece of wood every 40 feet and were located in a stand with a 40% canopy cover of 11-inch diameter trees.

Of additional concern is the absence of a cumulative effects assessment method. It will be necessary for an assessment method to be developed that explicitly identifies which cumulative effects are to be considered during assessment and describes how those cumulative effects are to be assessed. Furthermore, the proposed method appears to restrict analysis to the effects of a subset of the ongoing forest management activities on a small subset of the cumulative impacts of concern in a subsection of each watershed which excludes areas in which important cumulative effects on anadromous salmonids are likely to occur. Even if assessment methods had been described, this would not constitute a valid cumulative effects assessment.

As currently presented, the proposed method will not provide the information necessary to modify the default prescriptions of the HCP/SYP. Design of valid prescriptions will require valid cumulative effects assessments.

Addendum

Review of public comments concerning an earlier draft indicates that the community's comments were largely ignored. Had these comments been seriously considered and suggestions incorporated into the proposed method, many of the problems outlined above would have already been addressed.

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Dr. Leslie M. Reid
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Introduction to review

Staff of the National Marine Fisheries Service asked me to review this document and, in particular, to evaluate the effectiveness of the proposed method for evaluating cumulative watershed impacts. Because there is no distinct cumulative impact evaluation method included in the document, it was necessary to review the entire document to determine the extent to which information provided by the modules would contribute to a valid cumulative impact assessment. The review that I have prepared is based on my own research and on research published by others, and on observations I have made in the area. This technical review represents my own professional opinion as an expert in cumulative watershed impacts, hydrology, and geomorphology, and the review should not be construed as reflecting an official policy position of the USDA Forest Service.

It is important to note that technical reviews, by their nature, focus on opportunities for improvement of a report's content. Technical reviews are intended to point out errors, omissions, and weaknesses in technical content and reasoning. Given limited time available for preparation of reviews, it is not standard practice—and, indeed, is not possible—to acknowledge the points for which no change would be suggested.

Summary of review

Adequacy of the proposed standardized modules

The document under review describes a method for assessing watershed conditions on properties owned by Pacific Lumber Company and Scotia Pacific Holding Company. The document was prepared pursuant to requirements of the HCP/SYP adopted in March 1999 for the properties in question. The HCP/SYP requires that watershed assessment be carried out for each of 22 watersheds on the ownership. Methods are to be based on those developed for Washington State (WFPB 1997) but are to be modified to reflect conditions in the area of concern.

As a background for the review, it is useful to examine the expected differences in conditions between northwest California and Washington. These differences would presumably provide the basis for alterations in the WDNR method:

1. The ownership includes geologic substrates and tectonic settings not found in Washington, leading to differences in erosion processes, erosion rates, and runoff characteristics. Erosion equations developed for Washington thus will need to be modified before they can be confidently applied to the area. The distinctive history of interactions between uplift and sea-level change in northern California increases the importance of the estuarine portions of

watersheds and also increases the susceptibility of estuaries to environmental impacts from upstream land-use activities.

2. The forest type of primary concern—coastal redwood forest—is not found in Washington. Redwoods are world-renowned for their longevity, size, and decay resistance. Pre-management stream channel characteristics, riparian microclimates, hillslope hydrology, slope stability characteristics, and woody debris loadings will all reflect the peculiarities of this forest type.
3. The California coast-range climate is different from that in Washington: summer temperatures are warmer than those in western Washington, leading to warmer summer temperatures in streams draining the interior coast ranges. Species that require cold-water environments thus rely on strategies for coping with warm water that may not be as important in western Washington. In particular, the presence of thermal refugia in the form of deep pools and small, cold-water tributaries is very important.
4. The land-use history is different. Logging and roading practices have been different from those used in Washington, leading to differences in erosion processes, erosion rates, and runoff characteristics. In addition, most sites are experiencing the second or third logging entry, thus increasing the potential importance of multi-cycle cumulative effects.
5. The regulatory context in California is different than from in Washington, and coho and chinook salmon have been listed as threatened since the publication of the 1997 version of the WDNR assessment method.
6. The level of knowledge has increased since the Washington DNR procedure was developed. The WDNR method notes that “Periodic revision and incorporation of new methods and insight is a fundamental assumption of the diagnostic approach upon which this module relies” (WFPB 1997 p.E-75; quoted in the proposed Channel Module, p.62 ph.3). New information, and particularly information concerning watershed processes in northwest California, would need to be incorporated into the WDNR method to ensure its relevance to this area.

An adequate and valid modification of the WDNR modules would reflect these differences between conditions today in the area of concern and conditions that pertained in Washington State when the WDNR method was being developed to apply to that area. In addition, the proposed cumulative effects assessment consists of analysis of information provided by individual modified WDNR modules. To be adequate, then, the modified modules also must be capable of producing the information that would be required for a valid cumulative effects assessment. Each of the modified modules is at least partially independent of the others, so each is discussed independently below.

First, a brief note is required concerning the perceived limitations on the assessment that are described in the **introduction** section. This section explains that only 2.7 months are available for each assessment, since 22 watersheds must be assessed in 5 years. However, this is not a valid argument, as there is no reason that multiple teams cannot be working simultaneously. The section also implies that fieldwork is strictly limited under the WDNR approach. This also is not the case, as the standard method for WDNR analysis states that “Recognition of when, where, and how to undertake more detailed analyses necessary to adequately understand watershed processes is a crucial component of Watershed analysis that must not be constrained prior to conducting the standard analysis” (WFPB 1997; quoted in the proposed Channel Module, p.62 ph.1). WDNR Level 2 analysis is required for PALCO lands, and the WDNR manual states that “Level 2 specialists have flexibility in methods which allows the team to develop and test hypotheses, responding to the findings of the

Level 1 assessment" (WFPB 1997, p.16) and that "more time may be needed to perform a Level 2 analysis" (WFPB 1997, p.15).

The methods outlined in the **mass wasting module** are not sufficient to provide an estimate of natural background landslide rates or to determine the effects of land-use activities on those background rates. Such information is necessary if a valid disturbance index is to be developed. Further, the method described as providing a "probability of failure" does not do so, instead producing the equivalent of a sensitivity analysis for unmeasured variables. To estimate probability of failure, it would be necessary to determine the extent to which variables co-vary, and this is not done. Results of the modeling thus cannot be usefully interpreted. The module would be more useful and easier to conduct if a standard "epidemiological" analysis were undertaken: the qualitative and categorical information produced by the Level I WDNR analysis would be replaced by quantitative information, as suggested in the WDNR module for Level II analysis.

The **surface erosion module** includes methods to evaluate road-surface erosion, surface erosion from logged areas, and background erosion rates. Several changes to the method would be necessary to ensure that results of the analysis are valid for conditions present in northwest California. First, it will be important to make observations during storms to determine the extent of overland flow and subsurface soil piping. Without these wet-season observations, it will not be possible to assess the potential for delivery of surface-erosion sediment to streams. Such fieldwork would also allow sampling of road-surface runoff to determine whether the WDNR equation for road-surface erosion applies to the geologic conditions, climatic regime, and road standards present in this area.

The WEPP and SEDMODL approaches will also need to be tested for the area; the proposed approach of guessing suitable parameters for WEPP and testing results against the unvalidated SEDMODL results does not constitute an adequate approach. In addition, it will be necessary to evaluate tractor skid trails as roads rather than as a component of a homogeneous logged surface, since these skid trails can remain compacted and unvegetated for long periods in this area. Effluent from waterbars should be considered in the same category as effluent from road ditches and culverts in calculations of delivery ratios; this modification is necessary because of the road management practices in use in the area.

In addition, more information is needed concerning how process information will be combined to estimate background erosion rates. As currently presented, it appears that background rates will be overestimated because processes that occur "in series" are being treated as though they operated "in parallel": soil creep rates cannot be added to bank erosion rates because soil creep supplies the sediment that is eroded by bank erosion.

The **hydrology module** contains two components: 1) construction of a rainfall-runoff model to determine "how well flood history is explained by climatic variables alone," and 2) application of an equation for predicting peak-flow changes as a function of percent of watershed logged. This module was constructed specifically to address the conditions in the area. Several aspects of the method will require modification to ensure the validity of the results and to provide the information necessary for a cumulative impact assessment. First, the plan for rainfall-runoff modeling is inadequate for detecting the level of change expected. Under the study design, weather patterns will be found to be sufficient to explain flooding if no significant relationship is found between land-use variables and flood peaks. However, for a significant relationship to be found, data must be accurate enough that variance is sufficiently low to allow the relationship to be identified. In this case, there are no flow records or rainfall records for most of the watersheds in question. Application of data from elsewhere will necessarily increase the variance of the model results to the point that absence of

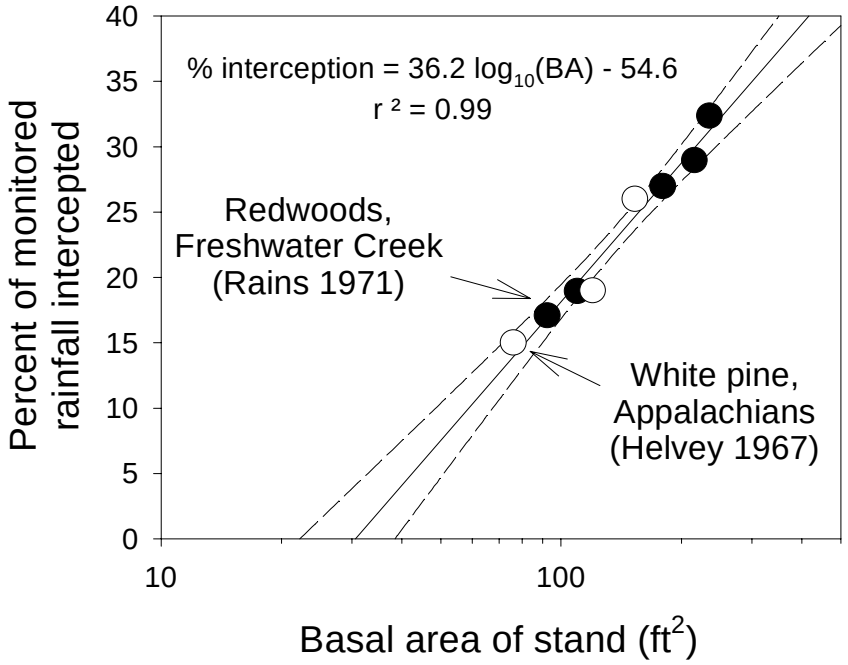

Figure 2. Relationship between stand basal area and percent interception(from Reid 1999)

statistically significant relationships is almost guaranteed. The method is simply not capable of detecting the level of change that would be expected, given what is already known about the magnitude of peakflow increases in similar areas.

The second part of the hydrology module employs a model for predicting peak-flow change that was derived using 4 to 6 years of data collected after clearcutting of 49.6% of North Fork Caspar Creek watershed. Minimal tractor yarding was used (7.6% of the watershed), and less than half of the logged area was burned and herbicided (19.5% of the watershed). Roads in the area are built along ridges, and road density is uncommonly low. The model will thus underestimate the hydrologic effects in watersheds with higher road densities, more widespread use of burning and herbicides, and a higher proportion of tractor yarding.

The model also assumes that hydrologic recovery is complete after 12.5 years, but several kinds of evidence suggest that this assumption is not well-founded. First, other chapters of the document indicate that 50 years are required for hydrologic recovery (for example, Mass Wasting, p.28). Second, relations between basal area and interception loss suggest that a substantially longer period for recovery is likely *(Figure 1, Figure 2).* Third, the estimate of 12.5 years is based on extrapolation from only 5 years of data. And finally, those 5 years of data did not include the effects of pre-commercial thinning, which will again substantially reduce canopy cover. Given multiple estimates of the period required for hydrologic recovery, it is not reasonable that the minimum of these value was selected. A valid analysis would perform the calculations for a range of potential recovery rates.

Furthermore, the hydrology module does not provide sufficient information to evaluate cumulative impacts on peakflows because some mechanisms for peak-flow alteration are assumed to be inconsequential. The NEPA definition of cumulative impact specifies that “Cumulative impacts

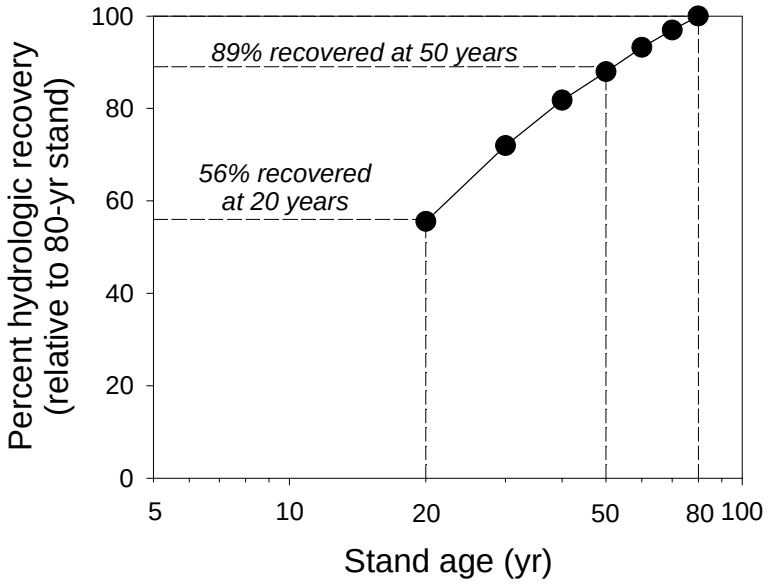

Figure 3. Estimated hydrologic recovery as a function of stand age (from Reid 1999)

can result from individually minor but collectively significant actions taking place over a period of time" (CEQ Guidelines, 40 CFR 1508.7, issued 23 April 1971). In the present case, the effects of both rain-on-snow melt and road runoff are to be excluded from calculations because they are assumed to be small. Road runoff is dismissed in part because of the problem of "synchroneity." However, the synchroneity argument is not valid where the road network is largely in place throughout the watershed, and it is not valid at the scale of small watersheds that are of concern for gullying. The argument that rain-on-snow events are uncommon also is not valid: it is the uncommon events that cause floods, and the 1964 flood occurred a few days after snow had fallen to sea-level. We need to know the likelihood of rain-on-snow events, and the extent to which an incremental increase in peakflow from rain-on-snow melt could augment increases from the other mechanisms of concern. Analysis of the cumulative impact on peakflow would require analysis of the combined effects of decreased foliage interception, downstream aggradation, increased road-related runoff, altered frequency of rain-on-snow melt, and other influences.

The **riparian function module** is restricted to assessing 1) the adequacy of riparian stands for woody debris input and 2) the adequacy of the forest canopy immediately over streams for stream temperature regulation. Neither of the analysis methods provided is adequate for addressing the problems posed, and neither provides sufficient information for evaluating cumulative effects on these riparian functions.

For large woody debris, the module states that "The LWD target is established at a level of in-channel LWD occurring within unmanaged lands such as Redwood National Park" (Riparian module, p.8 ph.1); the target level is thus implied to be that characteristic of old-growth conditions. The module then incorporates a method that defines a riparian stand with a 40% cover of 11-inch trees to be as capable of producing the target conditions as an old-growth redwood forest with a 90% cover of 48-inch to 80-inch trees. This reasoning is not logical. It is not possible for 11-inch-diameter trees to produce woody debris of sizes characteristic of old-growth redwood forests. Even if current levels of woody debris loading in a particular reach were equivalent to old-growth conditions, that debris

would not persist long enough for the 11-inch trees to become capable of producing old-growth logs by the time they are needed to replenish the wood. Only under the conditions that existing loadings are equivalent to those in old-growth forests *and* the adjacent stand is currently adequate for maintaining those loadings is it possible that no "hazard" call is required. Even under these conditions, the call would need to be tempered by information concerning upstream loadings, as a portion of the wood is expected to move downstream during major storms. It is also not appropriate to defer consideration of long-term cumulative impacts of inadequate riparian stands.

For stream temperature, the analysis focuses on assessing the current canopy condition immediately over a stream channel. The method to be used, however, is incapable of providing valid estimates of canopy cover. Cover is assumed to be adequate if most of a stream's water surface is not visible on a 1:12,000 aerial photograph. However, only the center point of an aerial photograph provides a vertical view of the landscape; farther from the center point, the view is increasingly oblique. Thus, the water surface of streams located increasingly far from the center point will be obscured increasingly by vegetation near to—but not above—the channel. Further, it is generally not possible to see the water surface of a Class II channel on a 1:12,000 aerial photograph. The method is guaranteed to systematically over-estimate the canopy cover present. In addition, the method specifies that only those reaches that have been found to be deficient in cover will be systematically field-checked, making it impossible to determine that the method is invalid.

Furthermore, only the canopy immediately above the stream is considered to be an influence on stream temperature. The effects of altered microclimate, non-vertical sun-angles, and warming of near-surface groundwater in Class III channels are all ignored. All of these influences on stream temperature would need to be considered if the cumulative impacts on stream temperature are to be assessed. If the cumulative impacts on anadromous fish from altered stream temperatures are to be considered, then it would also be necessary to assess the influence of upstream land use on potential temperature refugia along large channels. The module indicates that no such analysis of cumulative impacts will be possible because small streams entering large tributaries will be ignored; it thus will be impossible to assess the cumulative impacts even of altered canopy cover on temperatures and thermal refuge distribution in some of the channels most important to anadromous fish.

The **channel module**, as presented in the WDNR manual and reproduced in the proposed method, allows a comprehensive approach to geomorphological analysis of the nature and causes of channel change in a watershed. Sufficient latitude is provided to allow whatever analysis is needed to answer the critical questions. The supplementary analyses provided in the appendix to the module details some of the specific methods that might be used in this area. In several cases, however, the methods recommended in the original module (e.g., using datable vegetation to estimate rates of aggradation) may be more useful than those specified in the appendix. It will be important to include a description of how gully erosion will be assessed.

The **fish module** does not provide sufficient information to determine how cumulative effects of chronic turbidity will be assessed. The module seems to indicate that turbidity will be measured only during habitat assessment fieldwork, but this fieldwork is presumably to be done at a time when water is clear enough that the necessary habitat parameters can be observed. This approach would strongly bias the results toward an underestimate of turbidity-related impacts. The module also modifies the definition of "key piece" of woody debris from that presented in the WDNR method. In western Washington fir/hemlock forests, a 50-foot wide stream would require "key pieces" of 80-foot length and 27 inches in diameter. Under the proposed method, however, an equivalent stream in a redwood forest would require only 6-foot lengths of 4-inch diameter. This modification is not

appropriate. It is also not appropriate to exclude consideration of potential chemical contamination from road use, heavy equipment use, spills, fertilizer applications, and herbicide use. These effects cause synergistic cumulative impacts when combined with temperature changes and altered turbidity; valid assessment of the cumulative effects of altered sediment and temperature requires assessment of chemical contamination. Finally, estuarine environments are critical to the survival of anadromous salmonids, and these environments are highly susceptible to cumulative impacts of forest management activities. It is not appropriate to exclude from the cumulative effects analysis those cumulative impacts that are caused by PALCO land use activities but that occur downstream of PALCO ownership. It is the off-site cumulative impacts that are generally of the greatest concern.

The **amphibian module** does not supply sufficient information concerning how the Class II channels currently misidentified as Class III's will be identified and reclassified. More information is also needed concerning what "habitat diagnostic calls" and "vulnerability calls" are and how they will be made. The protocol for identifying potential habitat will need to be revised, since the proposed method employs diagnostics that will have been altered by past forest management activities; the method is thus capable only of identifying existing habitat, not potential habitat.

The **cumulative effects module** does not explicitly identify the effects that will be evaluated and it does not describe methods for evaluating those effects.

Adequacy of the proposed method for cumulative impact assessment

Due to the nature of the HCP/SYP, a federal EIS and a state EIR were required for the plans; these were prepared as a combined EIS/EIR. EIS's and EIR's both are required to evaluate the cumulative impacts of the planned activities, in combination with other past, present, and reasonably foreseeable future activities. However, review by state agencies found that the cumulative impact assessment contained in the HCP/SYP and in the EIS/EIR was inadequate, and on this basis denied approval of the cumulative impact component of the SYP. The final HCP/SYP requires that "a distinct cumulative effects assessment" method be developed (USFWS and CDF 1999a, p.P-37, item 7), and that method is included as part of the document under review. This review evaluates the ability of the proposed method to assess the cumulative impacts of land-use activities associated with implementation of the HCP/SYP.

Furthermore, the HCP/SYP specifies that certain modifications may be made to default HCP/SYP prescriptions on the basis of watershed assessment results. The default prescriptions included in the approved HCP/SYP are those specified by California Assembly Bill 1986. The ability of the proposed cumulative effects assessment to provide sufficient information for modifying the default prescriptions thus is also evaluated in this review.

To provide a context for this review, it is useful to first describe the kind of cumulative effect evaluation required by CEQA, NEPA, and the California Forest Practice Rules for development of land-use prescriptions. The Council on Environmental Quality defines cumulative impacts for the purposes of NEPA as

> *"...the impact on the environment which results from the incremental impact of the action when added to other past, present, and reasonably foreseeable future actions regardless of what agency...or person undertakes such other actions"* (CEQ Guidelines, 40 CFR 1508.7, issued 23 April 1971).

The guidelines for the California Environmental Quality Act provide a similar definition:

> *"'Cumulative impacts' refers to two or more individual effects which, when considered together, are considerable or which compound or increase other environmental impacts"* where *"the individual effects may be changes resulting from a single project or a number of separate projects"* (Governor's Office of Planning and Research 1994).

Technical Rule Addendum No. 2 of the California Forest Practice rules (CDF 1998) further specifies the kinds of cumulative watershed impacts that must be evaluated. Among other issues, the analysis is to consider "significant on-site and down-stream cumulative effects on beneficial uses of water, as defined and listed in applicable Water Quality Control Plans" *(Table 1)*, and is specifically to consider effects of sediment, water temperature, organic debris, chemical contamination, and peak flow.

An adequate cumulative impact assessment procedure thus would include methods for identifying and assessing potential cumulative impacts on the beneficial uses listed in Table 1. In addition, potential cumulative impacts on species of concern to the HCP are of particular importance. In this context, an adequate cumulative impact analysis would include methods to evaluate the potential for impacts on each of the life stages of the species of concern and for the cumulative impact of the combined influences.

Review of the document indicates that, contrary to the requirements of the HCP/SYP, there is no distinct cumulative effects assessment method. Instead, the modified WDNR procedure provides for (1) preliminary issue identification, (2) selection of cumulative effects issues that fall within the purview of the WDNR procedure, (3) implementation of standardized assessment methods for evaluating landslides, surface erosion, hydrologic change, riparian function, channel form, fisheries, and herpetofauna, and (4) a module analogous to the "synthesis" module of the WDNR procedure. The modified "synthesis module" is labeled the "cumulative effects module" but does not identify the cumulative effects to be evaluated and contains no methods for evaluating cumulative effects. Instead, the module suggests that the necessary information will be gathered in the course of the WDNR module assessments, and that module leaders will decide what information is necessary. Cumulative effects assessment appears to consist simply of "clustering" the causal mechanism reports produced during the standard WDNR synthesis module and estimating the proportional contribution of different mechanisms.

The proposed procedure thus does not allow for interdisciplinary fieldwork focused on evaluating particular cumulative effects, and will instead rely on information gathered by monodisciplinary teams in the course of standard WDNR module work. It is not possible to validly and effectively assess cumulative effects using only data collected by monodisciplinary teams for other reasons. Instead, hypotheses concerning cumulative effects must be developed and tested by looking for particular kinds of evidence in the field. Identification of appropriate hypotheses, data, and analysis methods are inherently interdisciplinary tasks.

Furthermore, the method appears to have redefined "cumulative effects" to include only a subset of the cumulative impacts that need to be evaluated under NEPA, CEQA, or the Forest Practice Rules. This change is not completely evident in the module, where it is stated that "The PALCO watershed analysis process will not consider issues that: 1) are outside scope of the analysis as defined by the HCP..." (Cumulative Effects Module p.6, ph.1). The module also notes that "Issues included in watershed analysis will be selected based on the relevance to the watershed assessment guidance provided by the HCP" (Cumulative Effects Module p.6, ph.1). That these statements are placed in the cumulative effects module indicates that the "distinct cumulative effects assessment" called for by the HCP/SYP is considered to be simply a procedural component of the standard

Table 1. Beneficial uses of water defined by Water Quality Control Plans (North Coast Regional Water Quality Control Board 1994)

Beneficial use	Description
Municipal and domestic supply	Community or individual supply for drinking and other uses
Agricultural supply	Irrigation, stock watering, forage development, etc.
Industrial service supply	Industrial activities not dependent on water quality
Industrial process supply	Industrial activities dependent on water quality
Groundwater recharge	Replenishment for extraction, water quality maintenance, or control of saltwater intrusion
Freshwater replenishment	Maintenance of surface water quantity or quality
Navigation	Shipping or travel by private, military, or commercial vessels
Hydropower generation	Power generation
Water contact recreation	Swimming, fishing, white-water activities, etc.
Non-contact water recreation	Picnicking, hiking, boating, etc.
Commercial and sport fishing	Collection of fish, shellfish, etc. for consumption or bait
Aquaculture	Includes cultivation of aquatic biota for consumption or bait
Warm freshwater habitat	Preservation or enhancement of warm-water ecosystems
Cold freshwater habitat	Preservation or enhancement of cold-water ecosystems
Inland saline water habitat	Preservation or enhancement of inland saline-water ecosystems
Estuarine habitat	Preservation or enhancement of estuarine ecosystems
Marine habitat	Preservation or enhancement of marine ecosystems
Wildlife habitat	Preservation or enhancement of wildlife ecosystems
Preservation of areas of special biological significance	Includes marine life refuges, ecological reserves, etc.
Rare, threatened, endangered species	Support of habitats used by listed species
Migration of aquatic organisms	Support of temporarily occupied habitats
Spawning, reproduction, and/or early development	Support of high quality habitat for producing fish
Shellfish harvesting	Collection of clams, oysters, etc. for sport or commercial use

WDNR method, and that the cumulative effects assessment is completely constrained by the scope of the WDNR watershed assessment protocol. Statements reported to have been made by PALCO consultants at public meetings suggest that only cumulative impacts on peakflows and species covered by the HCP will be considered, despite the fact that the HCP/SYP placed no restriction on the scope of the required "distinct cumulative effects assessment" (USFWS and CDF 1999a, p.P-37, item 7). It is not logical to restrict the scope of the "distinct cumulative effects assessment" to reflect that of the WDNR watershed assessment, since the reason that a "distinct cumulative effects assessment" was required by the HCP/SYP was presumably that the scope of the WDNR method is too narrow to allow adequate assessment of cumulative impacts, as defined by CEQA, NEPA, and the Forest Practice Rules.

It is of particular concern that it is not possible to determine from the module itself which cumulative effects are intended to be analyzed; the only examples presented in the module of what

might be assessed are "...flooding and impacts on salmonid habitat and populations" (Cumulative Effects Module p.4, ph.2). To be useful, a cumulative impact assessment method would need to identify the impacts to be evaluated and provide a description of the methods that will be used to evaluate them.

Although it is not clear which cumulative effects will be considered for assessment, the proposed method specifically excludes consideration of chemical contamination—an on-going and reasonably foreseeable aspect of forest land management on the ownership in question—because herbicide and fertilizer use "are not considered under PALCO's HCP and are therefore not part of this Watershed Analysis Methodology" (Fish module p.7, ph.1). If this statement were true, the exclusion would be arbitrary because other activities not considered by the HCP—including those carried out by other land owners on other ownerships—are to be evaluated in other modules (e.g., impacts of urbanization and dike construction). However, the statement is not true. The HCP/SYP documents consider use of herbicides in a lengthy discussion (USFWS and CDF 1999b, p.3.14-1 through 3.14-19) and include the conclusion that the company's use of herbicides and pesticides may have a significant impact on some aquatic species (USFWS and CDF 1999b, p.2-44). Furthermore, chemical contamination from road runoff and heavy equipment use is an expected impact from activities specifically covered by the Habitat Conservation Plan. Because chemical contamination is synergistic with impacts from increased stream temperature and increased chronic turbidity levels, cumulative impacts of altered temperature and sediment cannot be adequately evaluated unless they are considered in the context of the "*other past, present, and reasonably foreseeable future actions"* with which the impacts are expected to accumulate. Both NEPA and CEQA definitions of "cumulative effects" indicate that reasonably foreseeable future actions must be considered in a valid cumulative effects assessment. Further, both definitions specify that a cumulative effect is the result of multiple activities, thus indicating that the combined effects of those activities would need to be evaluated in a valid cumulative effects assessment.

The proposed method also excludes portions of the watersheds from analysis. Estuarine portions of the watersheds are excluded, as are several other parts of the "WAAs" that had been evaluated in the HCP/SYP (see USFWS and CDF 1999b, Figure 3.4-2). The HCP/SYP requires that "The entire watersheds/analysis areas where PALCO owns all or portions of the land will be assessed. A Level 2 assessment...is required for all lands PALCO owns in specified watersheds/analysis areas at the time of the analysis. A Level 1 assessment is required for lands not owned by PALCO at the time of the analysis" (USFWS and CDF 1999a, p. P-37, Item 10). Cumulative effects are expected downstream of an ownership. If the areal scope of analysis is designed to exclude consideration of impacts in susceptible areas downstream of the ownership, the cumulative effects analysis is not useful or valid. Courts have already provided several judgments indicating that it is necessary to evaluate cumulative impacts in estuaries when forest management prescriptions are being designed through the THP process. The exclusion from consideration of cumulative impacts to beneficial uses downstream of an arbitrary cut-off point makes it impossible to evaluate cumulative effects on the recently listed chinook salmon, since this species is heavily dependent on estuarine habitat during a major portion of the non-marine phase of its life cycle.

The method intended to act as the "distinct cumulative effects assessment" thus is designed to assess the effects of only a subset of the actions carried out by the land-owner, and the effects on only a small subset of the already-identified impacts of concern will be assessed. Furthermore, those impacts that are considered "eligible" for assessment are not identified, and no methods are described

for their assessment. The proposed procedure does not qualify as a cumulative effects assessment method.

Adequacy of the method for providing information required by the HCP/SYP

The HCP/SYP specifies that results of the watershed assessment are to be used to modify the default prescriptions of the HCP/SYP to suit site-specific conditions. Results of the watershed assessment may provide the basis to "modify the following elements of the Aquatics Conservation Plan: hillslope management prescriptions; channel migration zone prescriptions; Class I, Class II, and Class III prescriptions; the disturbance index, and monitoring" (USFWS and CDF 1999a, p. P-38, Item 2). Prescriptions for modification of forest practices clearly cannot be designed without an adequate analysis of the cumulative impacts of land-use activities in the watersheds. For example, if buffer strip prescriptions were to be rewritten on the basis of an analysis of an arbitrary subset of potential cumulative impacts, designs would be invalid; there would be no basis for determining whether the new prescriptions were in compliance with requirements of the Forest Practices Act or with the HCP/SYP, which requires that all activities under the HCP/SYP be in compliance with water quality laws and meet the objectives of the Basin Plan.

Of primary concern to the review, then, is whether the proposed method for cumulative impact assessment provides enough information to permit the valid design of forest practice prescriptions that would replace those adopted as defaults in the HCP/SYP. If the method does not provide sufficient information concerning the relationship between land-use activities in each watershed and cumulative impacts on the beneficial uses of water and on species of concern, then the proposed method is not adequate as a basis for rejecting the forest practice prescriptions detailed in the HCP/SYP and required by California State Legislature Assembly Bill 1986, and replacing them with new prescriptions.

Restriction of the "distinct cumulative effects assessment" to being a modified synthesis step of the standard WDNR modules makes it impossible for the assessment to provide the information needed to alter existing forest practice prescriptions, since there will not be sufficient information concerning cumulative impacts on the beneficial uses listed in Table 1 to evaluate the cumulative impacts of the new prescriptions. Such prescriptions cannot be validly planned in the absence of a full cumulative impact assessment. An assessment that considers only a subset of the effects that are required to be assessed by state and federal laws, and considers impacts only in a portion of the area in which cumulative impacts are likely to be expressed, and considers the impacts only of a subset of past, present, and reasonably foreseeable future forest management activities, does not provide a valid basis for developing new forest practice prescriptions.

Furthermore, the HCP/SYP itself requires that any activities carried out under the HCP/SYP—including prescriptions developed under the HCP/SYP—must comply with the Basin Plan objectives developed pursuant to the Porter-Cologne Act. If forest practice prescriptions are to be altered on the basis of information provided by watershed assessment, then that information would need to be sufficient to ensure that the requirements of the HCP/SYP are met: enough information would be needed to determine whether the prescriptions meet the Basin Plan objectives. As currently formulated, the method cannot do so because the method does not provide the necessary information concerning impacts to the beneficial uses of water specified by the Basin Plan.

Note: Review comments are organized by section of the document. Specific comments under each section are noted by page/paragraph/line.

Section: "Note to Reviewers" and "Introduction"

General comments:

The document's "note to reviewers" provides a series of reasons that the quality and scope of the analysis will be limited, and these reasons are repeated in the "introduction." Examination of the HCP/SYP and the WDNR manual, however, suggests that the reasoning is based on a misinterpretation of the source documents. The introduction also provides reviewers with an inadequate period for review, and blames the need for this "aggressive" time schedule in part on the public's participation in the process. However, it is not clear why delays in the development schedule would be compensated for by provision of an inadequate period for review. Further, release of the document was planned for 20 August, several weeks after public participation had ended. The release was incrementally postponed for nearly a month beyond this date, during a period when there was no public participation. This delay thus was not substantially influenced by public participation. It seems unreasonable to single out public participation as an important cause for delay.

The "Note to reviewers" and "Introduction" indicate that only a limited analysis is possible using the WDNR method, and that analyses on PALCO lands will be similarly constrained. However, the standard WDNR method makes it clear that the standard method requires that additional work be done if necessary to answer the questions posed: "Recognition of when, where, and how to undertake more detailed analyses necessary to adequately understand watershed processes is a crucial component of Watershed analysis that must not be constrained prior to conducting the standard analysis" (WFPB 1997 p.E-75). This passage is reproduced in the PALCO channel module at 62/1/6.

Specific comments, "Note to reviewers":

i/2/2: The HCP also requires that a "distinct cumulative effects assessment" method be developed (USFWS and CDF 1999a, p. P-37, section 6.3.2.1 item 7). There thus is no requirement in the HCP that the cumulative impact analysis procedure be limited by constraints on the WDNR process.

i/3/4: The calculation that each watershed analysis must be completed in 2.7 months is incorrect. The HCP nowhere requires that only one watershed analysis team be active. Further, the WDNR process incorporates an expected duration of 150 days (including comment period and revision) for Level II analysis (WFPB 1997). An equivalent of Level II analysis is required for the watershed assessment (USFWS and CDF 1999a, p. P-37, Item 10).

ii/2/4: The text here states that the "WDNR synthesis has been extensively modified to become a distinct cumulative effects assessment, drawing upon all elements of the watershed analysis." Part of this "extensive modification" appears to have been a shortening of the description of the "synthesis" module from the 38 pages in the WDNR manual (WFPB 1997) to the 14 pages present here as a "cumulative effects" module. There is no description of the cumulative impacts to be evaluated or the methods to be used to evaluate them. Instead, the text simply assures that cumulative impacts will be "addressed" if required. This is a statement of intent, not a cumulative effects assessment method. The document under review was intended to present the proposed methods for evaluating cumulative

effects, yet not only are we not told how those cumulative effects will be assessed, we are not even told which cumulative effects will be evaluated.

Specific comments, "Introduction":

2/4/4: "Synthesis does not include many elements related to assessment of CEs": Although this statement is valid, I suspect that it was not the statement intended.

2/4/15: The cumulative impact assessment process described here is not a "distinct cumulative effects assessment," as required by the HCP, but instead is a directive for module teams to communicate with one another while carrying out a standard WDNR procedure.

3/7/3: It should be noted that this "start-up" step has not yet been completed for the Freshwater watershed assessment. There still is no information available about "which cumulative effects issues are appropriate to cover in the watershed analysis and the scope of analysis and methods for each issue."

3/8/3: "Problem statement" is ordinarily understood to mean a description of what questions will be answered during a study. In this case, development of the problem statement appears to be an outcome of the study. Is "problem statement" being used in a different sense here?

Figure 1: There is no "distinct cumulative impacts assessment" shown in this depiction of the standard WDNR process. Instead, the extent of the cumulative impact analysis appears to be 1) "Watershed CE issues compiled," 2) "revisit issues list to ensure identified CE's have been addressed," and 3) "Cumulative effects report."

Figure 1: The direction of arrows or the nature of connections with respect to "Public Meeting..." and "Reports provided..." do not appear to be correct, and the interaction with the Prescription Team does not appear to be depicted consistently. There is also no provision for revising the report to incorporate review comments.

5/1/4: The cumulative impact assessment method is described as follows: "Cumulative watershed effects are identified during synthesis by: 1) revisiting the initial list of watershed issues of concern; 2) listing and ranking factors contributing to verified resource conditions of concern; and 3) explicitly addressing uncertainties in the analysis." This procedure does not constitute a cumulative effects assessment. The description provided here is not functionally different from that of the standard WDNR synthesis step.

5/3/5: The requirement that module team members must have formal education in the "relevant" discipline—later specified to include only the explicit subject area of the module—makes it impossible for the modules to adequately address cumulative effects, which are inherently interdisciplinary.

6/6/4: Note that the HCP specifies that "Members of the public who have been technically trained may also participate in the technical analysis" (USFWS and CDF 1999a, p. P-37, item 15). The HCP thus does not restrict public participation on a module analysis team on the basis of the level of technical training or the disciplines covered by that training. It thus appears that the only mechanism for providing interdisciplinary representation on the analysis teams—as would be required for a valid cumulative effects assessment—is through inclusion of public and agency members on those teams.

Section: "Mass Wasting Assessment"

General comments:

Many of the problems identified in this section may have resulted from unclear writing. It would be very useful for the authors to work with an editor to address grammatical problems and remove ambiguities. Note, also, that "mass wasting" differs from "landsliding" primarily because "mass wasting" includes soil creep. Since this module does not include soil creep while the "surface erosion" module does, it would be useful to alter the title to better reflect the content of the analysis: landsliding.

Results of the landslide assessment are expected to be used to: 1) evaluate the effectiveness of different widths of buffer strips for reducing landslide frequencies and delivery; 2) evaluate the importance of Class III protection for reducing landslide frequencies and severity; 3) evaluate appropriate rates of logging for reducing sediment inputs to acceptable levels through definition of a valid disturbance index; 4) refine the mass-wasting avoidance strategy; and 5) assess cumulative impacts. A completed analysis will be useful if it identifies the natural background rate of landsliding, quantifies the influences of specific land-use practices on those landsliding rates, describes how those rates are affected by different amounts of vegetation cover, and determines the rate of recovery after land-use activities are completed. Results would need to be distinguished for specific kinds of landforms on each major kind of bedrock in the area. To the extent that this information is not provided, the analysis is incomplete. Each of these tasks is provided for in Level II WDNR analysis.

As presented, the method does not appear to be capable of answering the primary questions that need to be answered to implement the HCP. The "Level III" analysis is not capable of evaluating probability of failure because it does not account for covariance between variables, and no explanation is provided of how the "Level I" and "Level III" analyses will be combined. There does not appear to be a provision for comparing rates of landsliding under different silvicultural practices, or for quantifying the naturally occurring background rate of landsliding.

Specific comments:

3/3/1: "Physics" is not a synonym for "rheology."

3/4/4: How do "deep-seated landslides" (section 1.2.3) differ from "debris slides" (section 1.2.1) and "small-sporadic deep-seated landslides" (section 1.2.4)? How is this distinction reconciled with the statement that "deep-seated landslides" can include "debris flows" and "debris avalanches"? Categories that are not mutually exclusive do not provide an adequate basis for classification.

3/4/5: If the discussion is to rely on results presented in the Czechoslovakian and Swiss literature, it would be useful to provide the correct and complete citations for that literature. Citations are not complete for Heim and Buss (1881), Stejskal (1939), and Zaruba (1929). However, it is not evident that these citations to relatively inaccessible literature actually add to the discussion.

4/1/1: The fact that 59% of deep-seated landslides in the Czech Republic are in agricultural land and 23% are in forests is irrelevant to the discussion, since there is no indication that conditions in the Czech Republic are similar to those on PALCO land, and there is no indication of the relative proportions of the Czech Republic in agriculture and in forest.

4/1/5: Relevant work actually has been published, and it would be very useful to cite and discuss the published work. An unpublished dissertation proposal is not a valid citation.

4/2/1: There is no definition provided for “small-sporadic deep-seated landslides.” Further, it is not reasonable that a 40-acre landslide would be considered “small.”

4/2/3: Is “...they reactivate...” intended, rather than “...they occur...”?

4/2/5: “Jargon” is inappropriately used, and “subdued topography” is not used for describing “this decay.” It is not clear what a “decay in movement” refers to—a deceleration, perhaps? But reactivation “at irregular time intervals” would not necessarily result in an overall deceleration. Also, “subdued topography” ordinarily refers to areas with low relief rather than to areas experiencing deep-seated landslides. Could “hummocky topography” have been meant, instead? However, “hummocky topography” is also associated with chronically active landslides.

4/3/4: A citation should be provided for the example of landslide reactivation during urban conversion. If, on the other hand, this is simply a hypothetical example, it does not appear to be particularly relevant to the problem at hand. More relevant would be a discussion of how the logging might influence the activity of deep-seated slides.

5/2/3: A citation is needed for these examples. To make this paragraph relevant, it would be useful to explain how this information would be used to evaluate the influence of land-use activities on landsliding rates.

5/4/4: It is not clear what is meant by “this ‘overprint’” If it means what I suspect it means, then this sentence displays a circular argument: 1) it is necessary to understand how land use affects landslides, because 2) this information is necessary for understanding how landslides are affected by land use.

5/4/7: Is “known resource conditions” being used to mean “impact”?

6/2/1: These requirements for educational level conflict with those specified in the HCP (see comment 6/6/4 of the Introduction). Further, restricting participation on the module to those disciplines listed here prevents adequate evaluation of cumulative impacts.

8/1ff: It might be more useful to focus on the questions that relate directly to the goals of the analysis rather than on questions that are relevant only because they might need to be answered en route to answering the important questions. Unless the most important questions are high-lighted, it is easy to miss the overall goal by focusing instead on preliminary questions. The list of 16 “critical questions” would be more useful if it were distilled into the following:

1. What is the naturally occurring background rate of sediment production from landsliding?

2. How much do particular forest-management activities and practices alter the naturally occurring rates of sediment production from landsliding?

3. How much do anthropogenically altered landsliding rates, in combination with other anthropogenic impacts, affect the beneficial uses of water?

The original list of questions might be more usefully distributed through the description of the analysis methods to show the utility of those questions for guiding the course of the analysis. Alternatively, the original questions—plus a few more that would provide guidance for dealing with storm sizes—could be incorporated as sub-topics arrayed under the critical questions, again to illustrate the intermediate steps of analysis.

10/1/4: It is not appropriate that the underlying assumptions are not expected to change as new information becomes available. Assumptions *must* be continually subject to testing and refinement.

10/2: Landscape evolution does not result solely from geologic events. Geomorphic, hydrologic, meteorologic, climatologic, and ecologic events in some cases play even greater roles in shaping landforms than do geologic events. It is not clear why this assumption is needed. If there is a reason for it, then it should be rephrased to make that reason clear.

10/3: Aerial photographs cannot be used to "calibrate mass-movement associated with...landform evolution," since much of the evolution of a landscape occurred under conditions that predate the onset of aerial photography by thousands to millions of years. Aerial photographs instead are used to evaluate rates of landsliding over the duration of the photographic record.

10/4/1: The "identification of existing mass-movement features" cannot "be used to predict the likelihood of future instability." Such prediction requires a level of understanding contingent on more than "identification" of existing features. Information is also needed concerning their distribution in space and time and their association with storms and land-use activities.

12/1/8: The suggestion that the method might be modified for application to "all California watersheds" is inappropriate.

12/4/1: This list of landslide types does not include some of the types listed earlier and later in the chapter.

14/1/2: It is not valid to *assume* that the activities have been carried out before within the watershed. Instead, it will be necessary to measure the effects of those activities elsewhere if they are not present within the watershed. At least one watershed analysis in Washington State proceeded with the standard analysis even when the assumption was demonstrably false: little of the watershed had been entered previously, making it impossible to determine the effects of logging on soil and slope stability using the standard method. Because it had not yet been destabilized, much of the unentered watershed was deemed stable. Had the false assumption *not* guided the analysis, the analysts would have been required to look at the outcome of logging in adjacent watersheds, where destabilization of similar soils had already occurred. It should also be noted that the keys to interpreting outcome are (1) the occurrence of a landslide-generating storm after the practices in question have been carried out and (2) the existence of both unentered (or recovered) and recently-entered areas at the time of the storm.

14/2: This paragraph does not adequately explain the basis for definition of mass-wasting units. Information within the paragraph is internally inconsistent. The units are first said to be based on landform or geologic features, but later the paragraph states that areas with road-related failures are to be separated from those with hillslope failures, implying that road corridors will be distinguished from logged hillslopes.

14/4/1: Was this not also the case for the "qualitative" approach? Why would the same classifications not be used?

14/4/6: This may not be a good example, as colluvial-filled bedrock hollows cannot be distinguished from planar slopes on topographic maps or aerial photographs. Or does this just imply that landforms will be grouped together if they cannot be distinguished from one another? In any case, it will be necessary to provide some indication of how a grouping is deemed to "make sense."

14/5/3: Moisture content varies with season and API. How will information on moisture content be used?

15/1/3: The method described by Koler (1998) does not actually result in a calculation of failure probability because the variables for which frequency distributions are measured or guessed are not independent of one another and because insufficient information is available for defining the magnitude, distribution, and frequency of storm-related pore-pressure fluctuations. Instead, the method provides a description of the distribution of calculated potential outcomes given random selections of input variables from measured or guessed distributions. (In the case of guessed parameters, this results in a form of sensitivity analysis.) For colluvial-filled bedrock hollows, for example, soil moisture is high, soils are deep, and root cohesion is low. The distribution of characteristics for the combined planar slope/colluvial-filled bedrock hollow unit, however, would treat these variables as independent, resulting in an underestimate of landsliding potential within the combined unit. There is no documentation provided to indicate that the model's predictions have been tested; the accessible documentation instead simply demonstrates how to use the model. It is not appropriate to rely on an untested model for an application such as this when other established methods are available, such as those recommended for use in Level II slope stability analysis in the standard WDNR approach.

15/3: Either there is no indication that the model outcomes will be tested or the writing style of this paragraph has obscured a statement of such an intention.

15/3/8: "For the quantitative approach, these associations form the basis for the mapping of mass-wasting map units in the watershed which will be the final product." I'm not sure what is meant here. Aren't the MWMUs already mapped? How will the comparison of two sets of polygons result in a third set of polygons? What is "confirmation in the accuracy of the two approaches"? If one approach is being used to calibrate the other, it cannot be used to confirm the accuracy of the other. Is there any intention to test either of the methods against measurements of landslide frequencies after major storms?

15/5/2: The "probability analysis" does not produce a probability of failure. Instead, it describes the proportion of combinations of conditions that would produce a factor of safety less than 1 if those parameter values are randomly and independently selected. Note that slopes generally are considered unstable if they have factors of safety of less than 1.3, rather than the value of 1 employed here. Note that no description is provided of what the "probability of failure" actually describes. Landslides per square km per year? Likelihood that there will be a failure within the map unit in a year? Percent of sites within the map unit likely to fail in a year?

16/4/1: The extent of the description of cumulative impact evaluation in the slope stability module appears to be a simple assertion that issues will be assessed, and that the "methods will be dependent on the issues and questions." An assertion that cumulative impact issues will be assessed does not constitute a "distinct cumulative effects assessment," as required by the HCP. Further, the section refers to section 6.0 for suggested methods for such analyses. These are, for the most part, the suggested methods for Level II analysis presented by the WDNR manual. The WDNR method was found inadequate for cumulative impact analysis by the HCP; hence the requirement for the "distinct cumulative effects assessment." Providing material from the WDNR manual as the intended means for evaluating slope-stability-related cumulative impacts thus is inappropriate and inadequate.

20/1/1: There is no "Historic Condition assessment" described in this document. What is being referred to here?

20/6/3: Considerable photographic documentation of erosion process type and distribution is reported to have been collected by the plaintiffs during preparation for the so-called “coho suit.” This information, which PL apparently has access to, will be very useful for the successful completion of the watershed assessments.

21/6/2: “Most efforts are obvious, but things like gullying or skid trails that turn grassy swales into defined channels may require coordination.” This sentence does not adequately convey the intended meaning.

21/6/3: No methods are described for how erosion rates will be assessed. Instead, the “method” states that “methods selected to estimate the input of sediment from each source will largely be a function of available information in combination with those methods specified by the assessment modules.” This does not constitute a description of a method, but is instead a statement of intent. Until we know how the sediment budget will be constructed, we have no basis for determining whether the intended procedure is likely to be useful.

21/7/3: “Some of these items suggest preliminary synthesis discussions.” What is this statement intended to convey?

22/point 4 under surface erosion analyst: “discuss relative importance” – calling for a discussion is not meaningful unless something substantive is intended to follow from the discussion. Will decisions be made during the required discussion? Are conclusions to be drawn? What evidence, if any, will the required discussion be based upon? Discussions are called for elsewhere in the list on page 22 with similar problems.

22/last paragraph/2: “the potential delivery of mass-wasting hazards” Is “mass-wasting hazard” here being used to mean “landslide debris”?

23/1/2: “Analysts may rely solely on the methods provided here.” This statement could be taken to imply that analysts cannot be required to do more than is presented in this module. Under Level II WDNR analysis, however, analysts are expected to carry out whatever feasible analysis is appropriate to answer the critical questions in the watershed being evaluated.

23/1/3: Under what conditions would it be considered “necessary” to carry out a more detailed analysis? Since the statement at 23/1/2 seems to indicate that such a decision is voluntarily, it is important to clarify what the obligations of the analysts are.

23/4 ff.: Reorganization of the text would allow much of the redundant text to be removed.

23/last paragraph: Photos taken soon after logging and major storms are not merely “helpful”: the analysis cannot be properly carried out if such photos are not procured, unless similar information can be acquired from fieldwork.

25/Process list: LPD and SSD landslide types have not been adequately defined in the text.

25/second to last paragraph: The code “SR” has not been defined.

26/2/2: How will radial distortion be corrected for when the landslide size is determined by “measuring directly off the photograph”?

26/2/12: Earthflow erosion cannot be validly estimated from “literature values for average rates of movement...” without some indication that those literature values are relevant for the area.

26/second to last paragraph: Active earthflows maintain a relatively continuous ground-cover vegetation, so presence of ground-cover vegetation is not a useful diagnostic in this case.

27/1/1: There is no valid method presented for determining whether a landslide “developed under a different geologic or climatic condition.” This category cannot be distinguished from the category of “dormant,” as described here. It does not necessarily follow that a 1500-year-old slide developed “under different geologic or climatic conditions,” so it would be necessary to know what caused the slide 1500 years ago before one could classify a 1500-yr-old slide as “dormant” or “relict.”

27/activity criteria for field-verified slides: A slide that has not moved within one year is not necessarily “dormant”; this classification is not consistent with that presented for landslides identified on air photos (p.26). Further, categories I and D cannot be distinguished according to the definition provided here: “the causes of movement remain apparent” on dormant slides. Presumably those causes of movement also remain apparent on the more recently active “inactive” slides. It would be far more useful to evaluate field evidence to estimate the slide’s age rather than simply classifying it qualitatively.

27/sediment delivery categories: It would be much more useful to evaluate field evidence to estimate the volume of sediment delivered rather than relying on a qualitative classification. It should be noted that delivery to a floodplain or channel migration zone counts as delivery to a stream, since the debris will have retroactively “entered” the channel when the floodplain is inundated or the channel migrates through its customary migration zone. Delayed entrainment adds a lag to the recovery period after the failure, prolonging the duration of any downstream cumulative impact.

27/last paragraph: It is also important to note the location and kind of land-use activities taking place upslope of the slide.

29/2/7: Because different kinds of information are gathered for these different efforts, this step is not primarily “an exercise to determine the reliability of existing data...”

29/3/3: “...to evaluate whatever can’t be seen...” is not sufficiently explicit

30/3/1: A standard geomorphological approach would be to carry out the previously described Level I analysis using measurements rather than categories; this approach is presented as an option for Level II analysis by the standard WDNR manual.

30/3/3: Here, too, it is not appropriate to suggest that a method that has been presented as being severely constrained by requirements of an individual company’s HCP (see introduction) is expected to be applied to other timberlands in California.

30/4/6: How would Tom Spittler have drawn conclusions in 1998 concerning a plan that was substantially revised in 1999?

31/1/2: The WDNR Level II analysis is here dismissed as being “a good screening tool but is not adequate for site-specific geological/geotechnical projects.” In its place, a new, locally untested method is proposed to be “used as a planning tool for identifying the need for additional geological/geotechnical investigations.” Essentially, this argument states that the DNR method is not adequate because it is simply “a good screening tool,” so a new method is being developed to use as a screening tool. This argument is not logical.

31/3/1: “The steps in the qualitative method...”: There appears to be an important typo here.

31/4/1: Where the source documents for an analysis protocol are not generally accessible, it is considered appropriate to provide some description of what the protocol is rather than just referencing their existence. It is not possible to assess the utility of inaccessible protocols.

31/5/2: What level of precision is considered to be required? In general, topographic maps are not sufficiently precise for mapping of inner gorges; aerial photographs must be used in combination with field verification.

31/1/1: Presumably the stratification is based on landform? What other stratification parameters will be used? Geology? More detail is needed here.

32/1/3: Collected soil samples cannot be used to determine soil depth.

32/1/3: Note that soil moisture is highly dependent on when the sample is collected. How is this information intended to be used?

32/5/1: The example described by Koler (1998) goes through these steps, discovers that the existing soil depths are inexplicably deep, and ignores the analysis. The operational message is that the observed soil depths are used to back-calculate ground water depths unless the result is not the one the analyst expects. The fact that the only readily accessible example (Koler 1998) illustrates a case where the method doesn't work provides the reader with little confidence in the procedure's validity.

32/5/1: This method appears to be based on the assumption that soil depth is controlled by landsliding, so that solving for the groundwater ratio is possible by assuming that the observed soil depth is marginally stable (i.e. has a factor of safety of 1.0). This is not a valid assumption. A variety of other processes also influence soil depth. Further, engineering geology texts make it very clear that factors of safety of up to 1.3 are also to be considered marginally stable. Failure can occur at calculated factors of safety of much greater than 1.0 due, in part, to the fact that the available theory does not fully capture the richness of reality.

33/3/3: Note that post-logging hydrologic change due to altered evapotranspiration and foliage interception of rainfall would need to be evaluated, also.

33/4/1: This procedure cannot be used to "find the probability of failure for each polygon" since the covariance between variables is not evaluated. Further, there is no valid approach presented for determining the probability distribution for groundwater depths through time, as would be needed to calculate "probability of failure." For example, if the same area is susceptible to failure, but a change in groundwater conditions increases the frequency of failure at those sites, it would be necessary to evaluate the change in failure *rates* (i.e., per unit time) if a "probability" is to be meaningful.

33/4/4: This reference is not readily accessible (also note error in citation). Because of the importance of rotational failures and earthflows and the apparent intent to apply the proposed approach to all landslides, it will be necessary to provide a description of how this application will be made. It is not possible to evaluate the appropriateness of methods that are not provided. It is also highly unlikely that a method based on the infinite slope model can be validly applied to rotational failures and earthflows.

33/5/1: It is not clear why "calculated probabilities" are compared to "known landslide areas." Probability is not usually expressed in terms of area. It is also not clear what "probability" is intended to refer to in this context: the probability that a slide will occur within a given polygon in a certain length of time? The probability that a given site within a polygon will fail within a certain period?

The results do not appear to produce a probability, but an estimate of the ratio between 1) combinations of input variables that would produce a factor of safety less than one and 2) the total possible combinations of input variables. The procedure described by Koler (1998) assumes that input variables are independent of one another within a landform type. If variables co-vary, as actually expected, random sampling of input variables would not provide a valid estimate of failure "probability." For example, if 1 out of 8 cats is calico and 1 out of 2 cats is male, it does not follow that 1 out of 16 cats is a calico male. Calico coloration is a gender-linked gene in cats, and only females are calico.

33/5/2: What is meant by a probability "threshold"? If the calculated "probability" is indeed a probability, no "threshold" need be defined to interpret the probability. The point of the mass wasting module is to compare background landsliding rates to those expected after land-use activities have taken place. No "threshold" is needed to do so.

33/5/5: Miller et al. 1987 is not a readily accessible reference; this protocol will need to be described if it is to be used.

33/6/1: MWMU's were already defined using the first kind of analysis described by this module. Is the intent here to redefine those units?

34/6/1: If "distributions and types of existing landslides by terrain are important in designating the MWMUs," it will be very important to account for land-use-related differences in sliding rates. For example, if inner-gorge slopes low in a basin have many landslides while similar slopes in the upstream reaches do not, it will be necessary to determine whether such a difference results from the land-use history rather than the inherent susceptibility of the sites. If all the lower-basin inner gorges have been logged while the upper basin has not yet been reentered, it would be necessary to assume that the upper basin slopes will respond in much the same way that the lower basin slopes have.

35/1/1: Is "features" being used to mean "landslides"?

35/2: There will need to be a procedure for testing the validity of the results before extrapolating them.

36/Landform: "...the active stream channel (that portion inundated during high flows) with little or no intervening low-gradient flood plain...": Note that "that portion inundated during high flows" *includes* the "intervening low-gradient flood plain."

37/Calculated failure probability: This has been left blank.

37/Non-forest practice related landslide density: This is important information for answering the most critical questions. It cannot be "optional" if the central questions of the analysis are to be addressed.

38/Comments: More extensive soil saturation also leads to a greater likelihood of causing, accelerating, or reactivating deep-seated slides.

39/Table: Such a summary table is not useful unless a measure of the area in each activity category is provided. For example, information that 15% of the landslides in a watershed occurred in undisturbed forest and 85% on recently logged areas *cannot* be interpreted until one learns that only 37% of the watershed was recently logged.

40/Factors considered: There is no indication of how these factors are to be “considered.” The word “considered” has been very loosely interpreted in a variety of policy documents of the past, so more explicit descriptions of what is intended would be very useful here.

41/2/4: It is not appropriate to judge landslide hazard “relative to the rest of the basin” or relative to the rest of the property. Using such an approach, a property on the Modoc Plateau would have the same range of landslide hazard ratings as a property in the King Range. In addition, it is very important to distinguish between the naturally occurring rate of landsliding and the potential for land use activities to alter that rate: sensitivity to impacts would need to be judged on the basis of the latter. An area with an inherently low landsliding rate may be more sensitive to land-use activities than one with a naturally high landsliding rate *if* land-use activities increase landslide rates by a higher percentage in the area with inherently low rates. The reason for this apparent paradox is that the natural form of a stream channel is adjusted to convey the distribution of sediment loads that the stream is subject to under natural conditions. If the characteristic sediment input increases *relative to natural input rates*, the form of the stream channel will readjust to compensate for the new sediment input regime. A doubling of sediment input rate can be as severe an impact in an area with a low natural landslide frequency as it is in an area with a high natural landslide frequency, though the increase in sediment yield would be lower.

41/4/4: How was the probability of failure calculated? It does not appear to make sense. Ordinarily, a probability of failure would be described as a number of failures per unit time per unit area. Does the 1% probability referred to here mean that 1% of the area is likely to fail each year? If so, this is an extraordinarily high probability of failure. Or does it mean that in one out of one hundred years a landslide will occur on each potential landslide site in this unit? This would also be an extraordinarily high probability of failure. Is the probability calculated for natural conditions? If so, what is the probability under various land-use regimes? The *change* in susceptibility—and therefore the potential impacts of land-use activities—is the issue in question.

42/3/3: It would take more than a year of inactivity if there is to be a useful distinction between categories of “inactive” and “dormant.” Further, an active earthflow may not show signs of activity in the summertime or during a dry winter. It is not useful to distinguish between those slides that are currently moving and those that are expected to resume movement under conditions which are known to recur frequently. Is a debris avalanche scar “active” two years after failure because its margins continue to slough?

43/5/1: The distinction between “high” and “moderate” is not valid, since “delivery is certain” also for the “moderate” category.

44/Table 2: It is not reasonable that an area with moderate mass wasting potential and high delivery potential would score as “moderate” in the reclassification. The description of “moderate” mass-wasting potential indicates that there is up to a 3% probability of failure (compared to 4-10% for “high”), indicating that landslides will occur regularly, but at a lower rate. Classification in the “high” delivery class suggests that once the slides occur, much of the sediment will hit the streams. In contrast, areas with “high” mass-wasting potential and “moderate” delivery potential (which can include delivery by overland flow alone) are appropriately reclassified as “high.” Given the descriptions in the manual, the “high landsliding, moderate delivery” (reclassified as “high”) actually has a lower potential impact than those with “moderate landsliding, high delivery” (reclassified as “moderate”). Consider this case:

moderate slides = 2%	high delivery = 75%	total delivery = 0.02 x 0.75 = 0.015
high slides = 8%	moderate delivery = 10%	total delivery = 0.08 x 0.10 = 0.008

yet the first would be reclassified as “moderate” and the second as “high”!

45/6/8: There needs to be more than simply “identifying which landforms or mass wasting map units have contributed LWD through mass wasting processes.” The analyst would need to know something about what sizes of LWD are likely to be contributed from what distances under disturbed and undisturbed conditions, and in what quantities. This is not a difficult thing to do.

46/1/3: Either LWD is an issue in all of the watersheds, or it is assumed that the maximum buffer widths will be applied. This is a central issue for the entire watershed assessment procedure, and the analysis cannot simply be presented as a list of things that the analyst “may” do. We need to be able to evaluate whether the specific methods that will be applied to this critical analysis are likely to be effective. Reviewers cannot evaluate this if we are not provided information about what *will* be done to analyze the issue.

46/3/6: Are these intended to be examples of what *will* be done? or examples of what *could* be done? If these are to be part of the analysis, *how* will they be done?

47/1/1: The introduction to this manual states that construction of sediment budgets is a necessary part of the analysis. Now we learn that erosion rates are to be evaluated (i.e., a sediment budget constructed) only in certain cases (such as “if stream enhancement is contemplated”). This is not consistent. The document would be more useful if it provided a summary of what work *will* be done and a separate list of what work *might* be done.

47/4/4: Note that the silt/clay and sand fractions are also the dominant components in aggraded banks of Freshwater Creek and Elk River, and that sand-silt aggradation is also very important within these channels.

48/2/2: “Mass wasting” is distinguished from “landsliding” primarily because “mass wasting” includes soil creep while “landsliding” does not. However, soil creep is not evaluated by the mass wasting module. Where will the information on soil-creep inputs come from? If it is not to be evaluated by this module, the module name should be changed to “landsliding” to reflect the actual content of the module. Or is “mass wasting” intended to simply mean “landsliding”?

48/4/1: “During the synthesis process, all the analysts will discuss the sediment sources...” “Discussion,” per se, is not an analysis protocol. There needs to be some indication of what will be done to analyze the impacts.

48/4/3: How is the “regulatory framework for sediment inputs” intended to be “addressed” by the group? The “regulatory framework for sediment inputs” includes the HCP/SYP directive that any activities carried out under the HCP/SYP must comply with the Basin Plan objectives developed pursuant to the Porter-Cologne Act. If forest practice prescriptions are to be altered on the basis of information provided by watershed assessment, then that information would need to be sufficient to ensure that the requirements of the HCP/SYP are met. Enough information would be needed to determine whether the prescriptions meet the Basin Plan objectives. As currently formulated, the method cannot do so because the method does not provide the necessary information concerning impacts to the beneficial uses of water specified by the Basin Plan.

48/5/1: A list of things to "consider" in evaluating confidence level of results is not sufficient. How will this information be used? What is a high confidence level? A low confidence level?

50/2: "...analyze mass wasting occurrence for trends associated with land uses, geology, and spatial and temporal patterns of mass wasting": This is *not* an optional addendum to the analysis. This is the major purpose of the analysis. If the analysis does not result in such information, it has been insufficient.

51/2: "Identify the mass-wasting potential from the existing forest roads": If this task has not been accomplished by the analysis, the analysis is not sufficient.

51/3: "Obtain a better understanding of deep-seated failures": If this task has not been accomplished by the analysis, the analysis is not sufficient.

55ff: Many of the bibliographic citations are incomplete.

61/2/1: "Bates and Jackson 1984" is not included in the bibliography.

61/4/4: A 100-year-old slide is not considered "ancient" in this area, as some earthflows and deep-seated failures likely to be of that age remain active. "Ancient" is not used as a category elsewhere in this module.

61/5: "Colluvium" is also used by geomorphologists to refer to soil displaced by creep or other transport mechanisms.

61/deep-seated landslide: Not an appropriate definition (all slides have a portion of their failure surface that intersects the ground surface, and shallow failures generally involve several soil layers).

62/gully: Not a valid definition

62/headscarp: "headscarp" is not an abbreviation of "escarpment"

62/inactive: This definition conflicts severely with that provided in the text.

62/landslide: This is not a useful definition in this context. How does it differ from "mass wasting"?

62/mass wasting: Is there any movement of soil and rock material on the earth's surface that is *not* influenced by gravity? This is not a useful definition.

62/ "morphology/geomorphology": these words are *not* synonyms, as implied here.

63/piezometer: not a valid definition

63/sheetwash, sheet erosion: sheetwash is not sheet erosion

The following passages have grammatical problems that in some cases are severe enough to affect the intended meaning: 5/4/2, 5/4/last 2 sentences, 10/3/last two sentences, 12/1, 14/3/1, 14/5/1, 15/3/6, 23/1/6-9, 27/2/1, 29/2/second sentence, 32/3/3, 51/1/2, 61/6/4.

In many cases, modifiers added to phrases detract from the clarity of the idea rather than contributing to it. In particular, the following might be usefully edited:

5/1/3: What is meant by "dynamic replenishment"? How does this differ from "replenishment"?

5/2/1: What is meant by "landscape evolution continuum"? How does this differ from "landscape evolution"?

5/2/3: "...co-seismic uplift associated with seismic activities..." Is this intended to distinguish this kind of uplift from co-seismic uplift *not* associated with seismic activities?

5/3/3: What is meant here by “cumulative probability”? How does this differ from “probability”?

15/1/1: “...will be built through a deterministic evaluation of samples...”: Do this and the following sentence mean that the measured frequency distributions for soil properties will be used to estimate the average failure condition for each polygon? If so, this is not clear.

28/“Soil type”: “general soil origin type” is not clear

28/ “Form” under “geomorphic characteristics”: Is “flow topography” intended to mean “topography characteristic of earthflow activity”?

31/3/3: “stochastic probabilistic analysis” is redundant.

31/4/6: “topographic geometry” is redundant

31/4/8: “formers” is not a useful description; it would be useful to explain what is meant.

42/3/11:“...that fail into debris flows/torrents” is ambiguous.

43/1/3: “landslide migration moving upslope” does not appear to mean what is intended

43/4/1: “...delivery is moderated by process or proximity to resources” is ambiguous.

50/3/7: What is meant by a “bust” in the data? Is this GIS jargon?

Section: "Surface erosion"

General comments.

The surface erosion module is more clearly written than the landsliding module. The major points of the analysis are clearly considered, and attention is given to testing the models that are proposed for application in the area. Many of the points noted below as "specific comments" may simply reflect the authors' current lack of familiarity with this particular region. Fieldwork is likely to allow refinement of some of the assumptions that are not appropriate for these sites.

Results of the surface erosion module would be expected to contribute to evaluation of effectiveness of different widths of buffer strips for reducing sediment inputs to streams, evaluation of the importance of Class III protection for reducing sediment inputs to streams, evaluation of appropriate rates of logging for reducing sediment inputs to acceptable levels through definition of a valid disturbance index, and provision of information necessary for assessment of cumulative impacts. A completed analysis will be useful if it defines the natural background rate of surface erosion, the influences of specific land-use practices are on those rates, how those rates are affected by different amounts of vegetation cover, and what the rate of recovery is after land-use activities are completed. Results would need to be distinguished for specific landform types on each major geologic type in the area. Of particular importance is definition of the role of road networks and road use in contributing fine sediment to streams.

As currently written, the module is not completely adequate for addressing the necessary questions, but the changes required to make the module effective are not major. Such changes would include:

1. inclusion of more information concerning the strategy to be used for selecting field observation sites and for the kinds of observations to be made in the field
2. consideration of tractor skid roads as roads rather than as a part of a homogeneous cut unit
3. field observations to be made during moderate and large winter storms
4. collection of sediment samples to allow testing and calibration of the models proposed for use; the current plan to test one model against the other would not be appropriate in this area.

Specific comments:

2/1/2: Technically, gravity is not energy

2/1/6: It would be useful to specify that "fine particles" in this case can include pebbles.

2/4/1: Include traffic with "road construction and maintenance" as an activity that exposes bare soil

2/4: Include construction of layouts as an activity that exposes bare soil

2/last: Should be "Road and landing construction and use"

2/ It would be useful to include another category of influences: "Activities that encourage overland flow by increasing soil moisture and runoff volume through decreased vegetation cover: logging, herbicide use, burning, mechanical site preparation, construction of layouts"

3/1: This is presented as a sub-topic under the introductory phrase "Forest management activities that have the potential to increase erosion, transport, and delivery include:" Clearly, "Forest management practices that can limit the delivery of sediment to streams" does not fit there. Either create a different introductory phrase or alter the wording.

3/2/1: “Topographic conditions” is not a “forest management practice”

3/second to last: This is a very limited set of qualifications. Why wouldn’t a hydrologist be useful? If a team is to be put together, it would make a lot more sense to bring in people with a variety of areas of expertise. In what sense is “forest engineering” a field related to “geology”?

4/2/1: The key here is their erodibility when disturbed, *relative to undisturbed conditions*

4/3/1: This is not a useful question since the answers are already known. What we need to know is *how much* sediment the relevant activities produce, relative to undisturbed conditions.

4/4/1: Again, the key issues are how much of which grain sizes is delivered under natural conditions, and how specific land-use activities alter these input rates.

5/3/2: We will need to know *how* the potential effect of sediment on aquatic habitat and water quality will be determined. A statement that it will be determined is not sufficient to allow review of methods.

5/5/1: “Surface erosion” is probably intended here instead of “sheet erosion”

6/4/4: The statement that “sediment is generally not delivered to the stream system if adequate buffers exist on hillslopes” is not true where overland flow or piping is widespread.

6/4/8: Note that the hydrology module states that “pipeflow may be an important pathway for the movement of subsurface flow in the watersheds that comprise PALCO’s ownership” as part of an argument for the decision not to evaluate peak-flow changes caused by the presence of roads. If the presence of pipes is being used as a reason for not providing a method to evaluate road-related peakflow increases, then the absence of pipes cannot be used as a reason for not providing a method to evaluate pipeflow-related sediment delivery.

6/6/1: This does not have to be an assumption. It should be tested by field observations, since even if “most surface erosion occurs within five years,” the remainder (that occurring after 5 years) may still be extremely important. The length of time over which surface erosion continues is important for evaluating cumulative impacts.

7/2/1: Include “waterbars” with “ditches or culverts”: flow concentrated by waterbars behaves the same way as flow concentrated by ditches and culverts.

7/2/2: What is the delivery ratio if a culvert drains 201 feet from a channel?

7/3/2: Overland flow is common during large storms in this area, and turbid flow has been observed to travel for more than 200 feet from a road. It is essential that winter field-work be carried out to measure the flow distances characteristic of this area.

7/7/2: It is not true that “roads not delivering sediment to defined channels” are not contributing sediment. In this area, pipeflow is common, and at many locations runoff diverted from road surfaces disappears into soil pipes in otherwise “unchannelled” swales. It is a simple matter to follow these swales downslope during storms and observe the highly turbid water that emerges from the pipe at the head of the first-order channel downstream.

9/4: Considerable photographic documentation of erosion process type and distribution is reported to have been collected by the plaintiffs during preparation for the so-called “coho suit.” This

information, which PL apparently has access to, will be very useful for the successful completion of the watershed assessments.

10/second to last/2: “Most efforts are obvious, but things like gullying or skid trails that turn grassy swales into defined channels may require coordination.” This sentence does not adequately convey the intended meaning.

10/second to last/4: No methods are indicated for how erosion rates will be assessed. Instead, the “method” simply indicates that “methods selected to estimate the input of sediment from each source will largely be a function of available information in combination with those methods specified by the assessment modules.” This is a statement of intent rather than a description of a method. Until we know *how* the sediment budget will be constructed, we have no basis for determining whether the intended procedure is likely to be effective. At this point, given that fieldwork for the Freshwater assessment is reported to be in progress, methods are likely to have been developed and should be described here.

10/second to last/8: It is not appropriate to limit the analysis of cumulative impacts to 5-10 years into the future when the HCP and SYP were approved on the basis of 50-year and 120-year land-use plans. Under NEPA, a cumulative impact is defined as *”...the impact on the environment which results from the incremental impact of the action when added to other past, present, and reasonably foreseeable future actions regardless of what agency...or person undertakes such other actions”* (CEQ Guidelines, 40 CFR 1508.7, issued 23 April 1971). It would appear that the rather detailed description of 120 years of planned land-use activities included in the approved HCP/SYP would make those activities qualify as “reasonably foreseeable.”

10/last paragraph/3: “Some of these items suggest preliminary synthesis discussions.” Is this statement intended to imply that preliminary synthesis discussions will be held?

11/mass wasting/point 1: Neither this module nor the mass wasting module describes how road failures will be evaluated.

11/point 4 under mass wasting analyst: “discuss relative importance” – for a discussion to be considered an analysis step, it must produce something substantive. Will decisions be made during the discussion? Are conclusions to be drawn? What evidence, if any, will the discussion be based upon? Discussions are called for elsewhere in the list on page 11, with similar problems.

11/stream channel/point 1: How will bank erosion be evaluated?

12/3/7: The surface erosion module states that the mass wasting module will determine background inputs from mass wasting. Unfortunately, the mass wasting module does not specifically include an analysis of background mass-wasting rates. Incidentally, soil creep is the component of mass wasting that is not included in the mass wasting module, and is the component of mass wasting that sets mass wasting apart from landsliding. It may be useful to label the first module as “landsliding” and this module as “surface erosion and soil creep.”

12/3/8: Note that 11/stream channel/point 1 indicates that it is not certain that “the stream channel analyst will handle streambank erosion.”

12/3/8: How will these three components of background erosion be combined? Sediment transported by soil creep generally enters the stream by bank erosion, so including both of these components would result in an overestimate of background sediment inputs. Furthermore, it is not clear how to

combine background landsliding rates and background creep rates, since soil creep also fuels landsliding to an unknown extent. Finally, if soil creep inputs are to be included as an input under undisturbed conditions, it would be necessary to consider the effects of land use activities on soil-creep rates. It is very possible that increases in soil moisture after logging would accelerate soil creep rates.

13/1/3: An “average” soil creep velocity for forested lands cannot be used to validly estimate sediment input from soil creep in Humboldt County steeplands any more than an “average” for landslide rates on forested lands could be used to estimate sediment input from landsliding in Humboldt County steeplands.

13/2/4: Note that the silt/clay and sand fractions are also the dominant components in aggraded banks of Freshwater Creek and Elk River, and that sand-silt aggradation is also very important within these channels.

15/3: As presented here, the erodibility based on K factor and slope is not equivalent to that used by CDF. The CDF method is reported to take soil characteristics, hillslope, vegetative cover, and precipitation into account. The alternative method, however, is based solely on soil characteristics and slope. The two methods therefore should not be mixed in the same analysis or results will not be consistent through the analysis area.

16/3/8: Is “determining” missing from this sentence?

17/Table 2: Levels of erosion potential are not consistent through the table. Tractor yarding with “waterbars intact” is represented to have a lower erosion potential than “some off-road vehicle use, mostly confined to existing roads” and to be equivalent in erosion potential to full-suspension cable yarding. In addition, intact waterbars don’t solve the erosion problem in areas where overland flow occurs.

17/Table 2: It is not clear what “3 inches deep or more” refers to.

17/Table 2: “Skyline” is not equivalent to “cable” yarding.

18/Figure 1: It is not appropriate to assume that the presence of a buffer prevents delivery of sediment. Turbid overland flow has been observed to travel considerable distances in this area, and extensive piping networks have been observed to carry culvert effluent for long distances along otherwise unchannelled swales. It will be necessary to observe runoff processes during winter storms to determine which delivery mechanisms are active in areas of different land-use histories and geologic types.

20/2/2: Weather records from Eureka and Scotia will under-represent storm intensities. It would be useful to compare results for equivalent record lengths from higher-elevation gauges in the area to those from the valley-bottom gauges.

21/Table 3: It is highly inappropriate to model a tractor-yarded clearcut with any combination of broadcast burn, short grass, no burning, or low severity fire, as the table suggests is intended. If the model has not been calibrated for the practices used in this area, the model should not be used. This table could not have been constructed without circular reasoning. For example, the “selective entry band” is being modeled as the equivalent of a “5-year-old forest.” There are no inherent similarities between a selectively logged forest and a 5-year-old forest, so the selection of the “analogous” treatment must have been based on an assumption that the amount of sediment produced by selective

logging practices in this area is equivalent to that produced by a 5-year-old forest in some other area. This reasoning presumes that one already knows how much sediment would be produced by selective logging in this area. However, the whole purpose of using the model is to estimate how much sediment is produced by these treatments in this area. It is not appropriate to guess the answer to select the method one needs to get the answer expected.

21/1/4: “Unit length is the length of sediment travel down slope” needs clarification

22/1/1: This sentence is not clear. What “assigned treatment” is being used to model the list of management activities described in Figure 2? How long is the “assigned treatment” applied to represent the 12.5-year period? Are the “assigned treatments” the surrogates listed in Table 3? Was the surrogate selected by figuring out which treatment gives the same answer as the sequence listed in Figure 2? This approach would be more valid than the “just guessing” approach discussed in the comment for 21/Table 3. However, the sequence of recovery steps and timing listed in Figure 2 is not appropriate for this area. It would be more useful to establish the sequence based on field observations, and to carry out calculations for the observed sequence rather than for a surrogate. Note that it will be necessary to evaluate skid roads as roads rather than including them as part of a homogeneous cut unit, since skid roads do not revegetate well in many areas and since they continue to generate excessive surface runoff for many years after logging is completed.

22/2/7: “...slopes delivering to a unit boundary had 100% delivery” This does not appear to be consistent with Table 4. Is this intended to mean that these slopes had 100% delivery *to that boundary*, but that what happens after the sediment reaches the boundary depends on the distance between the boundary and the stream?

23/Table 4: The delivery ratios for units not bounded by a watercourse are lower than expected for this area, where overland flow and sub-surface piping are common. Note, also, that it will be necessary to consider tractor skid roads as roads rather than as part of a cut unit; delivery from skid-road waterbars would be governed by the same considerations as that from road ditches and culverts.

23/1/2: Results of this kind of modeling cannot be applied in this way without providing for an independent check of the results. This could be through field observations during winter storms and comparison of predicted outputs to those calculated from sediment monitoring data.

26/2/3: Such fieldwork will need to be carried out during winter storms to evaluate the extent of delivery by overland flow and piping. Note also that the procedure being used is based on small data sets from limited areas. Winter field work would also allow simple grab-sampling of road-surface runoff to provide information needed to determine whether the Washington method is applicable to northwest California conditions without recalibration to account for the distinctive geology, road standards, and climate of this area. Such information would be easy to collect.

29/Table 8: The use categories in the table are not satisfactorily defined. A road that receives log haul traffic for less than 50% of the year would necessarily have traffic limited to “light traffic” for “the majority of the time.” Therefore, no road can fall into the “moderate traffic/active secondary” category. I see the distinction that the authors are trying to make, but it would be useful to construct the distinction more clearly to avoid misunderstandings. It should also be made clear that “50% of the time during the year” refers to units of weeks, not hours, since even a road that is hauled every working day is receiving log-haul traffic for less than 50% of the time. Technically, then, all roads would fall into the “light traffic/not active” or “abandoned” categories.

30/2/3: Field observations concerning road-surface condition and morphology would need to be made during the wet season.

30/5/1: This statement conflicts with that at 7/2/2, where it is stated that delivery from road ditches that drain within 200 feet of a stream is to be considered 100%. In this area, because of the widespread occurrence of overland flow and soil pipes, it is reasonable to assume that road ditches, culverts, and water bars that discharge within 200 feet of a channel deliver a very high proportion of their suspended sediment load to the channel.

30/7/1: It would be expected that travel distances for sand-rich batholith sediment would not be as large as for the silt- and clay-rich sediment of the northwest California Coast Range.

33/1/4: Unsurfaced seasonal roads will need to be observed at the end of the wet season to determine the extent of rilling and gullying before these surfaces are repaired for dry-season use.

33/6/2: Evaluation of the model's ability to predict delivery cannot be based on dry-season inventories of road delivery points. Delivery during storms is substantially higher than dry-season evidence would suggest, since the extent of piping networks and overland flow is not evident during the dry season. Instead, a few day's fieldwork during large winter storms would be necessary to test the model. A comparison of SEDMODL results and PWA's survey is not a valid test of the model.

34/2/4: Direct measurement of road erosion does not require a multi-year study, and it is very feasible to carry out such work in the time frame of the watershed analyses planned for PL lands. It simply requires evaluation of sediment rating curves for effluent from selected road segments during a half-dozen moderate to large storms.

34/3/2: WEPP cannot be used to test the validity of the WDNR estimates since WEPP must also be calibrated for the conditions present. Furthermore, sediment delivery ratios are a major source of uncertainty, and these cannot be evaluated without field observations during storms.

34/5/1: Both Federal and State definitions of cumulative impacts indicate that the effects of "reasonably foreseeable future actions" must be evaluated if cumulative impacts are to be validly assessed. It was considered reasonable by PL, State agencies, and Federal agencies for the HCP/SYP to forecast actions for a 120-year period. It therefore would be prudent to evaluate conditions resulting from the actions that Federal and State taxpayers have been told were reasonably foreseeable. Alternatively, if at this point the HCP and SYP forecasts have been found to be unreasonable, then it would seem necessary for an amended HCP/SYP to be prepared that describes what the reasonably foreseeable future actions actually are.

35/ff: This section is copied from Mass Wasting 46/ff. Instead of repeating the same text, it would have been useful to compile a section that describes *how* the sediment budget is to be constructed.

35/1/6: Are these intended to be examples of what *will* be done? or examples of what *could* be done? If these are to be part of the analysis, *how* will they be done?

35/6/1: The introduction to this manual states that construction of sediment budgets is a necessary part of the analysis. Now we learn that erosion rates are to be evaluated (i.e., a sediment budget constructed) only in certain cases (such as "if stream enhancement is contemplated"). This is not consistent. It is disconcerting to read the entire document and still not know what work is actually *intended* to be done. The document would be more useful if it provided a summary of what work *will* be done and a separate list of what work *might* be done.

36/4/3: How is the “regulatory framework for sediment inputs” intended to be “addressed” by the group? The “regulatory framework for sediment inputs” includes the HCP/SYP directive that any activities carried out under the HCP/SYP must comply with the Basin Plan objectives developed pursuant to the Porter-Cologne Act. If forest practice prescriptions are to be altered on the basis of information provided by watershed assessment, then that information would need to be sufficient to ensure that the requirements of the HCP/SYP are met. Enough information would be needed to determine whether the prescriptions meet the Basin Plan objectives. As currently formulated, the method cannot do so because the method does not provide the necessary information concerning impacts to the beneficial uses of water specified by the Basin Plan.

A-1/ff: Inclusion of information on the model proposed for use is very much appreciated.

Section: "Hydrology"

General comments:

The hydrology module contains two components, an analysis of "how well flood history is explained by climatic variables alone" and a calculation of the likely influence of logging on peak flows. The first analysis component will not be capable of discerning the magnitude of change expected, given the variance that will arise from the lack of rainfall and runoff data for the specific watersheds under consideration.

The second component of the analysis uses a model based on data from North Fork Caspar Creek watershed. At Caspar Creek, however, road density is low, less than half the logged area was burned and herbicided, and only 15% of the area was yarded by tractors. Where road densities are higher and burning, herbiciding, and tractor yarding are more common, calculations based on Caspar Creek results would be expected to underestimate the hydrologic effects of logging. The model also employs the assumption that hydrologic recovery is complete in 12.5 years. However, this assumption is not well-founded, being based on an extrapolation from the 5-year Caspar Creek record, which did not include data from after recovering stands were thinned. Calculations will also need to be carried out for more widely adopted hydrologic recovery rates, such as the 50-year period quoted in the mass-wasting module and the 30-year period used by the Forest Service.

Specific comments:

3/4/3: It should be noted that the 1964 flood was a rain-on-snow event even at low elevations in northwest California, and that areas as low as Kneeland are temporarily snowed in with some regularity. It should also be noted that ski runs had been planned for the Horse Mountain area, but that plans were dropped about 15 years ago when shifts in the weather made the snowpack less dependable. In short, the issue is not whether winter temperatures "are generally too warm to allow for significant snow accumulation"—floods, after all, are not ordinarily caused by the "general" conditions. Instead, the question is to what extent land-use activities would modify the outcome, should the uncommon weather conditions that promote flooding occur. It doesn't matter where rain-on-snow usually occurs. Instead, what matters is where rain-on-snow could occur during a flood-producing event.

There are relatively simple analyses that could be done to evaluate the potential importance of rain-on-snow in this area. It would be very useful to use local climatic records to assess the likelihood of occurrence of another rain-on-snow flood, and then to calculate the potential effect of the past, current, and future activities in the watershed. If the analysis shows no likely influence, the matter is laid to rest. However, if activities could increase the potential magnitude or frequency of rain-on-snow floods, then this effect must be considered in combination with other potential influences on peak flows to evaluate the cumulative impacts of land use on flood frequencies; this analysis, after all, was one of the specific requirements of the HCP. It will not be possible to assess cumulative impacts on peak flows unless some evaluation is made of the potential for rain-on-snow events.

Furthermore, changes in rain-on-snow frequency and magnitude are very important at the site where the rain-on-snow event occurs. Debris slides and debris flows are associated with rapid influxes of water to steep slopes at the upper elevations of watersheds, and these are the areas where the snowpack is likely to be deepest during a rain-on-snow event.

4/1/8: Upon what basis is the assertion made that the estimate of rain-on-snow elevations is "conservative"?

5/4/3: The “synchronization” argument is not valid for a case in which roads are already relatively uniformly distributed through tributary watersheds. In this case—which is typical of most areas after the first cycle of logging is complete—all tributary peaks would be rather uniformly altered, essentially preserving the general form of the original hydrograph but increasing the magnitude of the peak and diminishing the volume of runoff in the falling limb. Only in the case where a watershed is large enough that tributary hydrographs are markedly out of phase with one another would the effect not be an increase in the overall watershed peak. Even in this case, however, there would be increases in tributary watershed peaks, and these will be of concern because of their implications for increased sediment transport from tributaries. Increased transport in tributaries can contribute to increased aggradation in the receiving channel.

7/2/3: The general pattern is well-established that the more compacted area there is in a watershed, the higher the peakflows tend to be. This is the basis for the widely used “Rational Runoff Formula” (Dunne and Leopold 1978). If there is to be an analysis of the cumulative impacts on flooding, it will therefore be necessary to evaluate the potential influence of roads and skid trails. It should be noted that a report from the University of California Committee on the Scientific Basis for the Analysis and Prediction of Cumulative Watershed Effects stated that, with respect to peakflow changes in Freshwater Creek, “The effect of roads, which apparently was not apparent in the Caspar Creek study, are too well established elsewhere and in basic hydrologic theory to be ignored...” (Dunne 1999, p.7 ph.2). Dunne (1999, p.6 ph.2) also notes that, with respect to the results of studies by La Marche and Lettenmaier (1998) and Bowling and Lettenmaier (1997), “These calculations of the road effect should be taken as a minimum for Freshwater Creek basin.”

7/3/5: It would not be difficult to evaluate the potential magnitude of the influence of roads and skid trails in the time-frame available for watershed analysis. Estimates could be made on the basis of the proportion of each sub-watershed compacted by roads and skid trails. Or the model already used by Dennis Lettenmaier (Bowling and Lettenmaier 1997, La Marche and Lettenmaier 1998) for a similar analysis could be used—this would take considerably less effort than that being expended (in the time frame available for watershed analysis) to use the LISA model to attempt to evaluate slope stability. The need for “extensive climatic data” is not a barrier to using the model, as results could be modeled using the same gauge records that the module authors are assuming to adequately represent the watershed for the purpose of runoff modeling. “Synchronization” is not a barrier, for the reasons described in the 5/4/3 comment. It is not reasonable that this relatively simple exercise is being dismissed out of hand as being impractical, while even less-accurate runoff modeling is intended as the basis for answering the critical questions. Further, analyses in the surface erosion and landsliding modules are intended to be based heavily on the results of the WEPP and LISA models; these “spatially distributed, physically-based models” are not “considered beyond the scope of watershed analysis,” despite the “extensive...data inputs required, the need for long-term...records from within the watershed for model calibration, and the difficulty in model parameterization.” In this case of road runoff, we need only an estimate of the potential magnitudes and relative magnitudes of the road effect if cumulative impacts on hydrologic response are to be evaluated. Sensitivity analysis for more questionable input variables would indicate the level of certainty of the calculated results.

7/3/5: Note that “the time frame of a watershed analysis,” as presented in the introduction, is not valid (see comment i/3/4 under “Note to reviewers). The information concerning the impact of roads is necessary for a valid assessment of cumulative impacts, so it may be appropriate to involve personnel assigned to the cumulative impact module to help with the road analysis.

8/2/9: This sentence is ambiguous, as it can be construed to mean that the effect had ended after 8 years. However, effects were documented for the 8-year period after logging because only 8 years had passed at the time the paper was written. Keppler (1998) notes in her abstract that “NFC flow enhancements persist through hydrologic year 1997 with no recovery trend, as yet.”

8/3: It is not valid to assume that increased annual and summer water yields are beneficial. Important components of the herpetofauna depend on low-order channels and are adapted to the natural flow regimes in those channels. If these flow regimes are modified, the habitat on which those species depend is modified away from that in which the species evolved.

8/4/8: Why would a clearcut and burned watershed show decreased fog drip while a clearcut and unburned watershed does not?

10/1/1: In this case, too, achieving redundancy in expertise on the module team is not as effective as constructing a team with enough breadth of expertise and experience to be able to handle the interdisciplinary aspects of the cumulative effects problems likely to be assigned to the module team.

11/3/1: Given the lack of monitoring data and the variation in rainfall between adjacent stations, it is not reasonable to expect that an analysis of flood history could be used to determine how well it can be explained by climatic variables alone.

11/1ff: It may be useful here, too, to remove the “procedural” questions and focus primarily on the fundamental questions: How much do land-use activities in the watersheds alter naturally occurring peak flows? How much would any resulting changes influence downstream beneficial uses? The procedural questions listed on p.11 could then become sub-topics that provide an indication of what questions will be answered along the way to answering the fundamental questions. Such a structure is useful because it makes it clear that the only goal that really matters is answering the fundamental questions.

11/ Section 2.2 questions 3 and 5: Detailed stand-age maps for the next 120 years were submitted as part of the HCP/SYP, and cumulative impact analysis requires evaluation of “reasonably foreseeable future actions.” It will thus be necessary to evaluate the period for which future actions have been foreseen.

12/3/1: “Short-term gage records can be extended using multiple regression techniques”: This cannot be taken as an assumption. The method must be tested for the area to determine if the resulting uncertainty in estimated discharges is small enough to allow detection of the magnitude of change expected.

12/4/3: “Available precipitation data...can be extrapolated across the watershed...”: This also cannot be taken as an assumption. This procedure must also be tested for the area; it cannot be simply assumed to work. Comparison of Kneeland and Eureka rainfall records shows little correlation for large storms, suggesting that the approach is unlikely to provide the accuracy needed by the proposed method.

12/5/3: Note that the “growing season” in mild coastal areas includes the winter.

12/6/2: Insignificance of rain-on-snow: This also must be tested for the area; it cannot be simply assumed. See comment on 3/4/3.

12/7/1: It would be useful to clarify that “the best available science” includes a lot of studies in addition to those from Caspar Creek.

13/1/2: The treatment at Caspar Creek was not necessarily the same as on PALCO ownership: little tractor yarding was used, some areas were neither burned nor herbicided, and road densities are low.

13/5/2: Because forest management practices used during the North Fork Caspar Creek experiment are not necessarily the same as those commonly used on PALCO lands, model results will need to be adjusted for expected differences. Minimal tractor yarding was used at North Fork Caspar (7.6% of the watershed), and less than half of the logged area was burned and herbicided (19.5% of the watershed). Roads in the area are built along ridges, and road density is low. The model will thus underestimate the hydrologic effects in watersheds with higher road densities, more widespread use of burning and herbicides, and a higher proportion of tractor yarding.

13/5/3: Hydrologic recovery at Caspar Creek was estimated as linear for the first 5 years after logging. However, the study included data only through Spring of 1996, at which time none of the units had yet been thinned. It must be assumed that thinning of the stand would partially “reset the clock” in proportion to the amount of canopy removed. Furthermore, given characteristic growth curves, it seems unlikely that trends for the first 5 years can be extrapolated validly to characterize the entire sequence of stand development. Keppler’s study, for example, shows no hydrologic recovery after 7 years (Keppler 1998), and hydrologic recovery is elsewhere in the manual reported to require 50 years (Landslide module p.28). Relations between canopy interception and basal area of stands, in combination with those for stand development, also suggest that the hydrologic impact will last considerably longer than 12.5 years (Reid 1999; and see Figures 1 and 2 in this review). Hydrologic recovery rates relevant to rain-on-snow events should also reflect recovery rates relevant to rain events. Since part of the mechanism for increased flooding during rain-on-snow events (canopy interception and sublimation) is analogous to that expected for increased peaks during rain events (canopy interception and evaporation), it is not reasonable that hydrologic recovery is expected to takes ¼ the time for rain events as for rain-on-snow events.

13/6/6: Here, too, assumption of 8% recovery per year is not appropriate for the reasons discussed under 13/5/3.

13/7/2: The land-use history will need to be evaluated in the gauged watersheds at the time that gauging records were being collected. Without such analysis, it cannot be assumed that these records represent “baseline” conditions.

14/2/2: As described in earlier comments (7/2/3, 7/3/5), a quantitative analysis of the hydrologic effects of the road network is both feasible and necessary for the cumulative impact evaluation.

16/5/3: The “Historic Condition assessment” was also referred to at 20/1/1 in the landslide module, yet no description of such a module is provided. What is intended?

17/2/3: The statement that peak flow increases will be evaluated for their significance in the cumulative effects module is not borne out by the cumulative effects module, where no provision is made for such an analysis.

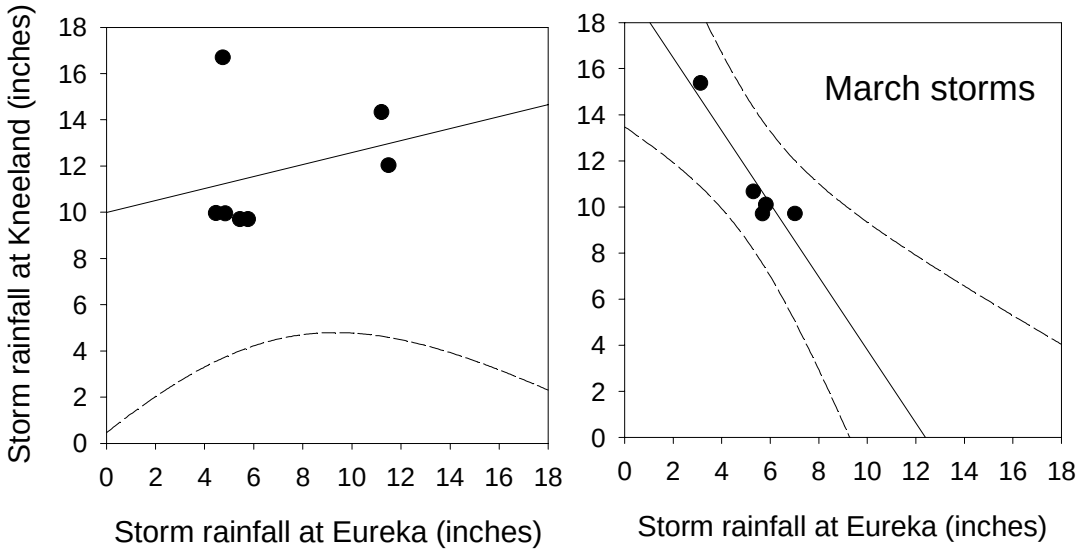

Figure 3. Relation between storm rainfalls at Eureka and Kneeland for the 7 of the 18 largest storms recorded at Kneeland (over a 40-year period) that occurred in November and for the 5 of those storms that occurred in March. The dashed curves indicate the 95% confidence interval for prediction.

17/2/5: Considerable coordination is also needed with the landsliding module to allow evaluation of the effects of altered hydrology on landslide susceptibility.

18/3/1: There is a big problem here. The intention is to determine whether the land-use history *in the ungauged watersheds* contributed to flooding by first estimating the peak flows in ungauged Freshwater, Stitz, Jordan, and Bear Creeks, among others, from records from nearby gauged watersheds that have undergone different histories of land use, and then comparing the peakflows predicted from rainfall to those estimated by regression. This just doesn't work. The overall approach would work only if 1) rain-gauge data in the area are adequate to characterize the rainfall in the watersheds, and 2) if stream-gauging records existed *for those watersheds*. Consider the expected magnitude of peak-flow increase that might occur in a 50% cut watershed immediately after cutting (18% to 30%). Now consider the accuracy of USGS estimates of flood peaks, the accuracy of the estimate of peak discharge based on extrapolation of records from a gauge in a different watershed, and the accuracy of rainfall estimates in a watershed in which there are no rain gauges. The expected magnitude of change could not be detected using the proposed method, even though an increase of that magnitude would cause a significant increase in the frequency of flooding. It is not appropriate to rely on the results of an analysis that is inherently incapable of detecting a change of the magnitude anticipated. As the authors stated previously (3/1/6): "...it is important to remember that non-detection of an effect is not equivalent to the detection of no effect."

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20/3: The PRISM maps are based on data from the same gauges that are being used to evaluate the individual storms. If an averaged map like PRISM actually provided a valid estimate of rainfall patterns within individual storms, then Kneeland storm rainfalls should be predictable from Eureka storm rainfalls. Examination of the rainfall records shows that there is no significant correlation between these stations for the 7 of the 18 largest storms recorded at Kneeland (Figure 3) that occurred in November and for the 5 of those storms that occurred in March, indicating that any approximations made using the PRISM maps will provide results with very high levels of uncertainty. If the expected uncertainty of this component of the analysis alone is larger than the anticipated magnitude of change, conclusions will necessarily be indeterminate.

25/2/3: How is the wetness index defined?

26/Figure 4: If there were 26 years of record, why isn't a storm shown for a recurrence interval of greater than 13 years? For that matter, why are only 11 of the 26 annual peaks shown?

26/Figure 4: Given the variance of the relationship for average wetness, it is not appropriate to simply select the two lowest and two highest points to estimate the relationships for minimum and maximum wetness. What is actually needed is a probability distribution for wetness at the time a major storm is likely to occur. This might reasonably be done by evaluating the distribution of wetness-index values by month.

27/1/9: *How* will this be used to evaluate the sensitivity of peak flows to canopy conditions?

27/3/5: What "Historic Condition assessment"?

28/2/5: There has in the past been some misunderstanding of the "materials assembled for the EIS." For example, an analysis by Matt O'Connor (1999) assumed, on the basis of forest-cover information provided by PALCO, that only 7% of the canopy cover in Freshwater and Elk watersheds had been removed between 1988 and 1998. However, data compiled by CDF from Timber Harvest Plans (CDF 1999a, 1999b) indicate that approximately 30% of the forest canopy has been removed from Freshwater watershed during the past 10 years. It thus will be important to ensure that the available information is actually correct.

31/1: Note that the area bared by roads and skid roads will need to be evaluated, also.

31/4/years since harvest: The 14-year limit is not adequate, as discussed above (see 13/5/3). Information will also be needed concerning when and by how much the units were thinned.

31/5/1: Hardwood and shrub canopy will need to be considered separately from conifer canopy, since many studies demonstrate that rates of interception by broad-leaved trees and shrubs are not as great as those by conifers. Leaf area indices are smaller on broad-leaved trees and shrubs, and branch structure and bark texture produce a higher proportion of stem-flow. An appropriate distribution of weightings for different kinds of canopy cover might be estimated according to estimates of leaf-area index in the original stand and the logged stand. Alternatively, data from the clearcut, unburned stands in Caspar Creek could be compared with those from the clearcut, burned stands to estimate the difference in peakflows that results when understory vegetation and duff is left more-or-less intact (except on skid trails). Average increase for the 2-year peak-flow from the clearcut, unburned watersheds was 22% at an average of 2.8 years after logging, while that for the clearcut, burned watersheds was 33% (data from Reid 1998 and Henry 1998).

33/1/2: For this analysis, use of regional peak flow magnitudes is justified even though the “baseline” values reflect some land-use influence, since these values are being used simply to allow comparison between hypothetical cases (compare comments on 18/3/1).

34/2/1: Tractor skid roads will need to be included as part of the road network.

35/1/4: How accurate is the available GIS coverage of stream length? How will it be tested?

36/1/1: Available methods should be used to estimate the potential magnitude of the effects of roads. Otherwise, the cumulative impact analysis will not be adequate.

36/1: A variety of other analyses will also be necessary, including analysis of the effects of altered canopy cover on rainfall intensities and soil moisture at potentially unstable sites on hillslopes.

37/2/10: Caspar Creek results show no significant influence of watershed size on the relation between peak-flow increase and percent logged, so there is no evidence that the relation will not hold for the watersheds in question. Furthermore, the North Fork Caspar Creek watershed is larger than most of the Hydrologic Analysis Units that will be evaluated in these assessments. One of the important effects of increased discharge is likely to be a resulting increase in sediment transport from tributary watersheds, so analyses of altered discharge in watersheds the size of North Fork Caspar and smaller are a very important part of the analysis.

Section: "Riparian function assessment"

General comments: The riparian module does not provide the information needed to evaluate the cumulative impacts of past, present, or future land-use activities in a watershed. Furthermore, the methods used for both parts of the analysis are not valid.

The introduction to the module states that it will evaluate "LWD recruitment potential for the near term...and long term trends; the latter extending beyond the life of the HCP..." Later, at 15/2/3, it indicates that only a qualitative prediction of the long-term trend will be provided, and it then states that "Assessment of long term recruitment potential does not result in hazard calls during synthesis" (16/2/1). Elsewhere (6/5/9) the module indicates that the evaluation of cumulative effects on LWD will not be done during the cumulative effects analysis, but by individual THPs or by the prescription team. This deferral of responsibility makes it impossible for the proposed method to evaluate cumulative effects on the species of concern to the HCP. Furthermore, the WDNR approach develops prescriptions only for those issues for which hazard calls have been made; the approach described here thus will not allow for prescriptions to be made to satisfy long-term woody debris needs. If only the qualitative long-term trend is to be evaluated, there will be no tenable guidance for near-term stand management. Near-term decisions concerning no-cut buffer widths, for example, would not be valid in the absence of specific information on long-term woody debris needs and cumulative impacts associated with past, present, and reasonably foreseeable future riparian stand management.

The information necessary for short-term management decisions is a description of the extent to which current woody debris recruitment potential differs from that expected under natural conditions, and how the recruitment potential will change in the future. This information is also needed if a valid cumulative effects assessment is to be carried out.

Although the module introduction states that "Desirable or target levels of LWD (amount, size and type) are defined" by the module and indicates that "The LWD target is established at a level of in-channel LWD occurring within unmanaged lands such as Redwood National Park." However, the module does not describe how this level will be defined. Examination of the fisheries module indicates that those values are defined in an appendix table to that module, but the target levels presented are not reasonable. According to the combined results of the riparian and fisheries modules, a creek the size of Godwood Creek in Redwood National Park—a tributary currently lined by redwood trees with basal diameters of 50 to 200 inches and hosting woody debris of similar sizes—would be *defined* to have conditions equivalent to those of an old-growth redwood forest if it had one 4-inch-diameter, 6-foot-long piece of wood every 40 feet and were located in a stand with a 40% canopy cover of 11-inch diameter trees.

The method used for evaluating stream temperatures is also not valid. First, only a single influence on temperature (overhead shade) is evaluated, making it impossible to assess cumulative effects on water temperature. Second, small tributaries are ignored, also making it impossible to assess cumulative effects on water temperature. Third, the role of thermal refugia is ignored, making it impossible to assess the effects of thermal change on anadromous fish. Fourth, the method proposed for evaluating cover density over streams is not useful because aerial photos provide a vertical view only of the center-point of the photo. Because most channels will thus be viewed obliquely, the water surface will rarely be visible even if there is no overhead vegetation. The method will lead to systematic and severe overestimates of canopy cover.

The results of the module analysis, as currently designed, will inevitably lead to systematic and significant underdesign of buffers because the level of existing and future cumulative impacts to riparian function will be systematically and significantly underestimated. The module, as presented, is

not capable of evaluating cumulative impacts associated with altered woody debris loadings and it is not capable of answering the critical questions that it poses.

Specific comments:

2/3/1: The module states that the mass wasting module will evaluate the "role of mass wasting events to transport LWD from headwater class III streams to downstream channels," but the mass wasting module does not describe how such an analysis will be done.

2/4/1: The mass wasting module does not describe an adequate analysis procedure for determining the role of trees in hillslope stabilization. The effects of foliage interception would need to be considered as well as those of root strength if the role is to be determined by modeling; a strategy for systematic observations would need to be described if the role is to be determined empirically.

2/5/1: The surface erosion module does not include an analysis of the role of LWD in buffering of sediment inputs.

2/5/1: The hydrology module does not include an analysis of the frequency of flood events mobilizing LWD.

4/3/2: Note that air temperature is a very important control on stream temperature, so microclimate must be evaluated also. Evaluation of shading alone is insufficient to evaluate the cumulative impact on stream temperature.

4/last paragraph/1: In many areas, the first riparian stand established is hardwoods. The composition of a late-successional riparian forest thus is not the same as that indicated by the tree species mix present when the forest was established.

5/1/1: Well-stocked riparian stands dominated by large conifers will not necessarily provide adequate and sustainable supplies of LWD 1) if those stands are selectively logged; 2) if species composition is not appropriate for the site; 3) if the size distribution is not characteristic of natural stands; 4) if stands are too narrow; or 4) if management practices adjacent to the stands result in unnatural mortality patterns in the stands.

5/3/1: Hardwood-dominated riparian stands are the only stands capable of providing appropriate woody debris inputs at sites that naturally supported hardwood stands.

5/4/1: LWD must also be large in small channels to remain functional, since part of the function of the woody debris in Class III and Class II channels is to prevent small slides or incipient debris flows from growing into large slides or large debris flows. Very large wood stops slides more effectively than smaller wood. It is not appropriate to consider woody debris function only in terms of aquatic habitat needs. Note that habitat needs for amphibians also must be considered.

5/5/2: Forest practices can also strongly and directly influence stream temperature regimes by modifying microclimates in riparian stands; water temperature strongly reflects air temperature. Forest practices also influence stream temperatures indirectly by contributing excess sediment that fills pools and widens channels, thus decreasing average water depth.

5/7/3: It is not clear what is meant by "...has traveled a regionally specific distance (or further)..."

5/last paragraph/2: It is not appropriate to assume that the proximity of Humboldt County to marine influences will override the influence of canopy closure. It has been demonstrated at a variety of sites

that decreased canopy cover increases stream temperatures to lethal levels for some species even within a few miles of the coast (see, for example, comments by Welsh et al. on the HCP/SYP).

6/1/1: Small, non-fish-bearing tributaries also significantly influence the temperature regime of even very large channels simply by providing localized cool-water refugia. Even though they may not individually influence the temperature of the larger river significantly, they may be a dominant factor in making the distribution of temperatures present at any given time habitable to the cold-water fish. Furthermore, the assumption that small tributaries can be ignored is not compatible with the requirement to evaluate cumulative effects: “Cumulative impacts can result from individually minor but collectively significant actions taking place over a period of time” (CEQ Guidelines, 40 CFR 1508.7, issued 23 April 1971). If multiple small, hot tributaries enter a larger channel, the cumulative effect of the inflows may be very significant.

6/2/1: Because the ability of riparian shade to moderate water temperatures diminishes as channel width increases, the importance of thermal refugia produced by small, cool-water tributaries increases.

6/3/1: Canopy closure of at least 75% is not in itself sufficient for thermal regulation of streams. In addition, the air temperature regimes within the riparian stands must be appropriate.

6/5/8: The module here seems to imply that it will not evaluate the long-term effects of practices on woody debris loads, but that such analyses will be done in individual THPs or during the prescription phase. This deferment of the cumulative impact analysis will make it impossible to evaluate cumulative impacts during watershed assessment, as required by the HCP.

8/1/8: The module first states that “The LWD target is established at a level of in-channel LWD occurring within unmanaged lands such as Redwood National Park,” and then states that “The riparian analyst may modify the loading rate based on field assessments where historic riparian conditions could not be expected to supply sufficient LWD.” If “historic” is intended to mean “that left by historic logging” instead of “natural,” this statement would suggest that the target levels of LWD loading will be determined on the basis of what the existing stand can provide. This would clearly not be a valid approach. The passage needs to be rewritten to clarify the intended message. Furthermore, it is not clear what is intended by “a level of in-channel LWD occurring within unmanaged lands”: would this be any level falling within the *range* of those measured on unmanaged lands, or would the *distribution* of debris loadings need to be equivalent to those on unmanaged lands? The former would not be a valid approach, since the naturally occurring range would include everything from zero to large. The intent of this statement must also be clarified.

8/2: It will also be necessary to evaluate the role of LWD in preventing landslides and debris flows and in restricting their size; this is a very important role of LWD in Class III and Class II channels.

8/5/2: The role of LWD in storing sediment is not being evaluated in the mass wasting and surface erosion modules.

9/1/1: The mass wasting module does not appear to be providing the information that would be needed to evaluate the magnitude and frequency of woody debris inputs.

9/9/3: I know of little local data on wood depletion rates, and it is not valid to apply literature values from elsewhere to characterize depletion rates of old-growth redwood. Fortunately, this approach is not necessary. If it is assumed that natural stand conditions provided an adequate supply of woody

debris (as implied by the statement at 8/1/4: “The LWD target is established at a level of in-channel LWD occurring within unmanaged lands such as Redwood National Park”), then all that is needed is the assumption that those stand conditions resulted in rates of input and depletion that produced an appropriate average loading level at each site along the channel system. The extent of deviation of the current stand condition from that of a natural stand is thus an index of the extent of deviation of debris loadings from the target levels.

10/2/6: It was stated earlier that the target LWD loading rates *are* the rates associated with old-growth watersheds (8/1/4). It is thus not clear why it is here specified that “Realistic LWD loading rates should be established no higher than those associated with unmanaged watersheds.”

10/second to last paragraph: Class III streams must also be included in the analysis because of the importance of LWD in stabilizing these sites and because of the potential importance of these locations for supplying LWD to downstream sites.

10/last paragraph: the area to be characterized must include the distance over which LWD could be recruited to the channel under natural conditions. Unless this entire area is evaluated, it will not be possible to answer the critical questions posed under “Large woody debris recruitment” (3/last two paragraphs). The width of this zone will need to be one old-growth tree height from the edge of the channel migration zone.

11/2/5: It is not sufficient to evaluate stand conditions on Class I and “some larger Class II” streams; characterization must be carried out for the entire channel system. This might be done by overlaying a channel network map, a silvicultural practice map, and a stand-age map. Representative sites for different WLPZ practices and stand ages would then need to be characterized.

11/2/7: If riparian condition units are based on vegetation type, size, and density, while channel units are not based on these characteristics, how could break points for RCUs be expected to correspond to break points for channel units?

11/3/5: The WHR system does not permit adequate evaluation of woody debris recruitment potential. According to this system, a 50-year-old redwood stand with 24”-diameter boles would fall into the same category as an old-growth stand with 70”-diameter boles. Target loadings were described at 8/1/4: “The LWD target is established at a level of in-channel LWD occurring within unmanaged lands such as Redwood National Park.” If the stand classification system is incapable of distinguishing between young second-growth stands and old-growth stands, it should not be used to determine whether existing stands are capable of sustaining LWD loadings characteristic of old-growth conditions. A valid system of categorization would instead use information about the logging history in the watershed. Riparian stands were routinely logged until the 1970s, so in areas logged before about 1970 the age of the present stand usually can be estimated from the date of the first logging. Since 1970, the treatment of WLPZs depended on the practices used, and these varied through time but can generally be recognized on aerial photographs.

11/Table 1: The headings on columns 3 and 4 refer to crown diameter, so I assume that the entries under hardwood/pole tree and conifer/small tree should be in feet rather than in inches.

12/3/1: This statement conflicts with that at 11/2/5. Are all Class II streams to be categorized, or just “some larger Class II streams”? All Class III streams will also need to be categorized if determinations are to be made concerning appropriate Class III buffer widths in the future.

12/3/3: What is meant by “This channel coding system will mirror the channel assessments coding system”?

13/Table 3: The recruitment potential ratings are not valid. According to this table, although “The LWD target is established at a level of in-channel LWD occurring within unmanaged lands such as Redwood National Park” (8/1/4), a 20-year-old second-growth redwood stand having only a 40% cover density of 11-inch-diameter trees would be defined to be as capable of sustaining target conditions as the original old-growth stand of 70-inch-diameter trees.

14/1/4: If “The LWD target is established at a level of in-channel LWD occurring within unmanaged lands such as Redwood National Park” (8/1/4), then the target level will not vary by “channel sensitivity.” Channels of particular characteristics would have the target values that characterize analogous channels in unmanaged forests. It is not appropriate that “Channel sensitivity” appears to have been selected as the primary consideration in the rating. The functions of woody debris for maintaining slope stability, salmonid habitat needs, and sediment routing are no less important than the function of maintaining channel stability.

14/3/2: This approach is incomplete. Woody debris can enter a reach from adjacent riparian areas, from upstream, or from tributaries. Woody debris can leave a reach in the near-term through decay or by channel transport. A large flood may move existing LWD from a reach and replace it with LWD from upstream. Therefore, whether upstream reaches are impoverished in woody debris is also important for determining whether a site is at risk. Existing levels of LWD loading are necessarily temporary, particularly if logs are smaller than those characteristic of natural conditions.

15/Table 4: “Low” hazard calls for reaches with insufficient woody debris but a low “channel sensitivity rating” are not appropriate. LWD has many functions in channels other than those associated with channel change (see comment 14/1/4).

15/Table 4: Such an approach would also need to consider the potential for input from upstream.

15/2/5: How will “field observations of stand type, seedling and understory tree density data, and published information on successional pathways” be used to “estimate long-term recruitment potential”?

16/2/1: It is important that analysis of long-term recruitment potential be capable of resulting in hazard calls because the only way to avoid long-term problems is to adjust current management practices. According to the WDNR, only those issues found to be problems (i.e., that generate “hazard calls”) are addressed through prescriptions. Unless future hazards are identified, it will not be possible to write prescriptions to avoid them. Furthermore, it is evident from current stand conditions and current debris loads what the future deviation from “target” conditions is likely to be and the length of time over which the deviation will be maintained. The module, as presented, is not capable of evaluating cumulative impacts associated with altered woody debris loadings and it is not capable of answering the critical questions that it poses.

16/3/1: The effect of stand characteristics on air temperature also can influence stream temperatures, and this effect will need to be evaluated. In addition, Brosofske et al. (1997) found that ground-water temperatures modified by logging of hillslope stands can influence stream temperatures. In order to evaluate the cumulative impacts of logging on water temperatures, it will be necessary to evaluate this influence, also. Simply assessing the effects of shade is not sufficient to evaluate cumulative effects.

16/5/1: In order to evaluate cumulative impacts, it will be necessary to assess the thermal influences of all tributaries, even those contributing less than 20% of the flow to receiving waters. Consider: if three adjacent tributaries each contribute 19% of the flow, nearly 50% of the flow will have been contributed by tributaries ignored by the existing method. Legal definitions of cumulative impacts make it very clear that it is necessary to evaluate the combined influence of incremental impacts; it is not valid to ignore them because each is individually small. Furthermore, small tributary inputs of cold water can provide thermal refugia in large, warm channels. If the proposed method does not provide for the evaluation of thermal refugia in an area where thermal refugia are an essential component of coho habitat, the method is not valid. The assumptions that shade is the only relevant influence on stream temperatures and that thermal refugia may be ignored probably reflect the method's origin in Washington State, where conditions are not as warm as in California.

17/1/7: NMFS established these values "for most streams" in what area? If it was not specific to northwest California, where thermal conditions are more severe than elsewhere in the species' range, then it would be necessary to adopt the CDFG standard, which was developed based on conditions in California.

17/2/5: Note that these are reasons that thermal refugia are important in larger channels in northwest California.

18/last paragraph: This paragraph is very difficult to follow, and would appear to lead to the opposite conclusion than that drawn. With Beer's Law, for example, it would seem that if light is attenuated more by high foliage than by low foliage, high foliage would be *more* effective than low foliage in providing shade because it pervasively lowers the ambient light level. Furthermore, the foliage canopy in intact stands is densest toward the tops of the trees—that is why it is called the "canopy." Quite often, foliage is not even produced from the lower half of the boles. It is not reasonable to disregard the top third of the site potential tree height.

18/last paragraph/3: Note that direct beam solar radiation can enter from directly above the stream only at noon. Even if direct beam intensity is diminished by 50% to 10% between 6:00am and 11:45am and between 12:15pm and 6:00pm, the duration of the oblique exposure is substantially longer than that for the vertical exposure. If riparian stands are sparse or narrow, oblique beams will be abnormally effective in warming the water.

19/2/3: The wide channels must also be included in the analysis area if cumulative impacts are to be evaluated and if the role of thermal refugia is to be considered. In addition, the influence of widening *and shallowing* must be considered in these larger channels.

19/3/3: Estimates of cover density for shade calculations cannot be made from aerial photographs because radial distortion is present on aerial photographs: only the center-point of the photograph shows a vertical view. Farther from the center point, the view is of the sides of trees. Therefore, the percent cover at any distance from the center point will appear to be significantly higher than that actually present. Furthermore, it is not be possible to see the water surface of many Class II streams on 1:12,000 air photographs anyway because these streams are small. In addition, canopy is also important when it is not directly over the creek, since the sun is directly overhead for only a short period each day. Where the riparian canopy has been thinned or where the riparian buffer is narrow, the creek is illuminated for a longer portion of the day than under natural conditions. The long duration of exposure can make up for the lower radiation intensity of oblique beams. The proposed method is thus not capable of providing the information required to address the critical question.

19/3ff: Even if it were possible to evaluate shade cover from air photos, the list of diagnostics is not valid. Class II channels are small enough that if the stream surface is visible, so are the banks. Class II's thus fall into either >90% shade or <40% shade; the categories do not permit any intermediate values. In contrast, the lower Klamath River has a visible water surface and banks are visible at times, indicating that it falls into the 20-40% shade category.

19/last paragraph: The method calls for field verification only of the indeterminate and high-impact categories—this is clearly inappropriate. Since the method is very strongly biased toward finding no impact (see comment on 19/3/3), a protocol that requires testing only of the high impact category is not effective in correcting the error.

20/1/1: For a valid cumulative impact analysis, the comparison needs to be relative to natural shade levels, not relative to target levels at a site. This is an extremely important point: downstream temperatures are influenced by the temperatures of tributary inflows. If all tributaries have less canopy, warmer groundwater inflows, and higher air temperatures, and therefore are warmer than they originally were, the cumulative thermal impact could still be severe in downstream reaches even if all reaches have >75% canopy. Large rivers in California tend to be warm, so California rivers are more sensitive to increased thermal stress and loss of cold-water refugia than are western Washington rivers. Modification of a Washington method to suit California conditions must take this difference into account.

Section: "Stream channel assessment"

General comments:

The initial 60 pages of the stream channel module are clear and informative, and, if carried out conscientiously, would provide a good overview of channel conditions in the watersheds. However, the module leaves considerable discretion to the expertise of the analysts. Such discretion is appropriate only if sufficient allowance is made for oversight and independent technical review. In addition, the module excludes analysis of channels with gradients of over 20%, and this is not appropriate for conditions in northwest California because of the recognized potential for fluvial destabilization of steep, low-order channels. It will be important to develop a method for evaluating sediment inputs from gullying and destabilization of low-order channels, and to evaluate the role of hydrologic changes in causing such problems.

Specific comments:

2/2/9: Channel sensitivity classes are defined with respect to the channel form, not with respect to the beneficial uses. It thus is not possible to partially base "hazard calls" for LWD on channel sensitivity alone. Other factors—such as biological sensitivity—must also be considered.

3/1ff: Individual modules are being expected to evaluate cumulative effects, and cumulative effects are inherently interdisciplinary. It is not valid to assume that interdisciplinary problems can be solved by a team of people all with the same expertise. It is fitting that the team leader have a specific set of qualifications, but it is not fitting that the same set of qualifications apply to everyone on the team. The requirement for cumulative impacts assessment means that it is necessary to have a wider range of expertise involved in each of the modules than is needed for the more limited Washington application.

10/3/1: Because of the logging history in northwest California, it is necessary that a broader selection of aerial photographs be examined if the history of channel changes in the area is to be understood.

13/4/7: Contour crenulations have been shown to misrepresent drainage densities in this region. However, field checking of crenulation-based networks can be used to calibrate the method for more reliable local applications.

14/1/2: The 20% gradient is not the upper limit for systems in which fluvial influences are important in this area. Gullying of steep, low-order channels is a very important process associated with logging and roads in northwest California. It is essential that this fluvial processes be evaluated during watershed assessment.

14/1/3: Presumably, Montgomery and Foufoula-Georgiou's paper is now published—what's the citation?

15/4/1: Needs clarification: the statement implies that "segment length" is synonymous with "distance between slope breaks," which earlier text suggests is not the case (14/3/1)

16/5/6: If it is "unfortunate" that the 3% gradient break is not used to define segment categories, then it would seem to be a simple matter to revise the segment categories to incorporate the 3% gradient break. It seems unreasonable that the module would be designed to *not* provide the information needed to interpret the results.

17/3/1: It must also be noted that “transport reaches” can act as “response reaches” when sediment inputs are particularly high. The category delineations may not be appropriate for conditions in areas of high uplift rates, such as northwest California. It may be useful to redefine the category boundaries according to field and air-photo observations of the distribution of alluvial deposits in the relevant watersheds.

18/4/5: “Measuring the same cross-sectional area (transect)...”: Channel cross-sectional area cannot be measured from aerial photographs, and “cross-sectional area” is not synonymous with “transect.” It is not clear what this sentence is intended to mean.

20/2/1: This example indicates that the standard WDNR procedure expects the analysis team to extend the analysis beyond the standard method “when faced with uncertainty.” It is important that this point be stressed. Without such a provision, any method would be inadequate for answering the critical questions, since every area is slightly different and thus requires slight differences in analysis method.

24/6/1: It should be noted that segments representing both disturbed and undisturbed conditions are intended to be examined. Because of the history of logging in northwest California, local conditions are such that accomplishing this task in many cases is possible only by visiting sites outside of the watershed in question.

25/3/6: It is not appropriate to defer interpretation of results to the synthesis stage. Fieldwork is effective only if it is designed to test the working hypotheses. Otherwise, it is not possible to identify the kind of information that will be required to test the “causal interpretations.”

27/3/1: It should be noted that bed morphology is readily changed by environmental impacts; channel bed morphology does not provide “...a general indication of the style of potential channel response” unless something is known about past changes.

31/3/4,6: Indications of units were lost in the print-out of the version I was sent. Are they present in the original document?

31/3/9: “...approximate measures should be taken to ensure accuracy...” How can an “approximate measure” “ensure accuracy”?

62/1/6: “Recognition of when, where, and how to undertake more detailed analyses necessary to adequately understand watershed processes is a crucial component of Watershed analysis that must not be constrained prior to conducting the standard analysis.” This is a very important statement, indicating that the standard method is not defined as being sufficient even under the standard WDNR procedure. The standard procedure itself calls for more detailed analysis if necessary to answer the questions.

A-1/Title: It is not clear why these modifications are “optional.” If they were designed to address specific points relevant to the ownership, then wouldn’t they be required under certain circumstances? If an analysis procedure is identified as “optional,” who has the responsibility of determining whether it will be used in a particular case?

A-1/2/3: What is meant by “subjective data”? The WDNR method does not collect “subjective” data. Is this what the appendix methods are intending to collect?

A-1/2/8: Here the “optional additional and supplemental methods” are referred to as an “optional substitute for the standard DNR survey protocol.” Which are they intended to be—additional and supplemental or a substitute? They are not adequate as a substitute.

A-2/2ff: As in the previous modules (or perhaps less so than the previous modules), this list of linkages to other modules consists largely of directives to determine who assesses what and to “discuss” particular issues. In the first case, these decisions should have been made during module development, and methods for the analyses should have been described in the appropriate modules. Deciding who should measure bank erosion does not constitute establishing a link between modules. Instead, it represents a deferment of module design to an inappropriate stage. In the second case, “discussion” does not constitute analysis. What decisions are intended to come out of the “discussion”? What conclusions?

A-6/1/7: It is not clear whether flooding is most severe locally below each of these confluences or below the downstream-most of the three tributaries. If the latter, then only McCready need be mentioned.

A-6/1/12: Another obvious potential cause is increased runoff. “Concentrated delivery of flow from multiple tributaries in a relatively short reach of channel” is not a likely cause, given that the frequency of tributaries in this reach is not much different from elsewhere and given that channel geometry tends to adjust to the long-term flow regime present. This factor would be likely to be important only if aggradation or altered flow regimes were present.

A-6/2/1: Data from this area demonstrate that accretion of banks is likely to be as important as increasing bed elevation. Rates of aggradation can be readily quantified by using datable vegetation as an “existing datum,” as described in the standard WDNR channel assessment module. Measurements of V* can also indicate where infilling of pools is likely to have occurred, as described in the standard WDNR channel assessment module. It is not clear why these methods are not adopted.

A-6/3/3: Contrary to the implication here, sediment budget and reservoir theory approaches are not the same thing, though reservoir theory can be used in developing a sediment budget.

A-7/3/1: “Long-term aggradation” certainly does not refer “to sedimentation effects within low gradient (<1%) reaches with relatively well-developed floodplains where sediment deposition typically occurs....” Instead, “long-term aggradation” refers to accumulation of sediment over a long period, irrespective of where that accumulation occurs. “Long-term aggradation” carries no implications for channel gradients or floodplain development. After all, steep alluvial fans are sites of “long-term aggradation.”

A-7/3/3: Freshwater Creek below Freshwater Park is here described as a low gradient reach where long-term aggradation typically occurs, yet Freshwater Creek below Freshwater Park (and above McCready Gulch) was described in the previous paragraph as being a steep reach in which aggradation is unlikely.

A-8/3/1: Why aren’t the methods described in the standard WDNR channel module going to be used to assess aggradation?

A-8/4/2: Several of these features do not “promote” sediment deposition, but instead can be a result of deposition having occurred (e.g., braided reaches, log jams, and low-gradient reaches).

A-8/5/1: Why are channel elevations to be compared to railroad grade elevations? Are there earlier data that describe their relationship in the past?

A-9/4/1: Note that tidal level and storms are not independent.

A-9/second to last/1: The volume of stored sediment cannot be determined accurately from bar dimensions in an aggrading reach. The extent of pool filling and increase in channel bed elevation must also be considered, as well as aggradation on banks.

A-10/2/2: How could the "sediment routing analysis" referred to at A-10/1/1 have been done without considering sediment storage on floodplains and channel banks?

A-10/5/1: The distribution of sediment sizes in bank and floodplain deposits must also be known before inferences can be drawn concerning sediment routing.

A-10/last paragraph: A pebble count does not take much more time than an "ocular estimate," and is a lot more dependable. Or is the intention simply to classify by whether the bed is dominantly sand, dominantly gravel, or dominantly cobble? If any D_{50} is to be estimated, a crude pebble count should be done. Otherwise, observer error and observer variability will need to be quantified to determine how accurate the "ocular estimates" are likely to be.

A-12/5/9: The assumption that sediment input and output are equal is not valid in an aggrading reach. In the present case, aggradation is a major issue, and the time period of concern is relatively short. The assumption that input equals output by definition excludes the possibility of aggradation.

A-12/6/3: Residence time cannot be calculated as storage volume divided by transport rate in an aggrading reach, since storage volume is increasing.

A-13/4/4: Note that watershed geology is not a management-related cause.

A-13/5/3: Under the circumstances, it cannot be assumed that only "low gradient and/or unconfined channel reaches" are experiencing deposition. Class III's and confined, moderate-gradient reaches have also been found to experience significant deposition in the area.

A-15/6/1: In contrast with the standard WDNR method, this supplemental method appears to include some analysis of Class III channels. This is a very important and useful addition.

A-17/1/1: How will the roughness contributed by bank vegetation and woody debris be assessed?

A-19/3/3: Actually, it will be more useful to perform the calculations for source reaches. The response reaches appear to be responding by aggrading due to increased sediment loads. The source reaches are presumably one source of the increased sediment loads due, in part, to increased transport capacities. Application to response reaches is likely to be less useful, since much of the aggradation is occurring by deposition of silts and sands on banks and floodplains.

A-22/Item 3, bullet 2: Why categorize by greater or less than 3' deep? Shouldn't this vary with channel size? Why not measure it?

A-23/Item 2: Data concerning the nature of the source stand will also be needed to interpret the information.

A-23/Item 3: It will be useful to date as many falls as possible (using ages of associated vegetation or other information) to calibrate the decay classes for the area.

A-27/2/1: Soil creep fuels landsliding and stream bank erosion; soil creep does not contribute sediment without the help of some other erosion process. Rates of soil creep thus cannot be added to rates of other processes.

A-27/3/1: Gully erosion can also be important in logged areas. This process does not appear to be considered in this procedure, except as a component of channel change (A-15/6/1). It will be necessary to describe the method to be used for quantifying this component of the sediment budget.

A-28/2/3: “Most efforts are obvious, however, phenomena such as gully erosion or skid trails that transform swales into defined channels may require coordination.” This sentence has appeared verbatim in multiple modules. The sentence tells us nothing about how this important process is going to be evaluated, and instead suggests that no actual plans have been made to evaluate it. The “coordination” that each module has appealed for might not be necessary if it is decided at this point how the process is to be evaluated and which module is to evaluate it.

A-28/3/3: Five to ten years into the future is not a sufficient period for cumulative effects assessment, since cumulative effects involves “reasonably foreseeable future actions.” These are specified for 120 years by the HCP/SYP.

Section: "Fisheries assessment"

General comments:

The fisheries assessment will not evaluate a major component of the watershed that is critical to the survival of anadromous salmonids and which is particularly sensitive to cumulative effects: the estuary. Furthermore, the module indicates that no evaluation of impacts associated with chemicals will be included, thus preventing adequate cumulative effects assessment for sediment or temperature, which generate synergistic impacts when combined with chemical impacts. Furthermore, the module apparently intends to assess habitat adequacy primarily on the basis of values established for other areas. In some cases where values have been adjusted for California conditions, the adjustments are invalid: a "key piece" of woody debris for a 50-foot-wide channel in the relatively diminutive hemlock-fir forests of western Washington must be 80 feet long and 27 inches in diameter, while a "key piece" for an identical stream in a redwood forest is defined to be over 6 feet long and 4 inches in diameter. As currently designed, the proposed module is clearly not capable of providing an adequate or valid assessment of fisheries issues.

Specific comments:

1/1/12: It is essential that a watershed assessment for salmonids include the estuarine portion of the watershed. Due to geomorphological, ecological, and climatic conditions in northwest California and due to past land-use history, habitat conditions in the estuarine portions of watersheds are extremely important to anadromous salmonids in the area. Estuary conditions are strongly influenced by upstream land uses, and past efforts to restore salmonid habitat in watersheds recognized to be cumulatively impacted by logging, such as the Mattole, have focused considerable attention on mitigations involving estuary restoration. It is simply not possible to assess cumulative effects on physical habitat of salmonids if the estuarine portion of the watershed is arbitrarily severed and ignored. The module later states that "The Fisheries Module will therefore consider all spatial and temporal scales appropriate to the fish species of concern so that incorrect assumptions about seasonal migrations into or out of the watershed are avoided" (2/2/17). An estuary is considered part of a watershed, and estuaries are important destinations for seasonal migrations within the watershed. Clearly, the relevant spatial scales are not being considered.

7/1/5: It is not appropriate to arbitrarily exclude certain on-going forest management activities of the landowner from the analysis on the basis that those activities "are not considered under PALCO's HCP," while other portions of the analysis include assessment of activities of other landowners on other ownerships, which were certainly not covered by the HCP. Impacts of urbanization and diking are being analyzed, despite the facts that the HCP has nothing to do with these activities and they are not even occurring on PALCO land. Yet an activity is ignored that is inextricably linked with the land-use activities specifically covered by the HCP, and which was considered in detail by the HCP/SYP documents (USFWS and CDF 1999b, p.3.14-1 through 3.14-19). These documents came to the conclusion that the company's use of herbicides and pesticides may have a significant impact on some aquatic species (USFWS and CDF 1999b, p.2-44). In addition, the Forest Practice Rules require that chemical effects be evaluated as part of a cumulative effects assessment, and chemical contamination produces cumulative impacts that are synergistic with those of altered temperature and altered sediment loading. It is not possible to adequately assess the cumulative effects on salmonids without considering the additive and synergistic effects of chemical contaminants, including those from road-surface effluent, potential spills, herbicide use, heavy equipment use, dust suppressants,

road surfacing compounds, fertilizers, and log-deck drainage. Chemical contamination from road runoff and heavy equipment use is an expected impact from activities covered by the Incidental Take Permit.

7/3ff: Note that climatic conditions are such in northwest California that it will also be necessary to evaluate the nature and distribution of thermal refugia in larger channels. These are very susceptible to cumulative impacts.

8/1/1: I'm not sure what is meant by "How do the above turbidity qualifiers in the WAU relate to turbidity within other managed and unmanaged forested watersheds within the region?"

9/6/1: "Factors that limit salmonid abundance can be accurately described as the sum of reach level habitat conditions across the WAU"—This does not appear to be valid. If salmonid abundance is limited by the presence of an impassable barrier downstream of the watershed, for example, "the sum of reach level habitat conditions" would have nothing to do with abundance. Whether a particular condition represents an incremental impact depends on what kind of habitat condition is present. If essential thermal refugia have been destroyed in a critical part of the watershed, the effect of this impact will be considerably more than an "incremental." Some kinds of habitat changes can carry disproportionate significance.

10/1/1: Note the distinction between "distribution" and "range" of a variable. Maintaining the value of a variable within a naturally occurring range is not sufficient—a 100-year flood is within the naturally occurring range of flow conditions in a watershed, yet maintenance of discharge at that level is clearly inappropriate. Instead, an appropriate distribution of the variable must be maintained.

12/1/5: The "Historic Conditions Module" does not appear to exist.

13/3/4: Water quality measurements for turbidity or suspended sediment would need to be made during storms. When is fieldwork intended to be carried out?

13/3/1: Winter distribution is different from summer distribution; some field work will need to be made during the winter. In particular, the validity of the "Class II" and "Class III" designations will need to be checked on the basis of winter distributions.

14/1/6: Note that channel type can change with morphological impacts. Habitat potential would need to be assessed for the channel type that would have been present under pre-management conditions.

16/1/4: Visual estimates during habitat surveys are notoriously irreproducible (Poole et al. 1997). Many of these variables can be easily measured. If they are important enough to be used in analysis, they either should be measured or the accuracy and precision of individual observers should be measured using a validly designed double-blind procedure and documented in the report. "Data" are not useful if their precision and accuracy are unknown.

16/2/3: How will "areas of degraded habitat" be recognized? What standards of comparison will be used? It is important that information concerning how these decisions will be made be included in the proposed method and be subject to review.

16/3/2: On what basis will fisheries specialists be diagnosing the causes of geomorphological and hydrological impacts? It will be necessary to have interdisciplinary representation on the module team to carry out this task.

16/4/2: How would “reach segments evaluated for physical habitat metrics be utilized to frame the surveys for impacts of water quality on fish abundance, diversity, distribution and habitat”? What is intended to be done? Does “frame the surveys” simply mean that water quality will be measured at the same location? Fish presence or absence is apparently to be measured at a time when water quality is not likely to be an issue, so how would the information be used?

17/1/list: Measurement of many of these attributes requires clear water so that they can be observed, so surveys are ordinarily carried out when visibility is good. A protocol that calls for turbidity to be measured during periods selected for low turbidity is not a valid protocol.

18/1/2: This passage implies that values presented in the Appendix A Table are to be used to assess habitat condition. Adjusting indices “where possible” for California conditions and geology is not sufficient. It is necessary that the method be adjusted for California conditions or results will not be valid.

A-1/Table: The table will require extensive modification to make it suitable for conditions in northern California redwood forests. For example, consider the entry at A-4/Table/row 1, below:

A-4/Table/row 1: The adjustments made to the WDNR process to make it fit conditions in redwood forests of northwest California—forests renowned throughout the world for the massive sizes of the trees—rather than conditions characteristic of the significantly smaller douglas-fir and hemlock forests of western Washington include redefinition of what is meant by a “key piece” of woody debris. In Washington, a 50-foot-wide stream channel is expected to require 80-foot-long pieces of wood of diameter greater than 27 inches to maintain conditions similar to those in old-growth channels. These values were not adopted in the proposed method. Instead, the proposed method asserts that a similar stream in the redwoods requires only 6-foot lengths of 4-inch diameter wood. This assertion is not valid.

A-4/Table/row 1: The reference LP-WA Manual 1998 is not listed in the bibliography.

Section: "Amphibian and reptile habitat assessment"

General comments:

The herpetofauna module would benefit greatly from editing. At this point, ambiguities in the text make it difficult to determine what is intended to be done. More information is required about how the channel classification system will be checked to correct the systematic misidentification of Class II channels as Class IIIs. More information is also needed concerning what "habitat diagnostic calls" and "vulnerability calls" are and how they will be made. A different protocol will need to be described for identifying potential habitat, since the method proposed here seems to depend on the presence of characteristics that will have been altered by past forest management activities; results will identify existing habitat, not potential habitat.

Specific comments:

2/2/5: Is water quality included as a physical habitat condition?

2/3/2: Why would differing habitat needs create a difficulty for analysis? It's not clear from the module how this consideration comes into the analysis at all.

2/2/11: Is there a paragraph missing here?

4/1ff: The critical question that the module needs to answer is "what is the effect of logging and associated activities on the species in question?" To answer this question, the module needs to answer some specific questions along the way: 1) what's the potential habitat for the five species? 2) what's the existing condition of the potential habitat? 3) is existing "good" habitat occupied? 4) if not, why not? 5) To what extent are Class II's misclassified as Class III's? 6) What is the likely effect of altered water quality on the five species?

4/section 2.1/item 2: The relevant question is "What is the distribution and relative abundance...," not "What is known about the distribution..."

4/section 2.3: There's no definition in the module of what is high- and low-quality habitat. If areas of both high and low quality are areas of special consideration, what's left?

5/1/1: "Regional species distributions and local conditions will be considered when developing the appropriate level of concern within the WAU": it's not clear what is meant here.

6/2/4: "Species distribution identification involves developing a map to show where different species and life history stages may be present in the watershed": not clear. The sentence seems to imply that species distribution will be determined by drawing a map.

6/2/8: Is the base map the topographic map? Why would the herpetofauna module be correcting a topo map? Or does this mean that the module will check the accuracy of the channel map? Or the stream classifications?

6/3/4: Which stream classification system is to be used? Or does this refer to the CDF stream classes?

7/3/2: What kind of stream typing is intended?

7/last paragraph/4: "Stream channel segments assessed in the field will be identified using the segment maps generated by the Stream Channel Module. In order to accomplish this, the following steps will be used: a) Conduct surveys....": This is not clear. Why would surveys be conducted to

identify the stream channel segments already assessed in the field? Are the stream channel segments assessed in the field the same ones that were assessed in the Stream Channel Module?

8/3/2: Riparian condition cannot be adequately evaluated using information from the riparian module.

8/2, 8/3: These other modules are not collecting information in parts of the channel network that may be very important to herpetofauna. The major herpetofauna issues are going to be in the Class III and Class II channels, but these channels are not included in fish habitat surveys and only a few are included in the channel surveys.

8/6/1: How does this survey relate to the survey just described in 8/1 to 8/5? Is this the same survey?

8/6/7: It sounds like this is to be a modeling exercise, not a survey. Why would 10 ac be assumed to be the cut-off point for Class II channels on all bedrock types? Part of the assessment protocol will need to be a method for determining the appropriate cut-off point between Class III and Class II. Since the mis-classified channels are not to be routinely surveyed under other modules, it will be necessary to select representative channels for field checking for the herpetofauna module anyway. Part of this protocol would reasonably be a field test of the classification protocol—or is that what this paragraph is describing? If this is the intended message of the paragraph, it would be useful to clarify the points. This field-check information would also be needed to assess the confidence of the revised classification system.

8/6/8: The appropriate information will not be available from the channel module, as it currently is written, since the standard procedure will not evaluate Class III channels. The optional supplement might, but it is not clear in which watersheds the optional supplement will be used.

8/6/14: What channel classification system is to be used?

9/7/2: Is it really intended that “all of the identified priority areas are included in the survey”?

9/last paragraph/4: Table 1 presents criteria that are easily impacted by management activities; thus these cannot be used to assess “potential” habitat.

10/Table 1: “Consolidated geology” is not an indicator of potential habitat. Some undisturbed areas on unconsolidated bedrock can also provide excellent habitat. However, the geology may indicate areas less sensitive to management-related impacts on herpetofauna, since unconsolidated bedrock is likely to produce more fine sediments when disturbed, thus impacting stream substrate.

11/Section 4.7: How does this survey relate to those described at 7/last paragraph/4 and at 9/1-6? Almost the same text is repeated here.

13/Section 4.9 ph.1: What “Habitat Diagnostic Calls”? What are these? How are they determined?

14/3/1: “LWD pieces per 100 meters” is not very useful; something must be known about the sizes and species, too.

14/last paragraph/4: What are “vulnerability calls”? How are they determined? How are they related to “Habitat Diagnostic Calls”?

Section: "Cumulative effects assessment"

General comments:

The introduction to the cumulative effects assessment module states that "The Habitat Conservation Plan for the Pacific Lumber Company (PALCO) requires development of a methodology to identify Cumulative Effects from land management activities" (1/1/1). This statement is not correct, as the requirement was for a method to "assess" such cumulative effects, not simply to "identify" them. The module, as presented, does not satisfy the requirements of the HCP. No methods for assessing cumulative effects are provided, and the cumulative effects that will be addressed are not identified.

The cumulative effects assessment methodology cannot be adequately reviewed until it is actually developed.

However, the module does describe an approach for identifying cumulative effects. The module indicates that issues will first be scoped, and that this scoping will result in distribution of cumulative effects assessment responsibilities among the various modules. The module implies that a major difference between the standard WDNR "synthesis" module and the rewritten module is that this up-front issue identification allows cumulative effects assessment to take place. The module also indicates that cooperative work between modules is an essential aspect of cumulative effects assessment. However, field crews report that fieldwork has already begun on at least one module for the Elk River watershed assessment. No scoping meeting has been held, so no cumulative effects issues have been identified. Module fieldwork is thus being carried out with no apparent recognition of the particular needs for cumulative effects assessment. Since modules are not even being carried out at the same time, no "dialog" between modules can be carried out, despite the fact that the "cumulative effects module" indicates that maintenance of inter-module "dialog" is a major function of the module. That the Elk River assessment is being carried as it is suggests that there is no operational difference between the standard WDNR approach and that proposed here. The proposed method does not seem to be being used.

Specific comments:

1/1/1: The HCP requires development of a method to "assess" cumulative effects, not simply to "identify" them.

1/1/3: The module indeed describes an "approach," but it does not provide the required "method."

1/2/3: The concept of cumulative effects does not necessarily imply a persistence of impacts over time, although this is frequently an attribute of the cumulative impacts that are of wide concern. Accumulations through space can be as important as accumulations through time.

1/2/8: An assessment approach that does not evaluate the cumulative impacts required to be evaluated under the Forest Practice Rules cannot be represented as providing a "comprehensive picture of the interactions between historical and current land management activities, hillslope processes and downstream effects on channels and aquatic resources." Furthermore, any procedure that does not evaluate the effects of "past, present and reasonably foreseeable future activities" does not evaluate cumulative effects, as defined under CEQA, NEPA, or the Forest Practice Rules. The proposed method indicates that the effects of certain activities (use of herbicides) will not be evaluated, and that some impacts of other activities (chemical contamination from hauling and yarding) also will not be evaluated, even though they have occurred in the past and are currently occurring. Any procedure that

evaluates only a subset of on-going forest management activities is not a “comprehensive” or adequate procedure.

2/6/4: “Evaluation of evidence will be used to assess the confidence...” How?

2/7/2: The module states that “The watershed resources to be evaluated are: 1) sediment effects; 2) water temperature effects; 3) large woody debris effects; 4) peak flow effects; and 5) stream channel condition” and implies that this statement represents “the language of California Forest Practice Rules.” However, not only are these items not “watershed resources,” but the list is not the one presented in the Forest Practice Rules. The Rules explicitly state that “Watershed impacts shall be based on significant on-site and down-stream cumulative effects on beneficial uses of water, as defined and listed in applicable Water Quality Control Plans” (CDF 1998 p.38). Furthermore, “Watershed effects produced by timber harvest and other activities may include one or more of the following: Sediment, Water temperature, Organic Debris, Chemical contamination, Peak flow” (CDF 1998 p.38).

2/7/4: What is the “biological resource assessment”? This has not been mentioned before. Is the “biological resource assessment” another name for the cumulative effects assessment?

2/7/6: The HCP/SYP states that various decisions will be based on the results of watershed analysis. It is therefore necessary that the results of watershed analysis provide an adequate basis for making those decisions. These decisions, being prescriptions for land-use activities, require the results of a valid cumulative effects assessment. Prescriptions for altering forest practices cannot be made unless cumulative impacts, as defined by NEPA, CEQA, and the Forest Practice Rules, are evaluated. If assessment of cumulative impacts (as defined under CEQA, NEPA, and the Forest Practice Rules) is deferred until individual THPs are planned, then prescriptions must also be deferred until that time. Valid prescriptions cannot be designed unless cumulative impacts have been evaluated.

3/1,2: These questions are not sufficient because they are concerned only with identification of cumulative impacts. The HCP/SYP requires a method for *assessment* of cumulative effects, and critical questions must reflect the goal of assessment.

3/2/1: The module indicates that cumulative effects of reasonably foreseeable future actions will not be evaluated, even though this is an essential component of cumulative effects assessments; this will be deferred to the prescription phase. Yet the module intends to determine whether aquatic habitat conditions will improve over time. This cannot be done unless the cumulative effects of reasonably foreseeable future activities are evaluated.

3/6/4: Cumulative effects analysis for individual THPs cannot “tier” off this assessment if this assessment is not designed to assess the cumulative effects that must be assessed for individual THPs.

4/1/1: The assessment approach does not clearly identify the cumulative effects that will be assessed during analysis: after having read the module, I still have no idea of what effects will be evaluated. We need a list of which cumulative effects are considered to be appropriate for evaluation and which are not. We also need a description of the methods that will be used to evaluate those cumulative effects.

4/1/4: Indicating that “it is important to identify the components and linkages” and that “One possible approach to accomplish this is by mapping the problem in a manner similar to that outlined by Ziemer (1998)” does not constitute a description of the method that will be used for cumulative effects

assessment. A statement of what is “important” and what is “possible” does not indicate what will actually be done.

4/1/5: “Once the suite of known issues and potential causal mechanisms are identified, the analysis process will be tailored to assess the potential identified cumulative effects”: This is a statement of intent rather than a description of a cumulative effects assessment method. Cumulative effects important in this region presumably were already “scoped” during the Freshwater watershed assessment, yet there is still no indication of which of those effects will be assessed. Since most of the work for the Freshwater assessment appears to have already been done, presumably some methods are already in use to assess the selected effects. These methods should be described.

6/1/4: This description of the restricted scope is not applicable to the “distinct cumulative effects assessment” required by the HCP/SYP. If the issues considered by the specified modules of the DNR method were thought to have been adequate for cumulative effects assessment, there would have been no need to require a “distinct cumulative effects assessment.” The HCP/SYP requires an assessment of “cumulative effects,” and guidelines for CEQA and NEPA, as well as the Forest Practice Rules, define “cumulative effects.” There is no indication in the HCP/SYP that this NEPA/CEQA document has changed the legal definition of “cumulative effects” to mean “issues covered by the DNR modules.” Such an intent on the part of the authors of the HCP/SYP would constitute misrepresentation. Valid decisions concerning the adequacy of the HCP/SYP will not have been possible if legally recognized terms were implicitly redefined in the document.

6/1/7: What is “time intensive research-level data-gathering”? The WDNR Level II approach requires that the necessary measurements be made to answer the questions posed, and there is no reason that the method presented here should have a lesser standard. Furthermore, the meanings of “time-intensive” and “research-level” are not clear: is mapping landslides on aerial photographs or drilling holes to describe the bedrock “time-intensive” or “research level”? Both of these data-gathering methods are widely used for research and take a lot of time, yet both are incorporated in the proposed method. What methods would be excluded on the basis of this proscription? What standards will be used to ensure that decisions concerning appropriate methods are not arbitrary or capricious?

6/6/1: There is no indication here that a method will be developed for analysis of each cumulative effect, only that an “appropriate approach (e.g., qualitative or quantitative)” will be selected. It will be necessary to actually describe the methods that would be used for assessment.

7/1/2: The only description of the actual assessment of cumulative effects is that “This step will assure that the issues and potential causal factors are evaluated through an interdisciplinary dialog. The watershed analysis will evaluate the most important components and links that affect the issues of concern...” This description does not indicate how the assessment will be carried out; a “dialog” does not produce an assessment.

7/1/8 and 7/2/1: The cumulative effects module here indicates that the methods to be used to evaluate cumulative effects will be developed by the individual module teams. This does not constitute a “distinct cumulative effects assessment” method. Further, cumulative effects specialists should design cumulative effects analysis procedures, not specialists in other disciplines.

8/Table 1, row 1: Contrary to the statement in the table, the Hydrology module will not evaluate whether forest roads contribute to detectable hydrologic change; instead, the module simply displays

the locations of high road densities without attempting to determine what the effects of those high road densities will be.

8/Table 1, row 1: What is meant by “detectable changes in the timing or magnitude of downstream runoff”? If “detectability” is defined as showing up in the rainfall-runoff modeling exercise, then the method presented in the hydrology module is guaranteed to demonstrate no “detectable” change, since the method is incapable of discerning the level of change expected. Furthermore, this was not a “critical question” in the hydrology module.

8/Table 1, row 1 and row 2: “Methods” is blank. Are there none?

8/Table 1, row 2: This is not a critical question in either module.

9/2/3: What is a “resource situation”? Is this being used to mean “cumulative impact”?

9/3/7: Note that the issue is whether a factor contributes to an already significant cumulative impact, not whether the factor itself causes a significant impact. Recall the definition of cumulative impact: “Cumulative impacts can result from individually minor but collectively significant actions taking place over a period of time” (CEQ Guidelines, 40 CFR 1508.7, issued 23 April 1971).

10/Table 2, row 1: Since the stated target loading levels are those present under pre-management conditions, “naturally sparse riparian stands” should not be considered a limiting factor. Instead, the percentages would be calculated for conditions *relative to pre-management conditions*. The point is not to load the system with woody debris but to sustain the appropriate level of woody debris.

11/1/2: Even if the module does not define threshold levels, it is required to provide the information necessary to develop an appropriate disturbance index. This requires that levels of cumulative impacts be related to levels of various activities.

11/1/6: The module cannot “determine future trends in resource conditions” if it will not evaluate the cumulative impacts of reasonably foreseeable future activities.

11/2: Item 4 is “Define watershed processes that will, over time, improve aquatic habitat conditions.” However, the paragraph referring to this item instead describes the need to identify desirable habitat conditions and to evaluate the effects of management practices. It is not clear how the paragraph relates to the heading; perhaps the problem is that the meaning of “watershed process” in this context is not clear.

12/1ff: The “causal mechanism report” is a concept taken directly from the WDNR process. This approach was designed to provide a linear, singular association between a land-use activity and an impact; it was not designed to address cumulative impacts. To address cumulative impacts, the concept would need to be turned around: a particular impact is affected by a host of direct and indirect mechanisms. Interactions between causal mechanisms also would need to be addressed. The proposed module indicates that “The causal mechanism reports are clustered around the analysis of each of the resource situations...and address the management factors.” This approach is not adequate for assessing interactions between mechanisms. The approach implicitly assumes that all cumulative effects are simply the combination of the individual effects evaluated using a standard WDNR approach.

12/5: “Where triggers overlap..., the causal mechanism report will refer to the first reference of the hazard and the impact associated with it to be addressed by prescriptions”: The meaning of this sentence is not clear.

13/2/4: The disturbance index also will need to reflect the potential for hydrologic change because sediment problems often are associated with changes in runoff.

13/2/5: The methods presented in the series of modules do not appear to be capable of providing the information required for modifying the disturbance index.

13/4/3: The 10-year limit for assessment is not reasonable because many potentially severe cumulative impacts may not manifest themselves over a 10-year time span. These impacts *cannot* be avoided if cumulative impact analysis is limited to a 10-year time frame. Furthermore, NEPA and CEQA definitions indicate that “reasonably foreseeable future actions” need to be evaluated. The HCP/SYP contains information concerning actions foreseeable over a 120-year period; acceptance of the SYP was in fact contingent upon actions being foreseeable over this period. If the Sustained Yield Plan does not represent reasonably foreseeable future actions, then it would seem necessary to amend the plan.

14/1/4: The cumulative effects module indicates that an additional cumulative impact assessment method will be developed in the future by the “prescription” team. Is it possible that the reason that no specific methods for cumulative effects assessment were described in the document currently under review is that the “distinct cumulative effects assessment” is to take place during the prescription phase instead of during the standard WDNR assessment? When will that document be prepared and released for review? If it is intended that the prescription-phase cumulative effects analysis procedure be based on information provided by the modified WDNR modules, it will be necessary to delay finalization of the modified WDNR modules until after the prescription-phase cumulative effects procedure is developed to ensure that the WDNR modules will provide the information necessary for the cumulative effects assessment procedure.

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