Review of the Final EIS/EIR and HCP/SYP for the Headwaters Forest Project

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Introduction: organization of comments

These comments are arranged to follow the pattern used by Reid (1998a) to allow convenient comparison with the original comments concerning the draft plan. Because of the dominating role played by alterations to sediment, woody debris and water regimes in the generation of cumulative watershed impacts, these components are discussed first. New information concerning the role of each is first described, if available, and the adequacy of provisions of the plan is then evaluated. Because the Final EIS/EIR is not complete, in that it postpones many of the most important decisions regarding management of cumulative impacts until after the plan is approved, I considered it necessary to assess available evidence to determine whether the likely outcomes of these decisions would adequately address the issues raised in the earlier comments (Reid 1998a). Many of these decisions are in the hands of state and federal agencies. The best available evidence for the agencies' stance on these issues is provided by their official responses to the issues raised. Where responses indicate denial of the seriousness of the issues, it can be assumed that agency personnel might have little motivation to establish adequate protection from impacts they do not believe exist. Sub-sections of this review are thus devoted to evaluation of the agencies' replies to comments concerning issues related to sediment, woody debris, and water.

The sections on individual components are then followed by a general assessment of provisions for managing cumulative watershed impacts; this section follows the same outline as those for individual components. Finally, observations were made during the review process concerning the procedure itself; these are described in the section following the discussion of cumulative impacts.

Because of the importance of the official responses to comments as an indicator of likely future agency actions, I evaluated each response to issues raised by the earlier comments (Reid 1998a). This discussion is presented in Appendix 1. Other documents have become available since 16 November 1998 which open additional discussion concerning issues raised by Reid (1998a); these documents are discussed in Appendices 2-5.

Sediment

Original concern 1. The provisions of the draft HCP are not sufficient to prevent continued high rates of sediment production in the affected watersheds. Several important sources of sediment (e.g. surface erosion near the smallest channels, logging-related landslides on planar slopes, and gullying of small channels) are not adequately considered by the plan, and proposed strategies for controlling the sources that are considered (e.g. logging-related landslides on inner gorges and road-related failures) are not adequate or timely.

New information regarding sediment-related concerns

After these concerns were raised, additional information became available that provides increased understanding of local issues regarding sediment production. Further analysis of the Bear Creek landslide data provided by Pacific Watershed Associates (PWA 1998a) allowed a direct test of the influence of logging on slope stability; PWA released a companion report describing landslide distribution in the neighboring Jordan Creek watershed (PWA 1999); and turbidity monitoring data became available that allow comparison of existing conditions to those required by water quality legislation.

Relationship between logging and landslide frequency

Considerable discussion concerning the influence of logging on landslide distribution had followed release of the Bear Creek report (PWA 1998a), and allegations were made that a simple “association” between landslides and logged areas was being confused with a causal relationship that would indicate that logging caused the increase in landslide rate observed on the logged slopes (PWA 1998b). Data presented in the original report allow resolution of this issue.

In the first step of analysis, the association between landslide distribution and recently logged areas is easily shown by a chi-square test to have a less than 1 in 10,000 chance of being produced randomly. If the distribution of recently logged areas can then be shown to be characteristic of the rest of the watershed in terms of its potential to generate landslides, then causation can be inferred: the landslides are where they are because that is where the logging occurred.

That lands on which the recent logging occurred are characteristic of the remainder of the watershed was first evident from examination of aerial photos, topographic maps, and geologic maps. This conclusion was supported by the DMG review of the Bear Creek report: “For assessment purposes, there is no substantial difference between harvested areas in subwatersheds with channel inner gorges and the subwatersheds where harvesting has not occurred within the past 15 years” (Spittler 1998, p.3 par.5), and “The functional homogeneity of the geology and geomorphology of the Bear Creek watershed allows for a more simplified assessment of the role of timber management on landslides triggered by the New Years 1996-97 storm” (Spittler 1998, p.3 par.6). The means to statistically test the representativeness of the recently cut lands is provided by the map of landslides present in 1966 (PWA 1998a, Map 3), when essentially the entire basin had been logged. If the landslide frequency in the 37% of the watershed that would later be cut in the 15 years preceding 1997 is the same as that in the 63% of the watershed that would not be cut again by 1997, then the two areas are functionally the same with respect to the potential for landslide generation. A chi-square test for landslide distribution in 1966 demonstrates that landslide frequencies did not show a significant difference between the two areas at the 95% confidence level. Thus, the difference in later landslide rates can be attributed to the treatment variable, the logging.

Landslide distribution in Jordan Creek watershed

The second new contribution to understanding of landslide distribution came with the long-awaited release of the PWA report on landslide distribution in Jordan Creek watershed (PWA 1999), located immediately north of Bear Creek watershed. This event, however, was marked by considerable confusion, because the actual contents of the report were found to directly contradict the results that had been quoted earlier by PWA (1998b), Dr. D. Opalach (1998) of Pacific Lumber Company, and USFWS and CDF (1999). Between 10 November 1998 and 29 January 1999, the results were said to demonstrate that in Jordan Creek, most recent landsliding was associated with older logging, and that recently logged slopes showed comparatively low rates of landsliding. This “result” was the primary piece of evidence used to assert that results of the Bear Creek report could not be employed to inform management decisions in nearby areas with similar geology, topography, land-use history, and climate: if a neighboring watershed shows the opposite pattern as Bear Creek, then it is clear that results cannot be generalized.

When the report was finally released, however, it became clear that the actual results of the study showed a pattern very similar to that measured in Bear Creek: "In Jordan Creek, 60% of the landslides and 77% of the landslide sediment delivery came from 50% of the watershed which had been harvested within the last 15 years" (PWA 1999, p.27). In other words, rates of landslide sediment delivery from recently logged lands in Jordan Creek watershed are 3.3 times higher than those from partially recovered forest (calculated as the ratio between (0.77/0.50) and (0.23/0.50)). Clearly, results in Jordan Creek support those from Bear Creek, removing the argument that had been used to prevent Bear Creek results from being used to inform management decisions on similar lands nearby.

A remaining concern is to determine why the misrepresentation of the report's results appeared just in time to be included as commentary on the draft EIS/EIR, and why the actual results were not released until just after the final EIS/EIR was published. It is highly unfortunate that the misrepresented results were the only ones made available to inform the decisions incorporated into the final EIS/EIR.

Measurements of turbidity in project-area streams

Data collected by the "Watershed Watch" monitoring program in Humboldt County provide a growing record of levels of turbidity found in project-area streams. In particular, repeated monitoring of tributaries in Humboldt Redwoods State Park and along Shively Road allow comparison of turbidity relationships in unlogged tributaries and in tributaries that have undergone different levels of logging-related disturbance. Details of the study are described in Appendix 6. Roads were not being used for hauling during the measurement period, and landsliding had not been noted as a problem in the area; these conditions are expected to reflect the "best-case" outcome of the FEIS/FEIR. Results indicate that salmonids in at least part of the project area are subject to turbidity levels likely to induce chronic impacts for a large portion of the winter period, while salmonids in unlogged watersheds would not be subject to these levels for more than a few days each year. Results also indicate that these high turbidity levels are associated with both high road densities and high proportions of watersheds logged (Appendix 6).

In particular, results show that turbidity rating curves for logged tributaries are significantly higher than those for undisturbed and less-disturbed tributaries on similar rock types (Figure 1). Basin Plan objectives call for turbidities of no more than 20% over background natural levels (FEIS/FEIR p.3.4-12, NCRWQCB 1988), but the shift in the turbidity rating curve evident in partially logged tributaries indicates that background levels are being exceeded by more than 400%.

New provisions of the FEIS/FEIR for sediment, and remaining problems

The FEIS/FEIR has strengthened several provisions to reduce levels of sediment production expected from implementation of the SYP/HCP. In particular, with respect to landsliding, a considerably larger area (encompassing areas of "extreme," "very high," and "high" mass wasting potential, rather than just the "extreme" area, as before) is now subject to geologic evaluation through the mass-wasting avoidance strategy; inner gorge slopes can be accorded protection irrespective of their length; and provisions have been made to allow consultation by landsliding experts who do not necessarily hold a California geology license. In addition, no-cut buffer strips along Class 3 channels will decrease the incidence of failure at these sites, and will also help to reduce the risk of gullying. Provisions to halt traffic when overland flow is generated from road surfaces will reduce the impacts expected from road-surface erosion, and no-cut buffers along Class 3 channels will reduce the potential for inputs of logging-related surface erosion. Overall, the new provisions *have the potential* to greatly reduce the risk of sediment inputs from future timber-management activities in the affected watersheds. Three important areas of concern remain, however.

First, the “biologically significant” level of landslide risk remains to be defined in the future. Until we know what the threshold of significance is that will be used by the plan, we have no basis for determining whether the mass-wasting avoidance strategy will be capable of reducing the risk of landsliding to levels that will not harm coho. It makes no difference how carefully or how widely geologic examination of potentially unstable slopes is carried out if the design threshold for landslide risk allows high rates of landsliding to continue. It is essential that design of the threshold of significance be subject to thorough independent review.

Second, many of the strengthened provisions can be reduced to the original levels of the Draft EIS/EIR through watershed analysis and adaptive management plans. Only if the wildlife agencies are fully committed to reducing levels of impact and actively employ their oversight authority for watershed analysis and adaptive management will the inherent latitude in the plan earn credibility. In particular, it is essential that deviations from the default prescriptions be judged in their own right, and not as part of mitigation trade-offs. For example, a package that trades cutting in a riparian zone for additional road repair defeats the intent of the provisions; cutting in riparian zones would need to be prohibited unless that cutting can be demonstrated to itself promote habitat recovery for the range of organisms in question. If this approach is not taken, mitigations will cumulatively fall short of off-setting the full range of impacts that will accrue from continued alteration of riparian function, resulting in long-term cumulative impacts; no single strategy for mitigation can off-set the full variety of impacts ensuing from such an action. Further, burden of proof must be on the shoulders of those who would request decreased levels of protection relative to those accorded by the default prescriptions. If these defaults are routinely reduced after watershed analysis or in adaptive management plans, then, as described by Reid (1998a), the plan will not have been successful in meeting even the objectives stated by the plan itself, and the plan’s assumptions concerning level of harm to threatened species will be falsified.

Third, 2175 acres of logging have been excluded from some provisions of the FEIS/FEIR, but there is no indication in the FEIS/FEIR of where that logging will occur. If those acres are on lands that would be subject to mass-wasting concern, then exemption from the provisions of the FEIS/FEIR that are considered necessary to prevent harm to coho could lead to excessive habitat damage in the affected channel reaches. Once minimum criteria for protection of a threatened species are established through drafting of the HCP, it is not reasonable that exemptions should be granted that diminish the level of protection below the minimum considered necessary.

Assessment of significance for sediment impacts

Because the definition of “biological significance” for landsliding has been postponed until after the plan is approved, the extent to which the mass-wasting avoidance strategy will be required to reduce the risk of landsliding is not yet known. Therefore, it is not yet possible to assess the level of risk of significant sediment-related impact. Despite this problem, the FEIS/FEIR provides such an assessment.

However, local data provide additional information that allows a more accurate assessment of risk than would ordinarily be possible, given the uncertainty concerning what the provisions of the plan will actually be. The risk assessment indicates that “the threshold of significance for sediment delivery to streams is based upon the reduction of management-related sediment such that discharge to streams will not degrade or impede the recovery of beneficial uses” (USFWS and CDF 1999, p. 3.4-44), and elsewhere indicates that the threshold of significance is defined according to Basin Plan objectives, which call for turbidities of no more than 20% over background levels. Documented shifts in the turbidity rating curves for partially logged tributaries (Figure 1) demonstrate that in at least part of the project area, thresholds of significance are already being significantly exceeded under conditions that reproduce those expected from a best-case implementation of the FEIS/FEIR. Any further contribution of sediment would thus constitute an addition to an already significant cumulative impact. The "risk" of exceeding the threshold of significance in these areas is not merely "high"—exceedence has already been documented. The FEIS/FEIR conclusion that the plan holds a low risk for exceeding threshold of significance for sediment loads and turbidity is thus in error.

Although the passage is not clearly written, the FEIS/FEIR appears to argue that "background" levels refer to the levels of turbidity present immediately prior to a given project: "Background turbidity would include some continuing effects of logging activity, grazing, and burning during a particular storm prior to any timber harvest, while natural turbidity would be the turbidity that occurs during a particular storm before any timber harvest or other ground-disturbing management activities" (p.3.4-44). Such an interpretation, however, conflicts with the intent of the Basin Plan objectives for recovery of beneficial uses (NCRWQCB 1988) and with Forest Practice Rule provisions for control of cumulative impacts. Under this erroneous interpretation, the more impacted a watershed is, the more the level of impact can be increased. For example, if an activity is initiated in an as-yet-undisturbed watershed with a turbidity level indexed at 1.0, then that activity can increase the index level to 1.19 and remain in compliance with the Basin Plan. Under the erroneous assumption that "background" refers to "ambient at the time of activity initiation," the next activity could increase the index by 0.23 (=1.19*0.19), to arrive at an index of 1.42; the subsequent activity could raise the level by 0.27 (=1.42*0.19) to reach 1.69, and so on. Under the CEQA definition of cumulative impact, in contrast, the *impact of the project when added to other closely related past, present, and reasonably foreseeable probable future projects* needs to be evaluated; and under NEPA, the *impact of the action when added to other past, present, and reasonably foreseeable future actions* must be evaluated. In both cases, the cumulative impact level before the third project is initiated would thus be 42% over the background level.

The Water Quality Control Plan for the North Coast Region, in fact, makes the definition of "background" very clear: "Turbidity shall not be increased more than 20 percent above naturally occurring background levels" (NCRWQCB 1988, p.III-4, par.1). The FEIS/FEIR, by redefining "background" to include "some continuing effects of logging activity, grazing, and burning during a particular storm prior to any timber harvest," materially misrepresents the requirements of the Basin Plan.

Adequacy of response to review comments

In general, responses indicated that the agencies do not consider data showing significant increases in landslide sediment inputs following logging in Bear Creek and North Fork Elk River watersheds to be compelling evidence that logging causes landslides (Appendix 1). Agency personnel also do not appear to be willing to consider quantification of relative rates of landsliding on logged and unlogged lands as a method for calculating risks associated with logging. This is highly unfortunate, because without such a method it will not be possible to evaluate likely outcomes of prescriptions resulting from watershed analysis. Such an approach would have been necessary if a valid CEQA assessment of impact significance and risk were to have been carried out. As long as decision-making continues to rest primarily on qualitative "professional judgment" and agency authority, and as long as the importance of the accumulating evidence continues to be minimized, little change in cumulative impact management is likely. Progress in problem-solving cannot occur while the existence of the problem is still being denied.

Woody debris

Original concern 2. The provisions are not sufficient to maintain the composition, size, and quantity of woody debris necessary to protect downstream channels from disruption by debris flows or to provide adequate habitat for aquatic biota. The HCP uses 100-year-old trees as the basis for calculating wood inputs in old-growth redwood forests. This is not appropriate, as old-growth redwood trees are considerably larger than 100-year-old second-growth trees.

New provisions of the FEIS/FEIR for woody debris, and remaining problems

New provisions in the FEIS/FEIR *have the potential* for greatly reducing the level of impact that would occur due to long-term changes in the composition, size, and quantity of woody debris resulting from implementation of the plan. Default no-cut buffers for Class 1 and Class 2 streams have been widened to 100 feet and 30 feet, respectively, and 30-foot buffers are now required on Class 3 streams. However, these provisions hold only until watershed analysis has been completed for the watersheds. Watershed analysis can result in recommendations to decrease no-cut buffer widths back to the levels that the agencies had found to be insufficient in the draft EIS/EIR. Wording in the FEIS/FEIR suggests that the wildlife agencies control the decision of whether default buffers will be modified after watershed analysis or through adaptive management plans, but close oversight of the pattern of ensuing decisions will be required to ensure that the agencies are taking long-term cumulative impacts into account as each decision is made.

The short-term prospect for impacts associated with changes in woody debris loadings is even more worrisome. Data in the FEIS/FEIR (p. 3.7-38) show that currently only 29.8% of the riparian forest on the ownership is “functioning” as required for sustaining adequate habitat for coho salmon; the remainder will require 30 to 100 years to begin to function as required, and, even then, the size of in-falling wood will be substantially smaller than under old-growth conditions. Only 5.4% of the riparian forest (i.e., 18.1% of that which is “functioning”) is capable of introducing wood characteristic of old-growth conditions, and is therefore functioning in the way necessary to sustain channel conditions characteristic of the ones in which the salmon evolved. The only other remaining source of wood characteristic of old-growth conditions are the residual old-growth trees that currently remain within falling distance of the channels. These will continue to be cut if they are located outside of the riparian management zones (but still within falling distance of the creek), and a proportion of those within the 170’ riparian management zones will also be cut. As a result, the input of properly functioning wood—wood large enough to perform the range of functions required by the original channel dynamics—will continue to decline through the life of the plan. This cumulative impact could easily be avoided if remaining residual trees within falling distance of a channel were left uncut until the recovering riparian zones attained the characteristics of old-growth stands. Without such a provision, implementation of the plan will contribute to a significant increase in the severity of an already significant cumulative impact.

In addition, 2175 acres of logging have been excluded from some provisions of the FEIS/FEIR, but there is no indication of where that logging will occur. If the exempted 2175 acres account for destruction of the functionality of a portion of the remaining 5420 acres of riparian land that is classified as “functioning” (or, worse, of the 976 of those 5420 acres that are actually capable of introducing wood characteristic of old-growth conditions), then this exemption will disproportionately increase the severity of existing cumulative impacts to habitat necessary for coho salmon. Exemption from the provisions of the FEIS/FEIR that are considered necessary to prevent harm to coho could lead to excessive habitat damage in the affected channel reaches. It is not reasonable that, once minimum criteria for protection of a threatened species is established through drafting of the HCP, exemptions should be granted that diminish the level of protection below the minimum considered necessary.

Assessment of significance for woody debris impacts

Levels of protection for woody debris inputs were calculated as averages for entire Hydrologic Units, rather than as the proportion of the stream system adequately protected within a Hydrologic Unit: “The HUs with higher values can be attributed to protection from other no-harvest areas within the project area” (p. 3.7-64). This implies that the “overall” high protection may simply reflect the existence of certain areas that will continue to function adequately. From a fish’s point of view, only the portion of the system that is functioning is particularly relevant, so fully protected areas should not be averaged in with “normal” areas. Furthermore, despite the comments by Reid (1998a), which demonstrated that using a 100-year-old tree to represent characteristics of old-growth trees was inappropriate and which also provided corrected calculations, the standard of comparison still appears to be based on the characteristics of a 100-year-old tree. The proportional level of protection reported in the FEIS/FEIR thus significantly overestimates the actual level of protection.

Adequacy of response to review comments

The official response to the 16 November comments by Reid (1998a) concerning woody debris followed the same pattern as that for sediment, as described in the previous section. It is worrisome that the state agency response again denied the existence of the problem, asserting that even though the plan stated that it would achieve 75.2% of old-growth woody debris input to Class 1 streams, if the plan’s provisions actually *prevented* achievement of that level the agencies did not consider it a problem (Appendix 1). In this case, the plan’s error resulted in a significant overestimate of the level of protection afforded by the plan to habitat critical to the survival of coho salmon.

As was the case with the state agency response, the federal response also down-played the fact that the plan had been falsely represented as providing 75.2% of old-growth woody debris input to Class 1 streams. When the disparity between 100-year-old second-growth and old-growth redwoods was pointed out, the problem was “solved” by changing the standard rather than by changing the calculations: the agency’s standard (with respect to this property) for “adequate” habitat conditions with respect to woody debris loads is now the woody debris conditions present in a 100-year-old second growth forest. However, this solution does not make biological or physical sense: reference conditions for woody debris loads are uniformly intended to be those for undisturbed channels rather than for those surrounded by second-growth of an arbitrarily defined age (see, for example, USDA and USDI 1994). That the agencies would consider the error to be inconsequential suggests that agency personnel are not likely to be adequately diligent in supporting the new default no-cut buffer widths in the face of requests to reduce those widths during watershed analysis or in the development of adaptive management programs. A plan that includes latitude for adjustment through watershed analysis or adaptive management will only be effective if agency personnel believe in the importance of sustaining healthy aquatic habitat. If agency officials do not believe those conditions are necessary, agency officials will not make the decisions required to achieve those conditions.

Table 1: Calculated daily interception loss for the storm of January 1995 at Eureka.

Date	Rainfall (inches)	Interception loss (percent)	Interception loss (inches)
5-Jan	0.32	31.6	0.10
6-Jan	0.40	29.8	0.12
7-Jan	0.51	27.9	0.14
8-Jan	1.43	21.1	0.30
9-Jan	1.97	19.4	0.38
10-Jan	1.01	23.2	0.23
11-Jan	0.57	27.1	0.15
12-Jan	0.37	30.4	0.11
13-Jan	0.92	23.8	0.22
14-Jan	0.49	28.2	0.14
Storm total	**7.99**	**23.8**	**1.90**

The FEIS/FEIR continues to base calculations of woody debris inputs on an invalid characterization of old-growth redwood dimensions, and continues to use data from an Oregon Douglas-fir forest rather than available data from a California redwood forest. These errors in the draft EIS had been pointed out in comments by Reid (1998a).

Water

Original concern 3. The provisions are not sufficient to prevent pervasive hydrologic changes due to the maintenance of immature vegetation cover over large portions of the watersheds in question. This will result in continued increases in peak flows. The HCP does not consider the impacts of hydrologic change, despite the fact that much of the concern over present practices on the ownership focuses on reported increases in flood frequency and magnitude.

New information regarding water-related concerns

Several kinds of new information have become available since the original concern was stated. Some of this information sheds further light on the mechanisms of hydrologic impact, while other information details the magnitude of the impact now being experienced in Freshwater Creek watershed.

Measurements of foliage interception loss at Jackson State Forest

First, as reported in the official response to comments by state agencies (Appendix 1), a study is being carried out on the Jackson State Demonstration Forest to measure the magnitude of foliage interception loss during rainstorms in coastal second-growth redwood forests. The response to comments quoted results from several storms that occurred during November and December of 1998: a 24-hour rainfall of 3.34 inches resulted in a mean interception loss of 16.8%, while three storms of about 1 inch in 24 hours resulted in losses of 21.4%, 23.7%, and 24.8%.

These values are for 24-hour rainfalls, rather than for multiple-day storms. Two of these 24-hour rains occurred over a single 48-hour period encompassing parts of three calendar days during

Table 2. Stand characteristics and interception loss in Appalachian white pine stands of various ages (total annual rainfall: 80 inches)

	Stand age		
	10 year	35 year	60 year
Average basal area (ft^2/ac)	76	120	153
Average height (ft)	20	57	74
Annual interception loss (in)	12	15	21
Annual interception loss (percent)	15%	19%	26%
Percent of loss for 60-yr stand	58%	73%	100%

which additional rain fell, and thus could alternatively be tabulated as a 1.97" storm with a total interception loss of 26.2% for the 3-day storm. Multi-day rainfall interception relationships are not expected to be the same as those for 24-hour rainfalls. Most flood-generating storms encompass multiple days of rain, and flood magnitudes reflect not simply the intensity on the heaviest rain day, but a measure of the accumulated rain during the storm. The storm of January 1995 in Eureka provides an example of this pattern (Table 1, columns 1 and 2): an 8-inch storm occurred over a 10-day period wherein the largest daily rainfall was 1.97 inches. This storm caused flooding along the downstream reaches of Freshwater Creek.

In the case of the January 1995 storm, the data cited in the official response (16.8% loss for a 3.34" day and 21.4%, 23.7%, and 24.8% for 1" days) were regressed using a log-log transform to develop a relationship between daily rainfall and percent interception loss, with the form of the equation assumed to be the same as that found to apply to other available data sets; this relation can be refined as more data become available. This relationship was then used to calculate the daily interception loss for the 1995 Eureka storm (Table 1, columns 3 and 4). Calculated losses varied between 19.4% for a 2-inch day and 31.6% for a 0.32-inch day, but the calculated overall interception loss for the storm was 23.8%. These results suggest that storm interception losses for second-growth redwood forests in coastal California are of similar magnitude to those for similar climates in New Zealand, and which provided the basis for the calculations presented by Reid (1998a).

Rates of hydrologic recovery

In the calculations presented by Reid (1998a), a period of 15 years was assumed to be necessary for full hydrologic recovery after logging, but it was noted that this period would underestimate the actual duration likely to be required. Actual levels of impact were thus likely to be higher than those calculated.

Helvey (1967) presents data which suggest the actual duration needed for recovery in Appalachian white pine forests (Table 2). In this case, if stands are assumed to be fully recovered in 60 years, then they are only 73% recovered after 35 years; at 10 years, recovery is only 58%.

A second data set provides an indication of a potential basis for calculating recovery times for redwood forests. The report by Rains (1971) included data not only from 80-year-old redwood stands with basal areas of 170 to 240 ft^2/ac, which were described in the earlier comments by Reid (1998a), but also from two stands with considerably lower stocking rates (90 and 110 ft^2/ac). Rains (1971) noted that there is a relationship between basal area and percent of rainfall intercepted. Interestingly, the data from Helvey (1971) plot on the same curve (Figure 2), although data from Appalachian white pine forests are not expected to represent either forest or climate conditions characteristic of

Table 3. Crest stages and recurrence intervals for recent floods along Freshwater Creek

Cross section	Location	21 November 1998		6 February 1999	
		stage (ft)	RI (yr)	stage (ft)	RI (yr)
upper	above Graham Gulch	bankfull	2?	<bankfull	<2?
XS-7	upstream of aggradation	69.2	7	68.9	6
XS-5	above Little Freshwater	49.1	100	48.1	35
XS-3	area of primary flood concern	38.5	70	37.1	35

California redwood forests. It should be noted that the interception percentages are with respect to the average interception during the analysis period rather than in terms of percentage interception for individual storms. Because of this, the results provide data relevant for assessing hydrologic recovery rate rather than for assessing per-storm losses.

Data from Lindquist and Palley (1963) were then used to estimate basal area for redwoods as a function of stand age (Figure 3). Combining the two relations then allows an estimate to be made of percentage hydrologic recovery as a function of stand age. For Freshwater Creek, hydrologic conditions characteristic of 80-year-old and older stands had been found not to present flooding problems. If interception rates for 80-year stands are taken as indicating “fully recovered,” then 50-year stands are found to be approximately 89% recovered and 20-year stands only 56% recovered (Figure 4). These results suggest that the 15-year recovery period assumed during the calculations detailed by Reid (1998a) significantly underestimates the actual time required, so impact levels calculated in that report underestimate the actual levels of impact likely to be present now and in the future.

It should be noted that the FEIS/FEIR itself describes hydrologic recovery to be slow (p. 3.4-22), citing a study that reported little peak flow recovery after 30 years in western Oregon and one that reported 50% hydrologic recovery in 25 years. It should also be noted that the above calculations are for recovery toward conditions characteristic of 80-year-old stands; complete recovery to old-growth-conditions would take considerably longer.

Flooding patterns and aggradation along Freshwater Creek

Five days after the original comments were submitted (Reid 1998a), a storm occurred which generated extensive flooding along Freshwater Creek. The maximum stage was observed during the peak at each of the cross sections that had been measured in 1975 by the Army Corps (USACE 1975) and remeasured in 1998 by CDF (Cafferata and Scanlon 1998). These stage heights were measured to within an estimated ±0.3 ft the following day using a hand level. Figure 9 in Reid (1998a) allows calculation of the expected recurrence interval for the measured stage heights at each of the cross sections, given conditions present in 1975 for XS-3 and XS-5. Recurrence interval for XS-7 is calculated using the 1998 cross section to account for the incision measured at this location. Results indicate that a flood with a 7-year recurrence interval at the uppermost cross section (located upstream of the aggraded reaches) had achieved stages characteristic of 100-year and 70-year floods at the middle and lower cross sections, located along aggraded reaches (Table 3). Observations upstream of the Graham Gulch confluence demonstrated that flows did not even go over-bank along that reach. Total storm rainfall measured at gauges in Freshwater watershed was on the order of 5 to 6 inches (Terry Roelofs, personal communication 11/22/98; Gary Sack, personal communication 11/22/98).

Flooding again occurred along the lower reaches of Freshwater Creek during a relatively minor storm in early December, but crest stages were not measured. In this case, too, flooding did not occur upstream of the aggraded reaches.

A moderate storm on 5-6 February 1999 again triggered extensive downstream flooding. In this case observations were made during the falling limb of the flow. Minimum possible crest stages were measured for XS-3 and XS-5 (i.e., the highest level of definitive evidence of flooding, such as silt deposits or rafted debris on the ground), and maximum possible crest was measured for XS-7 (i.e., highest level of non-definitive evidence of flooding, such as rafted debris on standing vegetation). Crest stage measurements show the same pattern as during the storm of 21 November (Table 3): a flood that did not exceed bank-full upstream of Graham Gulch produced no more than a 6-year flow at XS-7 and greater than 35-yr flows at XS-5 and XS-3.

Clearly, the distribution of flooding relative to that characteristic of 1975 conditions indicates that flood frequencies have increased markedly along the lower reaches of Freshwater Creek. This is the area where aggradation has been measured, and it is the reach fed directly by runoff from some of the most intensively logged tributaries in the watershed. Thus, this is the reach where hillslope hydrologic change and aggradation are most likely to interact to increase flood frequency.

Note that none of these calculations are contingent upon data from the 1998 remeasurement of the cross sections except for the adjustment of the recurrence interval for XS-7; without this adjustment, results would show an even larger disparity between upstream and downstream recurrence intervals.

Observations immediately after the floods of 21 November 1998 and 6 February 1999 also disclosed magnitudes of aggradation at locations on channel banks and floodplains along downstream reaches of Freshwater Creek. Depths of 5” to 14” of sand and silt were deposited on the south bank of Freshwater Creek approximately midway between the elevation of the terrace and that of the thalweg at a location approximately 400 ft upstream of XS-3. During the 6 February flood, an additional 1” to 2” of sand and silt was deposited at this level.

The terrace surface at the Howard Heights bridge also exhibited sedimentation during the 21 November flood. At this location, flow was approximately 4.5 feet deep at the center of the major flow strand across the surface, and 1” of silt had accumulated among the grass. In the area of XS-5, approximately 0.5” of silt accumulated at an elevation 2’ below the crest stage during this storm. Approximately 2” to 4” of sand and silt accumulated at XS-7 5.5 feet below the crest (i.e., at an elevation approximately half way between thalweg and terrace surface), while 1 to 2” accumulated 2.5 feet below the crest stage.

The observed depths of aggradation suggest that channel constriction has progressed markedly over the 1998-99 winter, resulting in additional reduction of cross sectional areas by on the order of 2% to 6% relative to cross sections measured in 1998. Thus, each flood contributes to an increase in flood hazard from subsequent floods by depositing sediment to further reduce flood conveyance.

New provisions of the FEIS/FEIR for water, and remaining problems

The FEIS/FEIR does not have new provisions for addressing flooding-related concerns, except insofar as the disturbance index is now to be calculated for “Hydrologic Units” rather than “Watershed Assessment Areas.” These Hydrologic Units are the size of Freshwater watershed, and, as described in a following section, the disturbance index is not sensitive enough to identify existing cumulative impacts in Freshwater watershed. The FEIS/FEIR indicates that the standard disturbance index threshold of 20% can be modified through watershed analysis, but there is no provision for hydrologic analysis during watershed analysis. Therefore, is no mechanism through which such an adjustment can be made. Further, the disturbance index continues to use the assumption that recovery from any influence of logging is complete after 10 years. This assumption is clearly unfounded, given the rates of recovery calculated above and those cited by FEIS/FEIR itself. Provisions are thus not adequate to address the concerns that had been raised in the earlier comments (Reid 1998a).

Assessment of significance for water impacts

The Final EIS/EIR evaluates the effect of the SYP/HCP on flooding impacts by first identifying a threshold of significance and then by evaluating the likelihood that the plan will exceed this level.

The threshold of significance defined for flooding is “the likelihood of changing the recurrence interval of a storm from five years to two years” (p.3.4-32), but no rationale is provided for how this level of impact was selected. First, I assume that “storm” is a misprint, and that any threshold of significance for peak flows would be referring to the peak flow rather than the storm. Second, Figure 7 in Reid (1998a) suggests that a change in the frequency of a 5-year flood to a 3.5-year flood can be accompanied by a shift in a 100-year flood to a 15-year flood. This would certainly be considered “significant” to those repeatedly inundated and to those who would need to alter the FEMA 100-year floodplain maps, but this would not be recognized as a problem by those designing this EIS/EIR. Clearly, the threshold of significance is not appropriately defined.

The risk of exceeding the threshold of significance is established primarily through assertion: “In larger basins such as the HUs, the effects on peak flows would be attenuated, and would not result in a measurable difference in peak flows. Therefore, there would be minimal effects related to peak flows at the HU level.” Data cited are those from Caspar Creek, wherein a 100% cut watershed was found to produce a 27% increase in peak flow for at least a 6-year period following logging, and the proportional increase was found to depend on the proportion of the watershed cut. No significant dependence was found on the size of the watershed, and at this point the persistence of the impact is not known. In the case of Freshwater, approximately 5% of the watershed is being cut each year. Because hydrologic recovery is not instantaneous, hydrologic changes will accumulate as the watershed is progressively cut. In the absence of calculations that take the cumulative proportion of the watershed logged and the likely rate of hydrologic recovery into account, the assertion that “effects on peak flows would be attenuated and would not result in a measurable difference in peak flows” cannot be supported.

In addition, Figure 9 in Reid (1998a) suggests that the recurrence interval of a 5-year peakflow has *already* been altered to two years in at least a portion of Freshwater valley; *any* further increase in peakflow frequencies through continued canopy removal or continued sediment input would therefore contribute to prolonging an impact that is already significant, thus increasing the temporal severity of the impact. The FEIS/FEIR conclusion of non-significance is thus in error: it does not take into account the changes that have already occurred, so it is not capable of evaluating the cumulative impacts on flooding. “Risk” of exceeding a threshold of significance is certain once that threshold of significance has already been exceeded. It is not valid to assess risk of exceedance in the absence of an adequate analysis of how close to exceedance the system already is.

Adequacy of response to review comments

Review comments again followed the pattern evident in those concerning sediment- and woody-debris-related impacts. State agency responses indicate that there is general denial that hydrologic change is a problem. However, analysis of the individual state-agency responses to points raised by the comments (Appendix 1) indicates that many of the arguments were not well-founded technically, did not address the issues raised by the comments, or actually supported the substance of the comments.

Federal agency responses assert that any such problem is adequately provided for by the FEIS/FEIR through application of the disturbance index. However, as described above (and in the following section), examination of the disturbance index procedure indicates that it is not designed to take hydrologic change into account.

Cumulative impacts

Summary of original concerns: The draft EIS/EIR contained no adequate evaluation of existing cumulative impacts in the affected area and contained no procedure for evaluating future cumulative impacts. Instead, cumulative impact analysis was to be accomplished by monitoring the trend of indicator variables. The disturbance index was the only mechanism for adjusting rates of logging over large areas, but no rationale was given for the values used to calculate the index or for the threshold level of 20%, and no adequate test of the index had been carried out. On the basis only of "simplicity," recovery was assumed to be complete 10 years after logging. Rates of cut that had led to existing cumulative impacts in Bear, Jordan, Stitz, Elk, and Freshwater watersheds would not have been constrained by application of the disturbance index.

New provisions of the FEIS/FEIR for cumulative impacts, and remaining problems

The FEIS/FEIR appears to have omitted the section describing the new provisions for management of cumulative impacts: both the section describing "additional mitigation" for soils and geomorphology (3.6.5.5) and that describing "additional mitigation" for watersheds, hydrology, and floodplains (3.4.3.10) refer the reader to section 3.4.3.10 to find "the major cumulative effects mitigation." However, section 3.4.3.10 contains no description of cumulative effects mitigation, but simply refers the reader to itself (section 3.4.3.10) to find that information.

Examination of Appendix P suggests that the major change in the FEIS/FEIR with respect to cumulative watershed impacts is the requirement that the disturbance index be calculated for Hydrologic Units (e.g. Freshwater Creek watershed) rather than for entire Watershed Assessment Areas (e.g. the area draining into Humboldt Bay). Further, if a Hydrologic Unit is found to be above the 20% threshold for disturbance index, calculations would also need to be made for tributary watersheds. If the disturbance index surpasses 20%, silvicultural strategies other than those similar to clearcutting and overstory removal must be used; strategies such as selective cutting and commercial thinning are allowed. Yarding must then be via cable skyline or helicopter. Logging must be done in a way that lowers the index on an annual basis and results in an index of 20% or below within 10 years.

Despite these steps, the fundamental underlying problem remains: the disturbance index approach is inadequate and is based on demonstrably false assumptions.

Calculation of the disturbance index for Freshwater Creek, for example, shows that the current value is approximately 16% and thus is well below threshold. However, the CDF has already declared Freshwater Creek to be cumulatively impacted. Cumulative impacts to flood frequencies had already been great enough to cause concern in 1995, when the disturbance index was approximately 9% for the watershed. Clearly, a 20% threshold is far too high to allow effective management of practices that have caused the levels of cumulative impact already noted by CDF to exist in the watershed.

Second, an arbitrary recovery time of 10 years is assumed in calculation of the disturbance index. Rationale for selecting this value was explained in the draft HCP/SYP: “For simplicity, 10 and 20 year recovery periods were selected for analysis....” (II-E-2 ph.3), and “...a 10-year DI time factor was selected for reasons connected to SYP planning and implementation” (II-E-5 ph.1). However, simplicity and convenience are not justifiable as the only basis for selecting a value upon which the strategy for managing off-site impacts will depend. Such an approach is particularly unjustifiable when substantive evidence has been presented that the selected value is inadequate (Reid 1998a), and when the FEIS/FEIR itself (p. 3.4-22) cites studies that suggest that hydrologic recovery is only 50% complete in 25 years.

In the face of such evidence, the official response to comments provides a further rationalization for the 10-year recovery period, stating that the value was selected because sites are “most susceptible to surface and mass movement erosion” during the first 10 years. However, this argument, too, indicates the arbitrary nature of the decision: the same could be said for the first eight years, the first 20 years, the first 50 years, and so on. Rates of erosion tend to decrease with time after disturbance, so any of these durations would fit the criterion as being the period “most susceptible to surface and mass movement erosion.” In addition, it is not appropriate to select a recovery rate on the basis of sediment input alone when hydrologic change is likely to be an important mechanism for the cumulative impacts already present in the watersheds.

Selection of an arbitrary threshold at which recovery is assumed to be complete cannot be justified in an area for which existing cumulative watershed impacts are acknowledged to exist by the regulatory agencies, and where those impacts are already damaging downstream property and habitat. Information such as that contained in Figure 4 and that provided by Madej and Ozaki (1996) would need to be taken into account to define a recovery rate that actually reflects recovery with respect to the mechanisms of impact present in the area.

Further, the disturbance index would allow 25% of watersheds such as Freshwater or Elk to be clearcut in a single year without surpassing the threshold of 20% if no cutting had occurred in the previous 10 years. Subsequent selective cutting of 8% of the watershed per year could then be carried out even if the 20% threshold were surpassed, since the requirement is only that, once the threshold is surpassed, subsequent use simply needs to allow the index to trend back toward 20% and to “refrain from all activities with the highest disturbance ratings” (Appendix P, p.47). Because 10% recovery per year is assumed, simply using a strategy other than clearcutting will result in the required trend. The disturbance index clearly does not provide any substantive protection from cumulative impacts generated by high intensities of logging. Unfortunately, as indicated in the original comments by Reid (1998a), increases in the rates of logging over the past decade appear to be the major cause of the cumulative impacts currently of concern.

Analysis of cumulative watershed impacts is intended by the FEIS/FEIR to be accomplished in part through watershed assessment. However, the method upon which the intended assessments are to be based (WFPB 1995) is not itself a method for assessment of cumulative watershed impacts; instead, it is a procedure for design of “best management practices” (Reid 1998b). No description is provided of how a cumulative impact analysis is to be conducted during watershed analysis, and requirements for watershed analysis still do not include a provision for analysis of hydrologic impacts. Without such an analysis, it will not be possible to design appropriate prescriptions to manage for cumulative impacts. As stated in the earlier comments by Reid (1998a), “Appeal to the future design of a procedure to assess cumulative watershed impacts based on an existing method that does not itself assess cumulative watershed impacts does not constitute an adequate solution to the cumulative impacts problem. It is not reasonable that such a procedure should be accepted as adequate before it exists..."

Assessment of significance for cumulative impacts

The analysis for the final EIS/EIR does not establish thresholds of significance for cumulative impacts associated with altered flow or sediment loads. Assessment of cumulative impacts is carried out with a standard of comparison based on present conditions. Because the HCP/SYP is said to contain provisions more protective than those implemented under the Forest Practice Rules, the analysis concludes that the plan will have a positive cumulative effect on each of the watersheds considered (e.g., p.3.4-62, p.3.7-86). This approach to assessment disregards the definition of cumulative impact and does not appear to satisfy the requirements of state and federal cumulative impact legislation.

According to the Council on Environmental Quality (CEQ Guidelines, 40 CFR 1508.7, issued 23 April 1971),

> *"Cumulative impact" is 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 (Federal or non-Federal) or person undertakes such other actions. Cumulative impacts can result from individually minor but collectively significant actions taking place over a period of time.*

And according to CEQA Guidelines (sec. 15355):

> *"Cumulative impacts" refers to two or more individual effects which, when considered together, are considerable or which compound or increase other environmental impacts.*
> *(a) The individual effects may be changes resulting from a single project or a number of separate projects.*
> *(b) The cumulative impact from several projects is the change in the environment which results from the incremental impact of the project when added to other closely related past, present, and reasonably foreseeable probable future projects. Cumulative impacts can result from individually minor but collectively significant projects taking place over a period of time.*

In each case, the focal issue is the impact on the environment or the change in the environment: a cumulative impact is defined with respect to the level of impact to the impacted entity, not with respect to a hypothetical level of impact that could occur under different circumstances (i.e., with Forest Practice Rules). In both cases the effect of a project is judged *when added to* that of past, present, and reasonably foreseeable future projects: the overall level of impact is to be assessed, not the incremental change to that level imparted by the isolated project. The interpretation that a cumulative impact of a project can be determined to be beneficial because the project is marginally less damaging than it could otherwise have been is clearly unsupportable. The FEIS/FEIR has not provided a valid assessment of the cumulative impacts of the proposed plan. It has instead evaluated the individual effect of the plan with respect to a hypothetical alternative outcome.

Adequacy of response to review comments

Examination of the official response to comments suggests that agency staff may hold several misconceptions concerning cumulative impacts.

First, the argument is repeatedly made by the response that the proposed plan is sufficient to avoid aggravating existing cumulative impacts because it provides levels of protection higher than those that currently exist (Pg.6 par.2(b), Pg.18 par.4; Pg.20 par.3 to Pg.21 par.7; Pg.24 par. 4 and 5, and Pg.24 par.5 to Pg.25 par.1; Pg.25 par.5). Even discussions of the adequacy of mitigations for individual impact mechanisms rest on the argument that the mitigations are an improvement over current provisions, rather than on arguments that the resulting cumulative impact levels will acceptable (Pg.18, par.5). However, in the case of both SYPs and HCPs, the intent is not simply to marginally decrease the rate of impact accumulation, but to establish a management plan which will not lead to destruction of public trust resources and damage to beneficial uses of water, and which will not "appreciably reduce the likelihood of the survival and recovery of the species in the wild." Each plan thus must be judged relative to the impacts it produces (in combination with those of past, present, and reasonably foreseeable future actions), not relative to levels of protection previously in place. In other words, a plan is not adequate if it allows impacts to continue to accumulate, albeit at a slower rate. In this case, current Forest Practice Rules are clearly not adequate to prevent cumulative impacts, or it would not have been necessary for CDF to declare Freshwater, Elk, Stitz, Jordan and Bear Creek watersheds to be cumulatively impacted. The plan must therefore be shown to be *enough* better than existing regulations to not impede recovery from these impacts; simply decreasing the rate at which the impacts are now accumulating is not sufficient.

In addition, the response to Pg.18, par.5 and that to Pg.23, par.3 suggest that the concept that "Cumulative impacts can result from individually minor but collectively significant actions taking place over a period of time" (Council on Environmental Quality Guidelines 40 CFR 1508.7, and CEQA Guidelines sec. 15355) is not fully understood. In each case, the response was based on the argument that decreasing the impact of individual actions would solve the cumulative impact problem; in neither case was the collective significance of the resulting individual actions discussed or evaluated.

The comments provided substantive evidence that the methods to be used to manage cumulative impacts are neither adequate nor valid. The response countered these arguments by quoting the Forest Practice Rule that states that "the sufficiency of the information provided in a SYP...shall be judged in light of what is reasonably feasible and necessary," thus indicating that the regulatory agency does not consider it "necessary" that a method upon which an SYP is based be demonstrated to actually work. CDF has judged the cumulative impact assessment methods of the FEIS/FEIR to be sufficient (Pg.26, par.2) even though that assessment will be carried out through an as-yet-undesigned procedure that is based on an existing method that does not address cumulative impacts (Pg.22 par.4(b)), and even though an earlier response (Pg.23 par.4) indicated that the agencies do not believe that any method is available that will address the range of impacts that might occur.

Procedural issues

Several issues arose during preparation of review comments that reflect on the NEPA and CEQA procedures and raised questions concerning the adequacy of the procedures implemented in the present case.

Feasibility of review

The Final EIS/EIR is approximately 2000 pages long, and the document also incorporates portions of the original draft SYP/HCP by reference (including the SYP volume and the method for calculating disturbance index, for example). Such incorporations would account for at least 250 additional pages. Thirty days were allowed for review. A thorough technical review ordinarily requires about an hour for 10 double-spaced pages or 5 single-spaced pages, plus additional time required for writing up the results of the review. In this case, review would thus have required at least 450 hours of reading and analysis and an estimated 40 hours to write up the results, for a total commitment of 12.25 work weeks. It is not possible to do an adequate technical review of a 2250-page document in 4.5 weeks.

The review process for the draft EIS/EIR suffered from the same problem. In that case, 45 days were allowed for public review, and the document contained approximately 1350 pages. This document, too, incorporated portions of the original draft SYP/HCP by reference, adding another 250 pages to the total required for review. Again assuming that a week is required to write up the results, total time commitment for adequate review would have been on the order of 9 weeks, rather than the 6.5 weeks allowed. Adequate technical review thus was not possible for that document, either, in the 45 days provided.

Further, cryptic organization of the FEIS/FEIR made it very difficult to determine what is actually in the document. Appendix pages, for example, are not numbered consecutively, and no Appendix identification is provided on each page; it is thus not possible to find a particular appendix without thumbing through the volume page-by-page. In the incorporated SYP, neither pages nor tables are numbered, again making review difficult.

Hasty preparation of the FEIS/FEIR appears to have left the document with peculiar flaws, such as the case where a section (3.4.3.10) references itself for the location of the absent "major cumulative effects mitigation." Although such a glitch is obviously simply an oversight, the frequent occurrence of such problems greatly increases the difficulty of reviewing the report.

Incorporation of new information

The task of review was further hampered because substantive new information provided in public comments to the draft EIS/EIR was not consistently incorporated into the Final EIS/EIR. For example, the FEIS/FEIR states that no turbidity information is available for the project area, despite the fact that such information was referred to by and presented in public review comments. Information directly relevant to plan design—such as the provision of a reference to a published study of woody debris source distances in a coastal California redwood forest—was ignored; data from a Douglas-fir forest in the Oregon Cascades continue to be used as the basis for design of riparian management zones. The process of foliage interception loss is also still not discussed, despite provision of many citations describing the process and its importance, and despite the provision of data from the project area. In fact, I could find no evidence in the FEIS/FEIR that any of the data, information, analyses, concepts, or references that I had provided had been incorporated into the document.

Oddly, where the wording in the FEIS/FEIR has changed from that in the draft, the changes are at odds with the content of substantive public comments. On page 2-63, for example, a sentence has been added: "Only long-term monitoring of channel conditions and stream flows could confirm an increase in flooding with channel aggradation, however." Given the comments provided by Reid (1998a) and the further discussion in the present comments (see Table 3, for example), it is quite clear that geomorphological methods used elsewhere to evaluate changes in flood frequencies in the absence of flow records can also be used in the present case.

Because substantive information provided by public comment was so consistently disregarded in the rewrite, the reviewer faces a serious quandary: is it worthwhile to use up a considerable portion of the 30-day comment period to reiterate the concerns and information already introduced in earlier comments, but which had not been incorporated in the document? Because of the lack of adequate time for review, this was not done. An adequate review, however, would have again pointed out the additional information that would need to be incorporated to make the FEIS/FEIR a technically sound document.

Adequacy of response to comments

Given the tendency of the FEIS/FEIR to disregard at least some relevant technical information known to have been provided by public comments, it became useful to evaluate the official response to my own comments (Reid 1998a) to determine whether the issues raised by the comments had been seriously considered. Response to the comments were in two parts, the first apparently prepared by staff of CDF and CDMG ("state response"), and the second apparently prepared by staff of NMFS and USFWS ("federal response").

In general, the federal response provided useful documentation of the modifications that were expected to address the points raised. However, no evidence was provided that the modifications would be adequate to solve the problems. In the case of hydrologic cumulative impacts, for example, the response indicates that "Effective rainfall is addressed through the disturbance index" (response LMR-9, p. T-208; also LMR-6, LMR-9, LMR-10, LMR-11). However, as discussed in earlier sections, examination of provisions for the disturbance index in the FEIS/FEIR indicates that the disturbance index does not address effective rainfall. The federal response to flood-related issues (LMR-13, p. T-209) simply referred to the state response (Pg.12 par.5a, Pg.12 par.5b, Pg.12 par.5c, Pg.12 par.6 to Pg.13 par.1), thus incorporating the latter by reference, but analysis of the state responses found that none of these adequately addressed the issues raised (see Appendix 1). With a few exceptions, the level of discussion in the federal response was generalized, and many specific points raised by Reid (1998a) were not addressed. Interpretation of the responses is made difficult because comments are referred to by code rather than by page and paragraph; the codes may have been explained on the CD containing comments, but I did not have a copy of the CD and I could not access Appendix U on internet.

In contrast, the state response attempted to demonstrate that, with one exception, 86 of the issues raised by Reid (1998a) were invalid; the response attempted to negate even those issues that appeared to have been accepted as valid by the federal response. However, evaluation of the responses to specific comments discloses a variety of problems with the responses (Table 4, and see Appendix 1 for discussion of specific responses).

For about 21% of the 86 issues for which responses were provided, the actual issue raised in the comment was not addressed by the response either because a trivial detail was addressed in place of the substantive issue or because the response was simply irrelevant to the issue raised. For example, substantive evidence was summarized by comments 3/3a and 4/3b (this notation refers to page/paragraph of the comment) that woody debris inputs would not be of the quantity, diameter, and character required to fulfill the objectives of the HCP/SYP; the official response simply explained that "the HCP/SYP will ensure the continuation of this process [of woody debris infall]" (3/3a) and that prescribed buffers "will provide a continuing source of functioning woody debris" (4/3b).

Table 4. Problems exhibited by official responses to comments by Reid (1998a). Examples refer to page/paragraph of the original comment.

Response problem	Of 86 issues: number	%	Examples
Not relevant to issue raised	18	20.9	1/4 4/3b 5/2a 16/3 26/3
Misrepresented comment	8	9.3	3/2 3/5 10/last 12/5a 16/2b
Technical error	9	10.5	6/2 9/2-3 9/8 10/2 23/3
Unintentionally supports concern	7	8.1	2/1c 2/3 6/5b 8/4 15/4

Substantive issues concerning the amount and character of woody debris were thus addressed by the non sequitur that *some* trees would continue to fall into streams.

The comment itself was misrepresented in 9% of the remaining cases, and the misrepresentation was addressed rather than the original comment. For example, the comment that the severity of an impact increases as the magnitude of a change increases was misrepresented as having said that only old-growth conditions are sufficient to sustain salmonids (3/2). Because original comments are available only on a compact disk, it is unlikely that anyone but the commentor would know that the original comment had been altered. When this category of problem is combined with the previous category, fully 30% of the issues raised in the comments were not addressed in the response.

For about 10% of the issues addressed, the response contained technical errors that are significant enough to reverse the conclusion of the response. Problems included confusion between the geomorphological concepts of “scour” and “incision” (9/2-3) and an error in calculation (11/4-5). In several cases, errors could have resulted from a lack of information: the response that flooding is an issue in Freshwater simply because houses were recently built within the 100-year floodplain (10/2) is clearly countered by the fact that the homes being impacted by flooding include some as old as 100 years, but it is possible that the respondent was not aware of the ages of the affected homes.

Finally, responses addressing 8% of the issues provided arguments or information that actually supported the concerns raised by the comments, despite the fact that the responses were attempting to use this information to conclude that the concerns were not valid. For example, in an attempt to rebut a comment that the mass-wasting avoidance strategy is not sufficient, DMG noted that provisions designed using the mass-wasting avoidance strategy were strengthened by DMG when a THP was reviewed (15/4 part 1). Rather than demonstrating that the strategy works, however, this reply illustrates the original concern: application of the mass-wasting avoidance strategy was not sufficient to bring the THP up to even the standards ordinarily applied to THPs by DMG during review.

In several other cases, responses employed strategies that are more worrisome. Responses to comments concerning landsliding issues included altering a direct quotation to make it appear to support the position of the response (15/4 part 4) and quotation of an allegation of scientific misconduct that had been prepared by a contractor to Pacific Lumber Company (16/2). Inclusion of the latter is particularly puzzling, as a state geologist (Spittler 1998) had earlier provided information that showed the allegation to be baseless.

In one case a comment appears to have been “disqualified” because the citation to the work being discussed was not repeated in the second paragraph of the discussion (5/5). It should have been obvious to the respondent that the topic of discussion had not changed, but this “technicality” was used as grounds for not addressing the comment. Communication is a tenuous process; it requires a good-faith effort on the part of the writer to do the best job they can to present their ideas clearly and concisely, and it requires a good-faith effort on the part of the reader to do the best job they can to interpret the writer’s intended message. Reliance on a technicality to avoid addressing the substance of a comment is strong evidence that this good-faith effort is not being made by the reader, and that the intent of CEQA and NEPA requirements for consideration of public review comments is not being fulfilled.

A double standard appears to be applied rather consistently in the responses. On the one hand, a response argues that the results of a 100% inventory of all landslides in Bear Creek watershed cannot be used to infer the likely results of continuation of the same practices within that watershed, while on the other hand, another response suggests that results of an as-yet-unreleased partial survey of landslides at selected sites in Oregon provide information relevant to all lands covered by the SYP/HCP (6/2a). Similarly, on the one hand a response argues that results of a study of woody debris source distances in a coastal California redwood forest cannot be used to predict woody debris source distances in coastal California redwood forests, while on the other hand, the response maintains that results of a similar study in an Oregon Douglas-fir forest can be used to predict source distances in a coastal California redwood forest (5/6). The pattern appears to be that data are more likely to be considered admissible if they support the general conclusions exhibited by the response: that the SYP/HCP will not be result in cumulative watershed impacts.

The approach taken by the state response conveys the strong impression that those preparing the response viewed the commentor as an antagonist to be defeated, rather than as a colleague sharing the common goal of endeavoring to produce a useful and technically accurate document. I had been under the impression that the role of those reviewing public comments in the preparation of an EIS or EIR was to make balanced use of the contributed information to improve the document; I had not expected the respondents to use whatever arguments are necessary to support the original text of the document. It may be useful to review responses to comments submitted under other circumstances, such as those prepared during the THP review process, to determine whether this pattern is characteristic of the agencies, or whether it is simply an aberration prompted by the controversial nature of the present project. It is possible that the short time available for completion of the document impacted not only the reviewers, as described above, but also those preparing the responses. It is not possible to prepare a balanced and technically adequate response if inadequate time is available to do so.

Assessments of significance thresholds and risk

Key to assessing the overall adequacy of the FEIS/FEIR is examination of the adequacy of document’s risk assessments.

The first step in risk assessment is identification of the thresholds of significance for changes. Several of these thresholds seem to have been selected arbitrarily, with little regard for the actual significance of changes as they would be experienced by downstream beneficial uses.

For example, the threshold of significance for increased flood frequencies was selected to be a change in frequency from 5 years to 2 years for a flood of a given size. However, there is no assessment of what the ramifications of such a change would be for downstream channel stability, flood damage, harbor dredging, or habitat conditions. Furthermore, as discussed in an earlier section, a 6-fold increase in the frequency of a 100-year flood (i.e. turning it into a 15-year flood) can occur without surpassing the selected threshold for flooding. Such a change would have tremendous significance for the ability of downstream residents to acquire home-owner's insurance or to sell their houses and it would greatly reduce downstream property values and even habitability, but this would not be considered a "significant" change for the purposes of the final EIS/EIR. To be valid, thresholds of significance must be based on levels of change that are relevant to downstream beneficial uses, levels that—using words from the Basin Plan—do not "cause nuisance or adversely affect beneficial uses." Such an evaluation appears not to have been done for the significance thresholds selected for the present plan.

Once a threshold of significance is selected, the risk of surpassing that threshold is assessed. These assessments, however, do not reflect the actual risk of harm to beneficial uses.

The FEIS/FEIR describes the procedure for sediment-related water quality objectives (p. 3.4-44): *"For CEQA, the threshold of significance for sediment delivery to streams is based upon the reduction of management-related sediment such that discharge to streams will not degrade or impede the recovery of beneficial uses,"* thus implying that levels which degrade beneficial uses exceed the threshold of significance. However, *"exceedance of a threshold of significance for sediment is evaluated based upon whether adequate management for sediment reduction would occur. That is, the thresholds...are evaluated in relationship to proposed management measures that reduce sediment delivery."* In other words, the plan is not being evaluated relative to the needs of the beneficial uses, as required by CEQA, but according to the extent of sediment mitigations.

Assessments of risk for the FEIS/FEIR also do not take into account existing levels of impact, and this invalidates the risk assessment. The risk of surpassing a threshold depends strongly on how close to the threshold the system already is. If a system is already over threshold, "risk" has changed to "certainty," while if a system is far below threshold, large changes are possible without risking significant impact. Risk thus *cannot* be assessed adequately without accounting for current levels of impact.

The assessment of risk for floodplains illustrates this problem. For floodplains, the threshold of significance is selected to be *"a change in the flood storage capacity...sufficient to substantially increase overbank flooding"* (p. 3.4-52). The assessment notes that the risk for coarse sediment input under the preferred alternative has been determined to be *"moderate (i.e., less than significant for CEQA purposes). The potential for significant aggradation and related increases in flooding is also considered to be moderate for this alternative. Therefore, the potential effects of flooding on people and property are also less than significant"* (p. 3.4-54). However, the Freshwater and Elk floodplain systems are already significantly impacted, as demonstrated by the data provided by Cafferata and Scanlon (1998) and by the pattern of recent flooding (Table 3): overbank flooding has already been substantially increased. Any further increase in the magnitude or duration of the impact constitutes aggravation of an already significant cumulative impact, and any increase in sediment input or runoff will increase the magnitude and duration of the impact. A "moderate" risk may be "insignificant" if the threshold of significance has not already been surpassed, but a different standard is required when the impact is already significant—this is the essence of the cumulative impacts requirements.

Risk assessments for the FEIS/FEIR also depend on arbitrary and undocumented definitions of "low," "medium," and "high" risk. To be valid, a method of assessment must produce reproducible results, even if it relies on "professional judgment." In this case, not enough information is provided to evaluate even the meaning of the various levels of risk: is a 30% chance of causing a 95% increase in landsliding rate a low, medium, or high risk? Is a 95% chance of causing a 30% increase in landsliding a low, medium, or high risk? The reader has no way of knowing.

In the case of timber-related mass-wasting, for example, no threshold of significance is defined, and, instead, *"...the relative risk of mass wasting occurring above the natural, background* *rate was assessed. Risk was ranked as low, moderate, or high, and was evaluated based on the potential for occurrence of landslides, the measures proposed to minimize landslides..., and the threat of affecting people, property, or aquatic habitat....The best available information from the literature, Coarse Sediment, Fine Sediment, and Soil Productivity along with professional judgement, was used for the evaluation. A high risk is considered to exceed the threshold of significance. A moderate or low risk is considered to be below the threshold of significance."* (p. 3.6-32). It would appear from this description that the likelihood of generating landsliding above natural rates is being assessed, but there is no definition of what constitutes "high," "medium," and "low" risk. A decision that there will be a moderate risk, based on professional judgment, is meaningless unless there is some description of what constitutes a moderate risk, and unless there is some justification that this level of impact is indeed less than significant for the beneficial uses that will be impacted. Further, the protocol for arriving at that decision must be described and must be shown to produce reproducible results. In this case, the risk of generating landslides above background rates on planar logged slope actually appears to be on the order of ten times greater than under natural conditions, according to "the best available information" in the form of the Bear Creek study by PWA (1998a); this estimate is based on complete implementation of the full protections possible under the mass-wasting strategy (i.e., complete protection of inner gorges and headwall swales). It is not clear how a 10-fold increase in landslide risk can be considered only a "moderate" risk.

Finally, because the plan is materially incomplete it is not actually possible yet to evaluate the level of risk associated with implementation of the plan. It will not be possible to determine what the actual levels of environmental protection are until watershed analysis is completed and prescriptions are written.

Conclusions

A large number of important decisions that will affect the level of environmental impact caused by implementation the SYP/HCP are deferred until after the plan is approved; the plan is, in essence, incomplete. Because of this strategy, considerable reliance is placed on the future discretion of federal and state regulatory agencies; important decisions regarding the provisions of the plan will be made without opportunity for public review and comment under NEPA and CEQA processes. In particular, new provisions in the FEIS/FEIR can be reversed through the watershed analysis process and through adaptive management "experiments." The consistent denial of existing problems that the agencies exhibited in their responses to public comments strongly suggests that the agencies are not likely to make the decisions necessary to adequately address the problems described by Reid (1998a). In particular:

1. Whether the FEIS/FEIR will be capable of preventing continuation of high rates of sediment production in the affected watersheds will depend on key decisions concerning acceptable risk and land-use prescriptions, but these decisions are postponed until after the Record of Decision is adopted. Official agency response to sediment-related concerns in public comments on the draft EIS/EIR essentially denied the existence of the problems, suggesting that the eventual decisions will not be adequate to address the concerns raised.

2. It is also not possible to determine whether the FEIS/FEIR will be capable of maintaining the composition, size, and quantity of woody debris necessary to protect downstream channels and provide adequate aquatic habitat because the FEIS/FEIR allows considerable reduction in levels of protection through watershed analysis and adaptive management plans, which will occur after the

Record of Decision is adopted. Agency response to woody-debris-related concerns again denied the existence of the problems, suggesting that future agency decisions concerning prescriptions will not result in measures adequate to address the concerns. In any case, a proportion of the residual old-growth within falling distance of channels will continue to be cut, so existing provisions are not sufficient to prevent continued aggravation of existing cumulative impacts to woody debris loads.

3. Provisions of the FEIS/FEIR remain insufficient to prevent pervasive hydrologic changes arising from the maintenance of immature forest over large portions of the affected watersheds. Such impacts can be managed only by adjusting logging rate within a watershed, yet the FEIS/FEIR procedure for doing so (the disturbance index) is still based on demonstrably false assumptions and still does not account for hydrologic impacts. There is no provision for hydrologic analysis during watershed analysis, so the changes that would be required to address hydrologic issues will not be possible. Further, agency response to concerns raised about flooding also denied the existence of these problems, suggesting that agencies are not likely to use their discretion during watershed analysis to increase levels of protection from hydrologic impacts.

4. The new provisions call for 2175 acres of future logging to be exempted from some FEIS/FEIR restrictions, but no description is provided of the location of the lands to be logged and no analysis is provided of the potential impact of that exemption. Currently, the FEIS/FEIR indicates that only 5420 acres of riparian land in the entire ownership is “functioning” as required for aquatic ecosystem health. If the exempted 2175 acres account for destruction of the functionality of a portion of these remaining 5420 acres, then this exemption will disproportionately increase the severity of existing cumulative impacts to habitat necessary for coho salmon.

5. Because implementation of the FEIS/FEIR is likely to result in increased severity of cumulative impacts, the consequences described in my letter of 16 November 1998 remain of concern: increased flood damage to downstream properties and salmon habitat through increased flood runoff, continued decline in salmon habitat quality through continued depletion of the potential supply of large woody debris, and decreased survival of salmon produced from other ownerships (including federal, tribal, and private) in the affected watersheds due to the salmon’s need to run the “gauntlet” of stream reaches impacted by provisions of the plan.

During the review, several issues have arisen concerning the NEPA and CEQA process that I believe are important to consider:

6. The 30-day comment period did not provide sufficient time to review the 2000-page document adequately. The document was poorly organized, and additional substantive information provided during earlier public comment was not incorporated into the document.

7. The FEIS/FEIR does not actually provide an assessment of the cumulative impacts of the plan. Instead, it maintains that if the provisions of the plan are incrementally more protective than those currently existing, the cumulative effect of the plan will be positive *irrespective of the overall impact of provisions of the plan on the environment*. This interpretation clearly disregards the definition of cumulative impacts provided by NEPA and CEQA guidelines. The FEIS/EIR thus may not satisfy NEPA and CEQA requirements.

8. Determinations of “threshold of significance” and “risk” appear to have been largely arbitrary. Definitions of “high”, “medium”, and “low” risk are not described, and there appears to be no reproducible procedure for arriving at the judgments. “Thresholds of significance” are defined in a

way that allows severe downstream impacts, such as the potential alteration of the frequency of a 100-year flood to once every 15 years, to be considered insignificant.

In summary, evidence in the FEIS/FEIR and in the official response to public comments indicates that the HCP/SYP, as written, is insufficient to avoid habitat modifications that will appreciably reduce the likelihood of the survival and recovery of coho salmon in the wild. There is no reliable assurance provided that the level of protection will not be reduced, through watershed analysis and adaptive management, to that already found to be insufficient in the Draft EIS/EIR.

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Appendix 1

Discussion of official response to SYP/HCP comments by Reid (1998a)

Pg.1, par.4 – The issue raised by the comment was that prolonging an existing cumulative impact at existing levels increases the magnitude of the impact, because increasing the duration of an impact increases its severity. The response does not address this issue, and instead simply describes provisions of the California forest practice rules. The response implies that the forest practice rules adequately address cumulative watershed impacts. However, the fact that five of the watersheds to be covered by the proposed HCP have recently been declared by the CDF to be cumulatively impacted indicates that the forest practice rules are not sufficient to prevent cumulative impacts.

Pg.2, par.1(a) – The response seems to imply that if a study is carried out in an area that differs from the project area in some respect, the results are not applicable to the project area. However, most information used to make decisions at any one location is usually derived from other sites. Later responses (e.g., Pg.5 par.2; Pg.5 par.6; Pg.6 par.2a; Pg.6 par.5a, Pg.10 last par.; and so on) indeed apply information from other sites to the project area, often using information "based on both topographic and harvesting conditions that are not representative of conditions in the proposed HCP/SYP."

Pg.2, par.1(b) – The citation of "DMG" as the source for a statement concerning the effectiveness of woody debris in capturing sediment is incomplete because it does not provide enough information to allow the source to be contacted to ascertain the nature of the field observations referred to. If the statement is true (although my own field observations suggest the range of effectiveness to be much higher), the statement supports the issue raised by the comment: many destructive debris flows are triggered by relatively small debris avalanches, so woody debris in channels is capable of decreasing the level of impacts from landslides even if it "will only catch 'much' of the sediment from landslides if the initial failures are very small."

Pg.2, par.1(c) – Again, the response relies on an unverifiable citation. In addition, the response, if true, would underscore the overwhelming importance of providing abundant large wood near the site of potential failures. Because many destructive failures occur in or adjacent to Class III channels, this would support the strong need for no-cut buffers along Class III channels. Additional supporting evidence comes from comparison of debris flow tracks evident on 1963 and 1997 aerial photographs from Bear and Jordan Creeks: debris flows that passed along channels through mature forest consistently appear to travel shorter distances than those that passed through channels along which the near-stream forest had not recovered from logging-related disturbance.

Pg.2, par.2(a) – The response seems to imply that the possibility that the destructiveness of a debris flow might be enhanced by the presence of woody debris is enough to negate the effectiveness of woody debris in decreasing the travel distance of flows. This is an unsupported assertion, and is contradicted by the previously mentioned comparison of debris flow tracks in Bear and Jordan creeks: flows either did not occur in forested areas or traveled relatively short distances.

Pg.2, par.2(b) – The response seems to imply that, in the Knowles Creek case, the victims were responsible for their own deaths because their residence "was built on a flat at a bend in the channel, very close to, or on, the centerline of the flowpath." However, houses sited to avoid floods are not necessarily sited to avoid unprecedented debris flows. A location that had been relatively safe can be modified, by upstream land use, into a location that is no longer safe. Additional examples of deaths caused by logging-related debris flows and landslides include that of a farmer in Skagit County, WA and that of four residents of Marblemount, WA (Evans 1988).

Pg.2, par.2(c) – The response indicates that destruction of or damage to 7 of the approximately 14 homes in the part of Stafford located to the south of Highway 101 does not constitute damage to "much of the town of Stafford." The families who lived in those homes, however, would be likely to have a different definition of the word "much."

Pg.2, par.3 – This response, too, relies on an unverifiable citation, and again actually supports the concern raised by the comment. As did the original comment, the response indicates that woody debris in the channel must be "really large and plentiful" to be effective.

Pg.2, par.7 – The response does not address the content of the comment. The point made by the comment was that a major sediment source to Class I channels is destabilization of Class II channels, so Class II channels must be well-protected if Class I channels are to be protected. In other words, much of the impact to Class I channels is an indirect outcome of disruption of Class II channels. The response does not address the importance of these indirect effects and, instead, limits discussion to direct impacts to Class I channels. The response relies on an unsupported assumption that the indirect effects—such as those from landslides on tributaries—are less important than the direct effects.

Pg.3, par.2 – The response misrepresents the original comment and thus avoids addressing the issue raised. The response then misrepresents the subsequent modeling and conclusions of the original comments to be based on the false premise that the response itself constructed.

The point of the original comment was that impacts occur in a continuum. We know that conditions before disturbance were sufficient to sustain the species of concern; those conditions, after all, are the ones under which those species evolved. The more that current conditions differ from the original conditions, the less likely it is that the new conditions are suitable for sustaining the species of concern. This is a basic precept of fisheries management in the Pacific Northwest, and aspects of the concept are described in a variety of recent papers and reports. Bisson et al. 1997, FEMAT 1993, Frissell and Bayles 1996, Frissell et al. 1997, National Research Council 1996, and Reeves et al. 1995 all note the necessity of understanding natural conditions if impaired habitat conditions are to be improved. This precept is also the basis for design of "desired future conditions" for stream systems: natural systems are examined to inform decisions of what the desired conditions should be (e.g., USDA and USDI 1994, NMFS 1996). The response, however, misrepresents the comment as having said "old growth forest habitat is a prerequisite to reducing current impacts on Pacific salmon." This false characterization of the comment is troubling, as most readers of the response do not have the opportunity to read the original comment and thus will not realize that the original comment was misrepresented.

Pg.3, par.3(a) – The response confuses "within the range of" with "characteristic of." A common fallacy in ecosystem management is that a condition is tolerable if it falls "within the natural range of variability." However, consider the case of flooding: a 100-year flood is clearly within "the natural range of variability" for a river system, but it would certainly not be considered tolerable or characteristic of natural conditions if it were to recur every year. What is important is the *distribution* of conditions through time and space, not the *range* of conditions at a particular time.

In the case of the comment in question, the conditions of concern are the future distributions of woody debris. The calculations were thus carried out for the conditions expected over the long-term, not for those expected soon after an area is cut. Edge effects result in increased riparian blowdown in the years following logging (Reid and Hilton 1998), thus depleting riparian stands of sources for woody debris that would replenish decaying wood later in the cycle. In contrast, the response notes that loadings after logging are “within the range” of those measured in old-growth forests. The response thus confuses a symptom of the problem—early depletion of large riparian conifers—with a solution.

The response then states that “the HCP/SYP will ensure the continuation of this process.” However, “continuation of the process” is not the point; preservation of a single tree for later in-fall would allow “continuation of the process.” The HCP/SYP had indicated that it will ensure the continuation of woody debris loads equivalent to 75.2% of those characteristic of old-growth forests along Class I streams. The calculations included in the original comments demonstrate that the HCP/SYP is not capable of doing so.

The response does not acknowledge the primary flaw of the HCP/SYP woody debris calculations: 100-year-old second-growth trees were assumed to be the same as old-growth.

Pg.3, par.3(b) – The response implies that the analysis of woody debris loads contained in the comments is flawed because it does not include data for short-term inputs following logging. As described above, the short-term response is not characteristic of long-term loadings, as discussed by the paper cited by the response (Reid and Hilton 1998).

Pg.3, par.4 – The response implies that a stand of redwoods repeatedly cut at a 60-year age is capable of producing, in perpetuity, the sizes and loadings of woody debris characteristic of old-growth redwood forests. This is not the case, because 60-year-old redwoods attain neither the height nor the girth of old-growth redwoods, and the decay resistance of second-growth is low relative to that of old-growth redwoods.

Pg.3, par.5 to 6 and Pg.4, par.1 to 2 – The response mistakenly states that the misrepresentation of the comment on Pg.3 par.2 is the basis for the calculations included in the comment. In actuality, the calculations were based on the SYP/HCP’s expressed intention to provide to Class 1 channels 75.2% of the wood inputs characteristic of old growth. The response also overlooks the problem that patterns of woody debris accumulation immediately following logging are known to misrepresent the long-term patterns that were the object of the calculations. The response concentrated on these issues rather than addressing the concern raised by the comment.

Pg.4, par.3(a) – The response again fails to address the issue raised. A second-growth forest cannot produce woody debris of the size characteristic of old-growth forests because a second-growth forest does not contain trees of the size characteristic of old-growth forests. Because large-sized woody debris is important to these systems—as is also indicated by the response to Pg.2, par.3—the continued removal of the last remaining sources of old-growth woody debris over much of the lands will clearly be detrimental to the habitat of the coho salmon both during the life of the SYP/HCP and for centuries thereafter. The statement that leaving existing residual trees capable of falling into the channel “would have little beneficial effect on fish populations during the life of the SYP or HCP” is clearly unfounded, when *not* taking that action will clearly damage the habitat depended upon by those fish.

Pg.4, par.3(b) – The response again fails to address the issue raised. Most of the area covered by the SYP/HCP has been logged at least once. There are few trees left that are large enough to contribute woody debris of sizes characteristic of channels in which the fish evolved. If many of those remaining trees are removed, the channel habitat will continue its trend away from the conditions characteristic of those under which coho salmon evolved in this area. The SYP/HCP calls for the establishment of buffers to provide a source of woody debris intended to fulfill the role required to prevent the extinction of the ESU. If the SYP/HCP contains provisions that ensure a continued decrease in the average maximum sizes of woody debris over the next century, then the SYP/HCP ensures that the trend of habitat degradation will continue for the next century. The only way to at least slow the further depletion of old-growth-sized wood in these systems is to halt the removal of the only source of old-growth-sized trees capable of falling into the creeks until trees in the second-growth riparian buffers attain sizes characteristic of old-growth forests.

The response avoids addressing this issue by simply stating that the prescribed riparian buffers "will provide a continuing source of functional woody debris to these streams." Providing a "continuing source" was not the issue raised by the comment; providing an "adequate source" was the issue.

Pg.4, par.4 – The response implies that because no documentation of the impact was found *in response to timber operations*, the issue raised by the comment was not significant. However, the effect is widely documented in the hydrology and waterways engineering literature (e.g. Dunne and Leopold 1978, Linsley et al. 1982, Linsley and Franzini 1979), and has been cited as a concern by CDF staff in relation to the flooding issues in Freshwater Creek (Cafferata 1997). It is common practice to clear debris from channels to increase channel conveyance. Thus, the presence of debris in channels reduces conveyance significantly, thereby significantly increasing the temporary storage of floodwaters.

Pg.4, par.5 – The response simply alludes to the earlier response to Pg.3, par.4; examination of the earlier response shows that it did not adequately address the issue.

Pg.4, par.5 and pg.5 par.1 – The response simply alludes to the earlier response to Pg.4, par.4; examination of the earlier response shows that it did not adequately address the issue.

Pg.5, par.2(a) – The response focuses on the terminology to be applied to a hyper-concentrated sediment flow rather than addressing the issue raised by the comment: that excessive sediment loads generated by landslides associated with logging in the area are documented to have destroyed habitat improvement structures and adversely modified kilometers of habitat critical to the survival of salmonids, and that a PL consultant attributed aggravation of the impact of the debris flow to the lack of large woody debris in the channel.

Pg.5, par.2(b) – This response, too, does not address the substance of the comment. Field observations by PWA staff in North Fork Elk River drainage—a portion of the property under discussion—indicate that gullying is an important process in the area (PWA 1998a). The response instead focuses on the lack of gullying at Caspar Creek, located south of Fort Bragg.

It should be noted that the Caspar Creek data suggest that the increased sediment yield from logged lands at Caspar Creek is associated with increased storm flows measured there after logging, thus supporting the point of the comment: logging increases storm flow, which increases sediment transport. An increased potential for sediment transport is a common mechanism for gully initiation. To the extent that gullying is associated with increased flows, gullying will increase as forest cover decreases in the catchment in question.

Pg.5, par.3 – This response also does not address the issue raised by the comment. The authors of the HCP/SYP themselves indicated that the plan for Class I and Class II channels would "provide 75.2 percent and 34.6 percent of the LWD recruitment expected in unmanaged forests, respectively" (SYP/HCP p.IV-D-58), but their calculations are based on 100-year-old second growth stands rather than on unmanaged forests. The authors of the SYP/HCP therefore established the standard of comparison and then ignored it, so the results presented in the SYP/HCP are in error with respect to the standards set by the plan. The response indicates that the comment is requesting a standard of comparison that the respondent believes to be too high, but the response overlooks the fact that the standard of comparison had been established by the SYP/HCP, not by the commentor.

Pg.5, par.5 – The response again does not address the issue raised, this time because the respondent did not realize that the reference to "measured distributions of debris sources in a redwood forest in northwest California" was to the same data set that had been the focus of discussion in the immediately preceding paragraph, that was illustrated in Reid's (1998a) Figure 4, and that was the only data set for redwood forests that had been discussed.

Pg.5, par.6 and pg.6 par.1 – The response focuses on wording rather than addressing the substance of the comment. Whether the stated level of woody debris input reported to be achieved by the plan is a "design goal" or an "expected level" is not particularly important. Instead, the issue raised by the comment was that "the levels of loading described in [the plan] were considered necessary by PL and NMFS in preparation of the plan, and they are the levels upon which the assumptions of the SYP/HCP are based." The fact that the plan's calculation of expected levels was erroneous leads to the conclusion that the expected levels of protection will not be achieved.

Furthermore, an earlier response (Pg.5, par.4) argues that differences between debris source distances measured in a northwest California redwood forest (Reid and Hilton 1998) and an Oregon Cascades Douglas-fir forest (McDade et al. 1990) may reflect differences in site conditions. Now, however, the respondent argues that because of this difference, the data from the Oregon Cascades Douglas-fir forest should be applied to northwest California redwood forests instead of the data from a northwest California redwood forest. This reasoning does not appear to be valid.

Pg.6, par.2(a) – To counter the comments' use of landslide data from PL land, the response cites an Oregon landslide study that has not yet been released and is currently undergoing technical review. Data from a PWA study (PWA 1998b) show that headwall failures in the Bear Creek watershed are considerably more common on slopes logged less than 15 years ago than on slopes last cut more than 30 years ago, and these data are considerably more relevant to Bear Creek watershed than are the unreleased data from Oregon.

Pg.6, par.2(b) – The respondent states that because timber management has gone on in the area for over 100 years, implementation of the HCP/SYP's provisions cannot increase the level of cumulative impacts already present. This response overlooks the substance of previous comments, which discussed the cumulative impacts that would continue to accrue if the plan allows many of the last potential sources of old-growth woody debris to continue to be depleted. Furthermore, the response appears to misinterpret the requirements of HCPs and SYPs: in both cases, the intent is not simply to marginally decrease the rate of impact accumulation, but to establish a management plan which will not lead to destruction of public trust resources and damage to beneficial uses of water and which will not "appreciably reduce the likelihood of the survival and recovery of the species in the wild." In other words, the plan is not adequate if it allows impacts to continue to accumulate, albeit at a slower rate.

Pg.6, par.5(a) – The response suggests that interception loss data from the Oregon Cascades should have been cited in the comments. These data, however, had not been considered relevant to coastal California redwood forests because the winter climate in the Cascades is significantly colder than in the Freshwater area (Figure A1-1). Measured average temperature differences would result in a 3- to 6-fold higher evapotranspiration rate in the Freshwater area during the winter, as calculated

Table A1-1: Calculated daily interception loss for the storm of January 1995 at Eureka.

Date	Rainfall (inches)	Interception loss (percent)	Interception loss (inches)
5-Jan	0.32	31.6	0.10
6-Jan	0.40	29.8	0.12
7-Jan	0.51	27.9	0.14
8-Jan	1.43	21.1	0.30
9-Jan	1.97	19.4	0.38
10-Jan	1.01	23.2	0.23
11-Jan	0.57	27.1	0.15
12-Jan	0.37	30.4	0.11
13-Jan	0.92	23.8	0.22
14-Jan	0.49	28.2	0.14
Storm total	**7.99**	**23.8**	**1.90**

using the Thornthwaite method; differences of this magnitude indicate that rates of evaporation would be dissimilar. As explained in the comments, New Zealand data exist for large storms in a maritime climate very similar to that of Freshwater, and data available for PL lands in Freshwater are consistent with the New Zealand data. The New Zealand results for storms of greater than 5”—and the extrapolation of the data from PL lands—indicate a range of interception values of about 17% to about 27% for large storms in climates similar to those in question.

The respondent also suggests that the measured interception rate of 16.8% for a storm that dropped 3.34 inches in a 24-hour period at Caspar Creek be used as the maximum value in interception loss calculations. However, storm precipitation is made up of multiple days of rain, and flood magnitudes reflect not simply the intensity on the heaviest rain day, but a measure of the accumulated rain during the storm. The storm of January 1995 in Eureka provides an example of this pattern (Table A1-1, columns 1 and 2): an 8-inch storm occurred over a 10-day period wherein the largest daily rainfall was 1.97 inches. This storm caused flooding along lower Freshwater Creek.

In the case of the 1995 storm, the data cited by the respondent (16.8% loss for a 3.34” day and 21.4%, 23.7%, and 24.8% for 1” days) were regressed using a log-log transform to develop a relationship between daily rainfall and percent interception loss, with the form of the equation assumed to be the same as that found to apply to the other available data sets. Interception was then calculated for each day’s rainfall (Table A1-1, columns 3 and 4). Calculated losses varied between 19.4% for a 2-inch day and 31.6% for a 0.32-inch day, but the calculated overall interception loss for the storm was 23.8%. Results suggest that the value of 16.8% is actually likely to be a minimum value for a multi-day storm rather than a maximum value, as suggested by the response.

The response cites a 1972 study that questions the significance of rainfall interception studies, but the arguments provided do not appear relevant to this case. First, measurement accuracy is high for the study which provided the data cited by the respondent (J. Lewis, personal communication). Second, the argument that there is a trade-off between evaporation and transpiration does not dispense with the issue. If winter transpiration rates are high enough that their decrease offsets the interception loss, then those transpiration rates would need to be considered anyway: the storm rainfall disappears *during the storm* either through transpiration or evaporation. In either case, the amount of water available to contribute to peak flows is the same. Potential trade-offs between transpiration and evaporation may be more relevant to water yield calculations than to storm runoff calculations.

The response also notes that mean annual maximum daily rainfall at Kneeland is 4.19 inches and that six events greater than or equal to 7.0 inches have occurred since 1970. Using the relationship constructed from data quoted in the response, the predicted interception loss is 15.8% for a 4.19” daily rainfall and 13.8% for a 7.0” rainfall. These rainfalls, also, will be embedded in larger storms and surrounded by days of lower rainfall, so the total storm interception will be higher than these values would indicate.

The response also argues that the data from PL land in Freshwater do not encompass large storms. However, the other data sets cited—which do include larger storms—produced relationships that are nearly parallel to one another on log-log paper. There is no reason to expect that the data from PL land would show a relationship of different form, so their plotting location—sandwiched between data sets from New Zealand—suggests that their underlying relation also falls in the range of those from New Zealand.

Pg.6, par.5(b) – The response states that “stream discharge events that are expected occur annually or every 2 years are normal high flow events, and are not generally considered to be floods.” This is precisely the reason that it is so important to determine why storms that are recurring 3 to 8 times a year are now generating major over-bank floods in Freshwater Creek. As the response indicates, they should not be. The fact that a 2-year flood peak is increased by an average of 27% in clearcut watersheds at Caspar Creek provides an indication of why events that occur even more frequently are now producing floods in Freshwater. Furthermore, Figure 3 of Ziemer (1998) shows that the proportional increases in peak flows are relatively constant for discharges between 3 and 10 L/s-ha, with a value of 8 L/s-ha representing the 2-year flow at Caspar Creek.

The response also suggests that “evapotranspiration may be an important process throughout the winter period.” Irrespective of whether the observed 27% increase in 2-year flood peaks is due to foliage interception (measured) or evapotranspiration (hypothesized in the response), it will need to be taken into account by the SYP/HCP. It should be noted that decreased foliage interception would have the same effects on winter soil moisture and pore pressures as decreased evapotranspiration.

The response states that “8 inches of precipitation in 24 hours would likely result in little interception loss.” Application of the relationship constructed from the data cited earlier in the response indicates that an 8-inch, 24-hour rainfall would be associated with a 13.3% foliage interception loss—hardly inconsequential.

The response also states that “the 1-year peak in the 50% clearcut [entire gauged portion of the North Fork Caspar Creek basin] was increased by 9% after logging,” thus suggesting that the proportional relationship between percent clearcut and percent peakflow change defined by the Caspar Creek data is not valid. However, examination of Figure 6, provided with the original comments and again attached (Figure A1-2), indicates that a single point has been selected from among the 13 used to calculate the regression relation. The response implies that because this selected point does not fall on the regression line, the relationship is invalid. However, regression is a statistical tool that takes the variance of individual data-points (i.e., the divergences of individual points from the best-fit line) into account to define the best-fit line. It is not expected that any point should fall on the line, but that the variance about the line should be normally distributed, as is the case in this particular instance. Note, also, that the relation had been tested for a dependence on basin size, and this was not found to be a significant influence.

Pg.7, par.6 – The response argues that interception loss alone is not sufficient to explain the observed increase in peak flows after logging redwood forests. This would imply that the calculations provided in the original comments *underestimate* the magnitude of the expected impact on peak flows. The respondent's discussion of rain-on-snow flooding, although interesting, addresses a point not raised in the comments.

Pg. 8, par.4 – The response indicates that antecedent precipitation also influences landslide frequency during a storm of a given size, and implies that because this influence was not mentioned in the comments, the concept may have been "erroneously applied" in the comments. However, the dependence of landslide rate on antecedent precipitation, as well as its dependence on hillslope gradient, geology, vegetation cover, and so on, is well known among geomorphologists. These influences were discussed in the source document (Reid 1998b) cited by the comments, and in that document it was made clear that they can be treated as random influences that increase the variance of the relationships. Influences such as antecedent precipitation are thus accounted for implicitly in development of the relationship.

In the present application, however, consideration of antecedent precipitation is more complicated, as was also mentioned in the comments. Because rates of interception loss can be very high for small storms, effective antecedent precipitation can be much higher on logged slopes than on forested slopes. Thus, antecedent precipitation is no longer a random variable when expected landslide frequencies on logged slopes are being compared with those on forested slopes: as the volume of interception loss decreases, the antecedent soil moisture increases, and *both* of these effects would contribute to increased landslide hazard. The calculations reported in the comments thus *underestimate* the impact of logging on landslide frequency, and the issue raised by the response thus adds support to the concern raised by the comment. The influence of logging on soil moisture through changes in effective antecedent precipitation was discussed in the paragraph of the comments immediately following that considered by the response.

Pg.8, par.5 – It is not clear why the response cites rainfall interception data from the Oregon Cascades, which are known to have a very different climate than the redwood belt in California (see Figure A1-1), rather than citing the data from areas in New Zealand that have winter climates very similar to that of California's northwest coast. The response also notes that losses for large storms are less than for small storms, but this pattern is not relevant to the issue raised by the comment. The comment had indicated that a decrease in the total seasonal interception loss is the factor likely to be important for increasing the activity of earthflows and deep-seated slides, rather than the interception loss during a particular storm. The response does not address the issue raised by the comment.

Pg.9, par.2 and 3 – The response has confused two different issues here. The comments describe the process of "aggradation" as follows: "Some of the sediment will then be deposited downstream..., thus contributing to aggradation of the downstream reaches.... Aggradation at these sites will decrease channel capacity...." In other words, aggradation fills in channels with sediment over a period of time. The following sentence describes the process of "scour": "Sediment is scoured from channel beds during periods of high flow, and new sediment is deposited to take its place as flows recede." In other words, scour refers to the mobilization of the channel bed during a high flow, but this scour is not necessarily evident as a change in bed level at the end of a storm. If aggradation is gradually filling a channel with sediment of sizes smaller than the original bed, the characteristic depth of scour during storms is likely to increase; this pattern was discussed by the original comment.

Pg.9, par.8 – The response appears to interpret the documented 27% increase in 2-year flood peakflow after logging as "relatively benign." Clearly, those people who are damaged by increased flooding would not consider such an increase in flooding "benign." The respondent further implies that because field studies relating logging-related peak-flow increases to degradation of fish habitat have apparently not been carried out, the impact can be ignored. However, the fisheries literature includes studies that document the impact of high flows on fish and fish habitat (e.g. Thorne and Ames 1987), and data from a California redwood forest demonstrate that peak flows increase in logged watersheds (Ziemer 1998). If peak flows are increased by logging and high peak flows damage fish, then it is reasonable to assume that logging can potentially harm fish through increases in peak flows.

Pg.10, par.2 – The response suggests that the increased concern over flooding simply reflects construction of new houses within the 100-year floodplain since 1975. However, two of the homes most impacted by the recent flooding were built in 1951 and 1895. Further, many people consider it worth the risk to construct a house at a location considered likely to flood an average of once in 100 years; entire cities, after all, are built in locations likely to experience devastating earthquakes or hurricanes within the next 100 years. These same people would not be likely to consider it worth the risk to construct a house on a 10-year floodplain. If the frequency of flooding on a 100-year floodplain is altered, by upstream land-use, to once every 10 to 20 years, it is not reasonable to blame the victims of the flooding rather than those responsible for increasing the flood hazard after the homes were constructed.

Pg.10, last par. – The response defends the plan against a concern that was not raised by the comment quoted. The final statement of the response notes that "...total tree cover is not always required to provide sufficient root strength to prevent an increase in landslide occurrence." The comment, however, did not imply that total tree cover is necessary. The response also states that "published literature can only document a correlation between landsliding and clearcuts on steep slopes." The response does not cite the landslide studies carried out on PL land in Bear, Jordan, and North Fork Elk watersheds (PWA 1998b, 1999, 1998a), which provide data that document increased landslide rates by factors of 9.6, 3.3, and 13, respectively, on lands logged within the past 15 years in those watersheds. Most of this logging was done by less intensive methods than clearcutting.

Pg.11, par.3 – The response documents the general pattern of increase in landsliding rate with clearcutting, which supports the argument provided by the comment, but the response then accuses the commentor of having "selectively cited the highest values." However, the values reported were described by the commentor as being the highest values: "Sidle et al. (1985) document as much as a 41-fold increase in landslide frequency after clearcutting and a 346-fold increase after road building." Had I not intended to cite the maxima, I would not have used the words "as much as," and had I reported anything less than the maxima after having used those words, I would have been misrepresenting the content of the paper.

Pg.11, par.4 and par.5 – The response states that the calculations included in the comment did not account for the fact that some landslides on logged lands would have occurred even without the logging. However, this effect had been taken into account by the calculations. When a ratio between two values is presented, the ratio describes the magnitude of one value relative to the other. Thus, the 9.6-fold increase in landsliding rates on recently logged lands in Bear Creek is an increase relative to that on lands logged more than 15 years before. In other words, if 10 yd^3 of landsliding had occurred per acre on 30-year-old forest and 96 yd^3/acre had occurred on recent logging, the ratio between the values would indicate a 9.6-fold increase *relative to the rate on 30-year-old forests*. The statement that the comment attributes "all landslide sediment delivery from within the harvested area to the impacts of recent logging" is thus not true.

Second, the response's suggestion that the strong association between landsliding and logging might not have been a result of the logging would, if true, require that those lands recently logged be abnormally prone to landsliding for other reasons. However, earlier examination of aerial photographs had disclosed no apparent bias in the location of recently logged units toward slide-prone lands. In fact, Spittler (1998) indicates that "there is no substantial difference between the harvested areas in subwatersheds with channel inner gorges and the subwatersheds where harvesting has not occurred within the past 15 years" and that "The functional homogeneity of the geology and geomorphology of the Bear Creek watershed allows for a more simplified assessment of the role of timber management on landslides...."

The response also argues that if the largest recent landslide in the Bear Creek watershed, which was located on a recently logged slope, had not been located on a recently logged slope, the results would have showed a lower rate of landsliding on recently logged land. On the face of it, this is a tautology: if an observed event had not occurred, it would not have occurred. However, in reality the respondent raises a good point: if the results of the calculation are unduly influenced by the occurrence of a single slide, then the results are not very robust. Consider the following calculations, however.

With the major slide (33% of total slide delivery) included, 85% of the landslide sediment came from the 37% of the watershed that had been recently logged, and 15% came from the 63% of the watershed that had last been logged more than 30 years previously. The relative rate between recently logged land and older logged land is calculated as:

$$\frac{\left(0.85/0.37\right)}{\left(0.15/0.63\right)} = 9.6,$$ or a 9.6-fold increase relative to the rate on 30-year-old forests.

If the major slide is not included, then the amount of sediment derived from landsliding on recently logged lands becomes 52% (i.e., 85% less 33%) of the original total, while that from older logged lands remains 15% of the original total. The ratios can then be recalculated:

$$\frac{\left((0.85-0.33)/0.37\right)}{\left(0.15/0.63\right)} = 5.9,$$ or a 5.9-fold increase relative to the rate on 30-year-old forests.

The results of this calculation demonstrate that the overall conclusions are not particularly sensitive to the occurrence of the major slide, and that even without that slide, there would have been a 5.9-fold increase in landsliding rates on recently logged land relative to those on lands logged more than 30 years previously. Even if the major slide had occurred on 30-year-old forest, the resulting calculation would show that the rate on recently logged land is 1.8 times higher than that on 30-year-old forest:

$$\frac{\left(0.52/0.37\right)}{\left((0.15+0.33)/0.63\right)} = 1.8,$$ or a 1.8-fold increase relative to that on 30-year-old forest.

Given these results, it is not clear what the basis is for the respondent's conclusion that "there would be no apparent effect of harvesting" if the occurrence of the largest slide is not considered.

The response goes on to describe the results of a report by PWA (1999) in Jordan Creek. Because federal agency personnel, state agency personnel, and the public had not been allowed to see the cited report until late January, one of two things appears to have occurred: either the respondent had not seen the report, or PL had allowed the respondent to see the report before releasing it for public and agency review. In the first case, the source of the information concerning the report's contents should have been cited rather than the report itself. In the second case, the respondent would have found that the contents of the report were not as the response has described. In either case, the response has misrepresented the contents and conclusions of the report. The response cites the conclusion that 85% of the landsliding in Jordan Creek occurred in the 50% of the basin that had been logged prior to the recent cutting cycle. In actuality, the report found that 77% of the recent landslide sediment delivery was from the 50% of the watershed logged in the previous 15 years, demonstrating an overall 3.3-fold increase in landsliding rate with respect to rates on older logged lands. Thus, the pattern of association between logging and landsliding is very similar to that in the adjacent Bear Creek watershed.

Interestingly, the authors of the Jordan Creek report are also quoted misrepresenting their own results: "...we discovered harvest and landslide associations that directly and dramatically contradicted those encountered in Bear Creek" (PWA 1998c, quoted on Pg. T-188, par. 5 of the response to comments). It is highly unfortunate that release of the report was delayed until the actual results could not be used to inform preparation of the FEIS/FEIR, and that the misrepresentations proffered by PWA (1998c) and Dr. Opalach (1998) of Pacific Lumber Company, and now by the official response, could thus stand unchallenged until the FEIS/FEIR was completed.

Pg.12, par.5(a) – The response does not address the issue raised—that the 1964 storm had the most profound impacts in heavily logged basin, and that considerably less landsliding was generated by a similar storm that occurred before logging began in the Redwood Creek basin (Harden et al. 1995)—and instead notes that landslides and channel disruption also occur under natural conditions. The comment, however, was not to suggest that such processes do not occur naturally; the comment, in fact, cited the occurrence of such a natural event. Instead, the comment was citing evidence that the magnitude and frequency of the changes are greater after logging than under natural conditions.

Pg.12, par.5(b) – The response simply alludes to the earlier response to Pg.3, par.2; examination of the earlier response shows that it did not adequately address the issue because it misrepresented the original comment.

Pg.12, par.5(c) – The response simply alludes to the earlier response to Pg.1, par.4; that response is a description of California Forest Practice laws and does not address the issue raised.

Pg.12, par.6 to Pg.13, par.1 – The response underestimates the competence and professionalism of the CDF employees who remeasured the cross-sections. The first cross-section was located at the closest approach of the channel to Freshwater Road; the second was located 600 feet upstream of a confluence, and the third was 50 feet upstream of a bridge. Channel form at each site is relatively uniform along the reach. None of these sites would be hard to locate to within 100 feet, and remeasurement anywhere within a 100-foot zone would be expected to show similar patterns.

The response indicates that there are no large, recent landslides upstream of the middle cross section, at which aggradation was noted, and on that basis reasons that "mobilization of stored sediments in the Freshwater Creek channel" is likely to be a more important reason for the observed aggradation in the middle and lower cross sections than recent sediment inputs. This hypothesis, however, is inconsistent with the field evidence. Recent aggradation at both the middle and lower cross-sections has been due primarily to the accumulation of silt- and sand-sized sediment on channel banks; the downstream cross section shows essentially no change in the maximum depth of the channel. These sediments would not have been stored in any great amounts in the Freshwater Creek channel, and the volumes of sediment available for mobilization in the Freshwater Creek channel would not have been sufficient to produce the magnitude of aggradation observed without producing some major incision along upstream channel reaches.

An alternative explanation had been suggested by the field evidence and was described in the comments on Pg.37, par.4: "Any aggradation at the mouth of Little Freshwater is thus likely to cause a backwater effect at XS-5, thereby promoting deposition of sediment." In other words, the excessive landsliding that occurred along Little Freshwater Creek, which joins Freshwater Creek 600 feet downstream of the cross section in question, would have been likely to aggrade the mouth of Little Freshwater. Aggradation of a foot of sediment at the confluence would be enough to provide a net bed gradient of zero between the XS-5 and the confluence. This reduced gradient would slow down flow at the site of the cross section and would thus accelerate deposition of the sediment load.

Meanwhile, abundant fine sediment has been introduced into the Freshwater channel system in the recently heavily logged Little Freshwater, Graham Gulch, and Cloney Gulch watersheds, and recent aggradation of the latter two channels by coarser sediment is also evident. Highly turbid water is regularly observed in these two channels during and after storms. These excessive loads of fine sediment from the recently logged tributaries would provide a very effective source for the fine sediments accumulating on the banks at the middle and lower cross sections.

Pg.14, par.4 – The first part of the response again alludes to the earlier response to Pg.3, par.2; examination of the earlier response showed that it had misrepresented the original comment and thus had not addressed the issue raised. The second part of the response again cites the response to Pg.1, par.4, which was simply a description of California Forest Practice laws and therefore also did not address the issue raised.

Pg.15, par.1 – The response states that the work required to test the landslide hazard rating system would take several years. This is clearly not true. Most of the work has already been done in the form of the landslide maps produced by PL contractors for Bear Creek, Jordan Creek, Stitz Creek, North Fork Elk River, and Freshwater Creek. It would now take no more than a few weeks to analyze the data. At the very least—and requiring less than a week's work—the distributions could be compared with existing GIS coverage of the landslide hazard zones. Not only is this "reasonably feasible and necessary", as required by the Forest Practice Rules, it would ordinarily be considered odd if such a step were not undertaken: how could a classification system for landslide susceptibility have been developed without ever having tested the classification to determine whether it reflected landslide susceptibility? It is disconcerting that staff of a regulatory agency would not have considered such a test to be "necessary."

Pg.15, par.4 to pg.16 par.3, and Pg.iv par.3 – The response presents the argument that THP 1-97-307 HUM did not need to follow the mass wasting avoidance strategy because it was originally submitted before development of the mass wasting avoidance strategy, and that recommendations for alterations to the THP to address slope-stability concerns were made *by DMG geologists during THP review*. This argument, however, further supports the points made by the original comment. Although THP 1-97-307 was originally submitted prior to development of the mass wasting avoidance strategy, it was not approved until several months after the mass wasting avoidance strategy was adopted in the pre-permit agreement between agencies and PL. The agencies and the company had agreed that the terms of the pre-permit agreement would be applied to this plan. Had the mass-wasting avoidance strategy been effective, the resulting alterations in the revised plan (revised maps and text dated April 1997) would have been sufficient to decrease slope-stability concerns to a level lower than that ordinarily seen in THPs *not* subject to the mass-wasting avoidance strategy. However, because the results of the application of the mass wasting avoidance strategy did not reduce the concerns to a level acceptable to DMG, DMG had to request additional changes to the THP "during the THP review process." In other words, adherence to the mass-wasting avoidance strategy resulted in a plan draft that did not meet the minimum standards of the regulatory agencies for properties that do not have a mass wasting avoidance strategy.

A second point raised by the response concerns the acreage to be clearcut in THP 1-97-307. The silvicultural prescription described on the 10/24/97 revision of page 5 of the plan (THP 97-307 p.5) is identical to that on the version of the same page revised on 6/11/98, which accompanies the revised landslide hazard map (THP 97-307 p. 122.7, revised 6/11/98). Clearly, the application of the mass-wasting avoidance strategy resulted in a plan draft that showed the intent to clearcut the same acreage as had originally been proposed before the mass-wasting avoidance strategy existed. Any changes made were thus made due to recommendations of regulatory agencies, and did not follow from application of the mass wasting avoidance strategy.

Third, the response states that none of the proposed clearcut is in the inner gorge or on headwall swales. However, comparison of the map of silvicultural prescription (THP 1-97-307 p.15, revised 10/24/97) and the map of inner gorges included with the plan (THP 1-97-307, p. 122.7, revised 6/11/98) indicates, instead, that all parts of the inner gorge that fall beyond the Pre-Permit Agreement WLPZ will be either selectively logged or clearcut. In addition, 18 of the 60 or so acres mapped as "headwall swale areas" are to be clearcut, and the remainder are to be either selectively cut or shelterwood prepped. If these prescriptions have changed, it was not through application of the mass-wasting avoidance strategy but through intervention of the regulatory agencies.

Fourth, the response uses a quotation from Dr. Ray Wilson to support the contention that Dr. Wilson had examined THP 1-97-307 and had found slope stability provisions to be adequate for that plan. That quotation has been altered by the respondent to misrepresent Dr. Wilson's conclusions. In reality, Dr. Wilson had specified that his comments concerned only THP 1-96-413, stating that "...this 'consensus' applies only to THP 1-916-413, and does not constitute a 'blank check' for any other THP or area."

The original quotation (Wilson 1998) reads as follows, following a paragraph description of conditions on Unit A and Unit B of THP 1-96-413 and of the planned modifications to those units: "**These modifications** were apparently the results of a dialog between PALCO and their consulting geologist, Timothy Best" (emphasis added). The respondent modified the original quotation by adding words which suggest that Dr. Wilson's comments referred to both THPs. The altered "quotation" reads: "**modifications (to the THPs)** were apparently the result of a dialog between PALCO and their consulting geologist..." (emphasis again added). The respondent was able to support the respondent's conclusion *only* by doctoring the quote. This is not acceptable.

Fifth, the issues raised by the comments were not contingent on the presence of the commentor at the THP site, and it was nowhere implied that the commentor was present. The response that "At no time during these inspections was Leslie Reid present" is thus irrelevant.

Sixth, the Bear Creek report states that "Approximately 85% of the 1996/97 landslide sediment delivery came from this 37% of the watershed" (PWA 1998b, Pg.18, par.4) where the 37% referred to is the portion that had been logged in the previous 15 years. The mathematics required to compare the rates described by that statement (85% per 37% of the land versus 15% per 63% of the land) are obvious and simple:

$$\frac{\left(0.85 / 0.37\right)}{\left(0.15 / 0.63\right)} = 9.6,$$ or a 9.6-fold increase relative to the rate on 30-year-old forest land.

Thus, the statement on Pg.18, par.4 of the Bear Creek report demonstrated that rates of landsliding on recently logged land are 9.6 times higher than those on lands last logged 30 years ago. The response that the “Reid’s conclusions regarding a 9.6-fold increase in landsliding rate are nowhere documented in that report” is thus unfounded. The response also refers to the “previous response to issue on page 11, paragraph 3” in support of that statement. However, examination of that response indicates that it has nothing to do with the issue raised by the comment, which is that measurements of landsliding rates at an analogous, nearby site existed and were ignored by those preparing THP 1-96-307.

Seventh, the response concludes that “...the conclusions reached by Reid...are either erroneous or based on incomplete information as illustrated in the examples she uses.” Given that none of the rebuttals prepared by the respondent are valid (see above discussion), the respondent’s claim is unfounded.

Pg.16, par.2(a) – The response further supports the substance of the comment by indicating even more clearly that experts in geomorphology and hydrology and landscape-scale slope stability issues who do not happen to have a California state license in geology will not be allowed to prepare “reports, documents, or exhibits” regarding the issues under discussion. DMG have made clear through other channels that such a proscription extends even to the preparation of reviews, such as this one, of public-record documents. As an “unlicensed” expert in geomorphology and landscape-scale slope stability, I technically may be liable for a $2500 fine for having done so.

At this point, we know from the reports of landslide incidence in Bear Creek, North Fork Elk River, and Jordan Creek that the strategies used by state licensed geologists to avoid excessive landsliding associated with logging have not worked. The point of the original comment was that it is unreasonable to exclude those who have a broader expertise—those who carry out research on landsliding and on related aspects of geomorphology, forest ecology, and hydrology but do not hold California geology licenses—from contributing to the solution of the existing problem.

The response then states that the original comment “is based on an unreasonable expectation that DMG geologists or any other geologists are able to ‘prevent’ landsliding.” However, the expectation that geologists can prevent all landsliding is neither stated nor implied by the comments; the comments, instead, state that those geologists have been “unable to prevent the 9.6-fold to 13-fold increase in landsliding rates that is currently occurring.” This statement is based on the documented rates of landsliding reported by PWA 1998a and PWA 1998b. The comment thus focuses on the need to prevent *increased* rates of landsliding, which is an objective also incorporated into the original statement of the mass-wasting avoidance strategy. The comments, in fact, base calculations on an expectation that some level of increased landsliding is inevitable (see Appendix 5 of the original comments).

The response concludes with the statement that the issues raised by the original comment are “...especially questionable under the unproven 9.6-fold to 13-fold increased rates she is advocating.” First, I am not “advocating” increased rates of landsliding. Second, I simply reported the results of the calculations implicit in statements contained in the cited reports. If the response refers to these as “unproven,” the response is challenging the validity of the cited reports, not the validity of the calculations they lead to.

Pg.16, par.2(b) – The first part of the response is based on reference to an earlier response, which was demonstrated above to have been invalid (Pg.11, par. 4 and 5). The second part of the response misrepresents the original comment and then addresses the misrepresentation. The comment neither stated nor implied that "state licensed geologists are unable to identify and develop mitigations for mass movement problems." The comment, instead, made the point that the strategies that have been and are being used by state-licensed geologists have resulted in documented 9.6-fold and 13-fold increases in landsliding rates associated with recent logging.

Pg.16, par.2 and pg.17, last par. – The response relies on quotations from a document prepared by a consultant hired by PL. Most of the content of the response was shown to be invalid under the discussions of the response to Pg.15, par.4 to pg.16 par.3, and Pg.iv par.3 (above) and to Pg.11, par.4 and par.5 (above). The response concludes with a personal attack on the commentor through the use of a direct quotation from a PL consultant.

It is odd that the response would rely upon—and reproduce without question—an unsupported allegation of scientific impropriety from a PL consultant rather than carry out the simple statistical analysis that demonstrates that the allegations are unfounded. The response should have noted that the report produced by the quoted PL consultants provided all of the information needed to infer causality. First, the association between landslide distribution and recently logged areas is easily shown by a chi-square test to have a less than 1 in 10,000 chance of being produced randomly. If the distribution of recently logged areas is characteristic of the rest of the watershed in terms of potential to generate landslides, then causation can be inferred: the landslides are where they are because that is where the logging occurred.

That lands on which the recent logging occurred are characteristic of the remainder of the watershed is evident on the basis of examination of air-photos, topographic maps, and geologic maps. This conclusion was supported by the DMG review of the Bear Creek report: "For assessment purposes, there is no substantial difference between harvested areas in subwatersheds with channel inner gorges and the subwatersheds where harvesting has not occurred within the past 15 years" (Spittler 1998, p.3 par.5). The means to statistically test the representativeness of the recently cut lands is provided by the map of landslides present in 1966, when essentially the entire basin had been logged. If the landslide frequency in the 37% of the watershed that would later be cut in the 15 years preceding 1997 is the same as that in the 63% of the watershed that would not be cut again by 1997, then the two areas are functionally the same with respect to the potential for landslide generation. A chi-square test for landslide distribution in 1966 demonstrates that landslide frequencies are not significantly different between the two areas at the 95% confidence level. Thus, the difference in later landslide rates can be attributed to the treatment variable, the logging.

In this case, had the respondent read the source documents, the respondent would have seen that the quotation from the PL consultant was unwarranted. The PL consultant had carried out an erroneous analysis, and had concluded that "It is reasonable to assume that a significant portion of this reduction [in landsliding] can be attributed to the increased protection afforded by implementation of the Forest Practice Rules..." (PWA 1998b, p.25). The corrected analysis showed, instead, that rates of landslide sediment production on recently logged lands were 9.6 times higher than on older logged lands, and that rates of landsliding on recently logged lands during recent storms were 1.5 times higher than rates during storms before forest practice rules were implemented. If it had been "reasonable" for the PL consultant to assume, on the basis of the erroneous analysis, that Forest Practice Rules had been responsible for a (non-existent) decrease in landslide rate, then it was even more "reasonable" for the assumption to be made, on the basis of the corrected analysis and on observation of the distribution of logged areas, that land use governed by the current Forest Practice Rules was responsible for the documented increase in landsliding rate over background rates.

Pg.16, par.3 – The response does not address the issue raised by the comment, and instead provides arguments that institution of a storm-proofing program is desirable.

Pg.16, par.4 – The response relies on reference to a previous response (Pg. 11, par.4 and 5), which has been shown to have been an invalid response. The central issue of the comment is thus not addressed: data from Bear Creek and North Fork Elk River demonstrate that even if all road-related landsliding had been prevented through storm-proofing in those watersheds, it would not have offset the increased landsliding associated with logging.

Pg.17, par.1 – The response does not fully address the issue raised. Although the maximum limits have been increased, the problem still exists: the company cannot be “compelled” to exceed the new maximum limits.

Pg.17, par.2 – The response indicates that the results of Megahan’s work are from an area “with much higher surface erosion potential than soils located in the HCP/SYP area.” However, examination of the soil erosion hazard map in the HCP/SYP indicates that some soils in the HCP/SYP area are mapped as having “extreme” soil erosion hazard. Discussion of the remainder of the response is moot, as additional protection has been accorded the Class 3 channels; however, it must be noted that overland flow is evident during storms on logged areas visible from public roads in Freshwater watershed.

Pg.17, par.4 – The comment raises the issue that a proportion of the documented landslides occur on slopes that would not require geological review under the plan. The response addresses the issue by noting that “identification of unstable areas and slides is required by the Forest Practice Rules.” However, the Forest Practice Rules define unstable areas as areas that already display evidence of failure. In the case of logging-related shallow slides on planar slopes, many of these sites do not become unstable until they are logged; they are not “unstable” at the time of geological review. They are also not located in inner gorges or headwall swales and so do not fall in areas mapped as having “extreme” mass wasting potential. The draft plan’s mass wasting avoidance strategy would not call for the examination of these sites. The response also relies on an earlier response to Pg.11, par.4-5; this earlier response has been shown to have been in error.

Pg.17, par.4 to pg.18, par.1 – The comment provides substantive evidence that the disturbance index approach is not valid. The response first relies upon reference to a response to Pg.11, par.4-5; that response has been shown to be erroneous. The response then indicates that, in the final plan, the areas over which the disturbance index will be calculated are smaller than in the draft plan. This change does not address the problems that had been described by the comment. The response also provides no indication that use of smaller areas will negate the concern that existing habitat degradation could not have been avoided through use of the disturbance index.

Pg.18, par.2 – The response addresses the details of the comment, rebutting the statement that “...the HCP/SYP appears to call for a long-term conversion of vegetation from a redwood dominated to a Douglas-fir dominated forest” by demonstrating that the proportional volume of Douglas fir changes from 30.7% to 48.5% over 12 decades, and thus does not quite achieve Douglas-fir dominance. However, the substance of the comment was that a shift toward a higher proportion of Douglas-fir being logged will increase the impact of logging on loss of root strength on hillslopes because Douglas-fir roots do not remain alive after logging. The substance of the comment was thus not addressed by the response.

Pg.18, par.3 – The comment raises the problem that fine sediment is brought to the surface of roads by traffic between rainstorms, so that proscriptions on traffic during storms will not be sufficient to adequately curtail road-surface erosion. The response states that the forest practice rules and HCP/SYP mitigations are designed to "...prevent sediment transport of materials from the road into streams..." and that "The problem of road-related sediment production will also be mitigated by wet weather road use restrictions that are included in the final HCP/SYP." However, the FPRs and HCP/SYP clearly cannot "prevent" transport of sediment from roads to streams, and examination of the new provisions indicates that wet road-beds will continue to be trafficked between storms. The issue raised by the comment was thus not addressed.

Pg.18, par.4 – The response relies on references to previous responses to Pg.3, par.4 and Pg.12, par.6 to Pg.13, par.1, but examination of these responses indicates that neither adequately addressed the issues raised. Second, the statement that it is unlikely that the plan will cause impacts to be worse than they are at present is irrelevant, because existing practices have been demonstrated by the CDF, through CDF's declaration of 5 of the watersheds to be cumulatively impacted, to be inadequate. A plan cannot simply be designed to be less inadequate than previous measures; it must be designed to be adequate to achieve the intended outcomes.

Pg.18, par.5 – The original comment provided supporting documentation to show that even an equipment exclusion zone is not sufficient to reduce the level of cumulative impacts to an acceptable level. The response simply argues that keeping heavy equipment away from creeks creates a lower level of impact than allowing machinery to work near the creeks. This response overlooks an important part of the definition of cumulative impacts: "Cumulative impacts can result from individually minor but collectively significant actions taking place over a period of time" (Council on Environmental Quality Guidelines, 40 CFR 1508.7, issued 23 April 1971). Whether a measure is marginally better than previous measures is not relevant; instead, the measure must be adequate to avoid the cumulative impact of concern.

Pg.18, par.6 – The comment notes that, on the basis of evidence described in earlier sections of the comments, provisions of the plan are not sufficient to reduce the level of chronic turbidity impact to salmonids to an acceptable level. The response, however, simply states that FPRs and the HCP/SYP are designed to "prevent the discharge of sediment into watercourses in amounts that would have adverse impacts on salmonids." They may be "designed" to do so, but the available evidence suggests that they are not capable of doing so.

Pg.19, par.4 – The response relies on an earlier response to Pg.11, par.4 and 5, but this response has been shown to be in error. When the association between landslide distribution and recent logging is so strong that there is less than a 1 in 10,000 chance that it is "accidental," and when the staff of the DMG have indicated that "The functional homogeneity of the geology and geomorphology of the Bear Creek watershed allows for a more simplified assessment of the role of timber management on landslides..." (Spittler 1998), it is disconcerting that the respondent is requiring "proof" of causality before the information can be used to inform management decisions. It would be interesting to learn what kind of information would be considered sufficient by the respondent to allow the respondent to act on the assumption that recent logging in Bear Creek watershed did cause an increase in landsliding.

Pg.19, par.5 – The response again relies on the earlier response to Pg.11, par.4 and 5, but that earlier response has been shown to be erroneous.

Pg.20, par.3 to Pg.21, par.7 – The comment provides substantive evidence that the plan will contribute to extinction of the ESU of coho by furthering habitat degradation, but the response does not address the issue raised. Instead, the response seems to indicate that if the plan is incrementally better than existing regulations, then all concerns about the impacts of the plan are negated: "Implementing these additional mitigations under the HCP/SYP will not lead to greater cumulative impacts than on-going operations that do not include these measures." This response suggests that the agencies assume that "better than nothing" is the standard against which the provisions of an HCP and SYP are to be judged, but examination of the requirements of each demonstrate that each must be judged relative to the impacts they produce, not relative to levels of protection previously in place.

Pg.21, par.8 to Pg.22, par.1 – The comment provides substantive evidence that the plan does not adequately evaluate the cumulative impacts that will accrue from the plan, and that this lack demonstrates the insufficiency of the plan with respect to the requirements of Forest Practice Rules. The response states that the final HCP/SYP must meet requirements of the Forest Practice Rules if it is to be relied on for THPs, and a subsequent response indicates that CDF has judged the plan to be sufficient (Pg.26, par.2). The final plan, however, does not incorporate changes that would be adequate to counter the cumulative impact concerns raised by the comments: management is still based on a demonstrably invalid disturbance index. The response thus fails to address the issue raised by the comment.

Pg.22, par.2(a) – The response again relies on the earlier response to Pg.11, par.4 and 5, but that earlier response has been shown to be erroneous.

Pg.22, par.2(b) – The response does not address the issue raised by the comment, which was that the mitigations used will not necessarily take place in the watershed in which a plan's sediment will be discharged, and thus will not mitigate the impacts of the plan. The response simply repeats the information that raised the original concern: mitigations are not required to be located within the watershed being impacted.

Pg.22, par.2(c) – The response disregards the fact that the road-based mitigations being discussed *are* the mitigations that are being used to off-set erosion from proposed silvicultural activities (see, for example, the mitigation plans developed for THP 1-97-307 and THP 1-96-413); the response's statement to the contrary is not true. Further, the response again relies on the earlier response to Pg.11, par.4 and 5, but that earlier response has been shown to be erroneous.

Pg.22, par.3(a) – The response relies on the earlier response to Pg.6, par.5, but that earlier response has been shown above to be inadequate. Further, the mitigations outlined by the plan for woody debris were demonstrated by the original comments, with supporting evidence, to be inadequate to satisfy even the levels of input *assumed by the plan* to be necessary and expected. The response cites earlier responses to "issues on pages 3 through 6," but, as shown above, these earlier responses did not adequately address the issues raised.

Pg.22, par.3(b) – The comment provided substantive evidence to show that mitigations that do not fully cancel out impacts will allow the accumulation of impacts through time by prolonging existing impacts, and that the plan's mitigations do not fully cancel out the impacts. The response does not address the issue raised. The response first implies that downstream conditions are currently recovering, but this has been demonstrated to be untrue by the recent listing of many of the waterways under consideration to be cumulatively impacted and to be impaired by excessive sediment. The response then asserts, without supporting information, that the provisions of the plan will not lead to an aggravation of existing impacts because provisions include "additional mitigation measures."

Pg.22, par.4(a) – The issue raised by the comment is that the method to be used for cumulative impact assessment does not, in fact, assess cumulative impacts, but instead merely designs BMPs. The response first explains that the planned method does not evaluate cumulative impacts and will result in the development of BMPs, and then asserts that information developed during the process "can be used, however, to meet these requirements." Saying that the information produced *can* be used to assess cumulative impacts avoids the essence of the issue: that the method *does not* assess cumulative impacts.

Pg.22, par.4(b) – The response addresses the issue raised by the comment—that it is not sufficient to base the cumulative impacts portion of the SYP on an appeal to the future design of a cumulative impacts procedure that will be based on an existing method that does not address cumulative impacts—by stating that the final HCP/SYP must contain adequate provisions if it is to be relied on for THPs. However, a subsequent response indicates that CDF has judged the plan to be sufficient (Pg.26, par.2), even though the final plan contains the same provisions that raised the concern in the original comment. The issue raised by the comment was thus not addressed by the response.

Pg.23, par.2 – The comment points out that the disturbance index is based on an arbitrarily constructed procedure that has never been tested. The response simply notes that the Forest Practice Rules state that "the sufficiency of the information provided in a SYP...shall be judged in light of what is reasonably feasible and necessary," thereby implying that the regulatory agencies consider the disturbance index to have satisfied this requirement. It is worrisome that a regulatory agency would not consider it "necessary" for a method proposed for adoption to be shown to be based on justifiable assumptions, given adequate technical review, and tested against available data sets before it is accepted. The substantive evidence provided by the comment to show that the disturbance index is inadequate is ignored by the response.

Pg.23, par.3 – The comment provides substantive evidence that the assumption that all impacts recover after 10 years will necessarily lead to accumulation of impacts. The response, however, simply states that sites are "most susceptible to surface and mass movement erosion" during the first 10 years. This response does not address the issue raised, and it suggests that agency staff may have misunderstood the importance of the fact that even residual effects can accumulate: "Cumulative impacts can result from individually minor but collectively significant actions taking place over a period of time" (Council on Environmental Quality Guidelines, 40 CFR 1508.7, issued 23 April 1971). Furthermore, that the decision is said to based on the assumption that sites are "most susceptible to surface and mass movement erosion" during the first 10 years indicates the arbitrary nature of the decision: the same could be said for the first eight years, the first 20 years, the first 50 years, and so on. Rates of erosion tend to decrease with time after disturbance. Selection of an arbitrary threshold for which recovery is then assumed to be complete cannot be justified in an area for which existing cumulative watershed impacts are acknowledged to exist by the regulatory agencies, and where those impacts are causing downstream property damage. In addition, Helvey (1967) provides evidence for white pine forests in the eastern US that hydrologic recovery is no more than 58% complete after 10 years and 73% after 35 years.

Pg.23, par.4 – The comment provides substantive evidence that the disturbance index is not capable of addressing the range of impacts that might occur, and comments also demonstrate methods that can be used for evaluation of particular cumulative impacts. The response simply asserts that no method is available that will address the range of impacts that might occur, thus disregarding the analyses actually carried out for the comments, and notes that the Forest Practice Rules have accepted the ERA approach. The response does not address the substance of the comment, and further overlooks the point—raised earlier by the comments—that the disturbance index will not be as effective as the ERA method in managing impacts.

Pg.23, par.6 – Thank you.

Pg.24, Item 3 – The response relies on earlier responses to Pg.11, par.4 and 5, to Pg.6, par.5-9, and to Pg.23, par.4-6. However, these earlier responses have been shown to be erroneous. The comment was thus not addressed by the response.

Pg.24, par.4 and 5, and Pg.24, par.5 to Pg.25, par.1 – The comment provides substantive evidence to demonstrate that the trend of habitat degradation associated with declining coho populations will continue under the proposed SYP/HCP. The response simply states—despite the available evidence to the contrary, as described in the original comments and indicated by the CDF's declaration that five of the watersheds under consideration have been cumulatively impacted—that habitat conditions are improving and that, since the SYP/HCP provisions are better than those in the Forest Practice Rules, the SYP/HCP cannot make the situation worse. This response ignores the substance of the comments and fails to address the issue raised: if the plan is not well-enough designed to reverse the trend of habitat degradation, it is not sufficient under the terms of the HCP legislation.

Pg.25, par.4 – The response indicates that the final draft will contain additional discussion of cumulative impacts, but a general discussion would not address the issue raised by the comment: that the SYP/HCP contains no analysis of cumulative watershed impacts, and it contains no discussion of cumulative watershed impacts as a changed and unforeseen circumstance. It would have been useful if the response had indicated whether or not the "additional discussion" would be included in a manner that would be relevant to the issues raised by the comment.

Pg.25, par.5 – The comments provide substantive evidence that institution of the provisions of the SYP/HCP will provide a higher likelihood that coho salmon of this ESU will become extinct. The response simply asserts that "additional mitigation measures specified in the HCP/SYP will lead to additional reductions in ongoing downstream impacts." This response disregards the evidence provided by the comments that the draft plan would lead to *increased* downstream impacts, and fails to acknowledge that the projected continuation of habitat degradation would not satisfy the requirements of HCPs and SYPs.

Pg.26, par.2 – The comments provide substantive evidence that the draft plan does not satisfy the state's requirements for SYPs. The response simply states that CDF has reviewed the final draft of the plan and has found it sufficient.

Pg.26, par.3 – The comments provides substantive evidence that the plan will not adequately evaluate cumulative impacts, and that the disturbance index method is not a valid method for cumulative impact evaluation; the calculations for the SYP projections are thus not valid. The response merely states that results of the disturbance index method are taken into account in the SYP projections. This response does not address the issue raised by the comment.

Pg.26, par.4 – The comments provides substantive evidence that the plan does not meet state requirements for a SYP because it does not adequately evaluate the cumulative impacts of the plan on coho salmon and on other aquatic species. The response simply asserts that assessments of fish and wildlife issues were included, and states that "any issues that do not meet requirements of the Forest Practice Rules...must be addressed in subsequent timber harvesting plans." However, an earlier response (Pg.26, par.2) has indicated that, in the opinion of the regulatory agency, the plan fully meets the requirements of the Forest Practice Rules. The response thus fails to address the issue raised by the comment.

Pg.26, par.5 – The response relies on the response to Pg.26, par.4, which has been shown above to have failed to address the issue raised by the comment.

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Appendix 2

Discussion of points raised in a letter from Dr. Daniel Opalach of PALCO to Mr. Lee Michlin of the California Regional Water Quality Control Board, dated 12 November 1998

1. Concerning the statement that results of the Bear Creek study "cannot be used to compute statistics that can be appropriately applied to the entire watershed" because "stands harvested in the last 15 years are not representative of some theoretical watershed 'average' condition": It should be noted that the practices used recently in the area appear to be characteristic of those used elsewhere in the same terrain; both past and future practices presumably depend "on a number of factors including access, stand condition, economic markets, equipment availability, and environmental considerations." The data thus provide a good indication of the *proportional* increase in landsliding rates for the current mix of standard practices, as calculated *per unit area* of land of the given category. The recent cutting history becomes relevant *only* if rates on recently logged areas are significantly higher than those on lands logged earlier. In other words, the comment that "stands harvested in the last 15 years are not representative..." is meaningful only if the overall conclusion from the calculations of landsliding rates is valid: landslide rates are higher on recently cut slopes.

The SYP/HCP prepared for PL lands indicates that no major changes in silvicultural strategies or yarding methods are expected in the next few years: the current mix of standard practices that depend "on a number of factors including access, stand condition, economic markets, equipment availability, and environmental considerations" is thus likely to continue on most hillslopes. The only impending changes that appear likely to affect landsliding rates are implementation of a mass-wasting strategy and widening of stream-side buffers. Because data were stratified according to landform, it was possible to calculate the potential influence of *complete* implementation of the mass-wasting strategy: calculations were carried out for *complete* protection of all sites subject to examination under the mass-wasting strategy. This, of course, overestimates the level of protection actually likely to arise from implementation of the strategy.

In short, not only did the calculations contained in the 8 October letter address likely future changes in management practices, they addressed the best-case scenario of future changes.

2. Concerning the use of observation rather than "a designed experiment": Most of the body of scientific knowledge has accumulated not through controlled experiments, but through observation of existing conditions. Were experiment required, the fields of epidemiology, astronomy, and paleontology would not exist. Many of the most important questions facing land managers today are at a scale not amenable to experimentation: influences of land-management activities across a landscape, effects triggered by the interaction of major storms with land-management patterns, and long-term ramifications of environmental change. In each case, observations of the outcomes of past practices provide the most useful basis for understanding. In many instances, observations are made using statistical sampling from within a given population. For such applications, an appropriate "sampling design" is necessary if valid inferences are to be made from the results. In contrast, the Bear Creek study inventoried the entire population of landslides occurring from the 1950's to the 1990's, and a statistically-based sampling design is thus not an issue.

At a more general level, the argument seems to be that information from the past is not relevant to the future because things may be different in the future. Logical extension of this argument would suggest that by the time new data are collected, they also will be irrelevant because the future, once again, may be different. But this argument skirts the central issue. Dr. Opalach has indicated that past management decisions have depended "on a number of factors including access, stand condition, economic markets, equipment availability, and environmental considerations." It is hoped that the calculations presented for Bear Creek will allow future management decisions to be based "on a number of factors including access, stand condition, economic markets, equipment availability, environmental considerations, *and an understanding of the likely outcomes of proposed practices*."

3. Dr. Opalach cited a draft study of Jordan Creek watershed that had been prepared by PWA, indicating that results showed a pattern opposite to that evident at Bear Creek in that recent landsliding at Jordan Creek was said to have occurred predominantly on slopes cut more than 30 years ago. Since Dr. Opalach's letter was written, the Jordan Creek report has been released. In actuality, results of the study show a pattern very similar to that at Bear Creek: "In Jordan Creek, 60% of the landslides and 77% of the landslide sediment delivery came from 50% of the watershed which had been harvested within the last 15 years" (PWA 1999, p.27). In other words, rates of landslide sediment delivery from recently logged lands are 3.3 times higher than those from partially recovered forest (calculated as the ratio between (0.77/0.50) and (0.23/0.50)).

4. Concerning the argument that the analysis of landsliding is invalid because "such models [are] overly simplified (i.e., they do not come close to representing the underlying ecological systems)": Landslide models are not ordinarily expected to represent ecological sysems.

5. Concerning the form of equation (5): I very much appreciate Dr. Opalach's careful scrutiny of and interest in the calculations presented in the 8 October letter. I believe that Dr. Opalach's misunderstanding here arose from my failure to emphasize more strongly the fact that the rate variables are defined per unit area, as indicated by the definition provided for the variable *S* as "landslide sediment delivery per square mile." The easiest way to test the suitability of the equation suggested by Dr. Opalach, $S_o = aR + (1\text{-}a)R$, is to evaluate the resulting expression of units:

$$\text{yd}^3\text{-yr}^{-1}\text{-mi}^{-2} \neq \text{mi}^2 \text{ x yd}^3\text{-yr}^{-1}\text{-mi}^{-2} + \text{mi}^2 \text{ x yd}^3\text{-yr}^{-1}\text{-mi}^{-2}$$

$$\text{yd}^3\text{-yr}^{-1}\text{-mi}^{-2} \neq \text{yd}^3\text{-yr}^{-1}$$

The mis-match of units suggests that the definition of the variables had not been clearly understood.

The second cause for confusion, I believe, is the fact that equation (5) is so self-evident that it begs for a more subtle interpretation. None is necessary. I simply needed to partition 90% of the slides (0.9*R*) into the sensitive sites and 10% of them (0.1*R*) into the rest of the area as a lead-in to equation (6). The partitioning had to be on the basis of the number of slides rather than on the rate per unit area of the land-types, since no information about the area represented by each land type was provided in the PWA report.

Let me see if I can explain the derivation of the method more clearly using a concrete example:

Consider a watershed in which landsliding rates are 9.6 times higher on recently logged land than on land logged more than 15 years previously. If that watershed had remained uncut over the past 15 years, let it be characterized by a landslide count of 10 for a hypothetical storm. If that watershed had been completely cut within the past 15 years, it would then be characterized by a landslide count of 96 for the same storm.

Now, consider that 90% of the landslides were identified to occur on inner gorges, streamside slopes, swales, and headwalls. Under conditions with forests older than 15 years, there would thus be one landslide on the hillslopes outside of these areas, while under recently cut conditions, there would be 9.6 landslides outside of these areas.

If all areas of inner gorge, streamside slopes, swales, and headwalls are left forested, and thus assumed to experience the rate present for forested conditions, there will be 9 landslides in these areas under either cut or uncut conditions.

The total number of landslides under uncut conditions would then be 10, and under recently logged conditions, 18.6 (9 in the protected areas and 9.6 outside of those areas).

If the objectives of the Basin Plan were to be attained, management would be designed to reduce landsliding rates to less than 20% over background, so a target number of 12 slides or less would be desired (assuming that landslide sizes are distributed randomly among the site types). To achieve this level, an excess number of 6.6 slides would need to be prevented. Thus, 6.6 / 8.6 (equal to 77%) of the area outside of the protected areas would need to be covered by forests older than 15 years at any time. This means that 23% of the watershed (exclusive of the protected areas) can be cut over a 15-year period, which is equivalent to 1.5% of the unprotected area of the watershed per year.

The equations that describe this calculation were those presented in the report, “Calculation of cutting rate for Bear Creek watershed.” I regret having made this simplistic model seem more complicated than it actually is by introducing it first with equations rather than with words.

6. Concerning the difficulty of establishing background rates for landsliding: This is an important point. The Bear Creek calculations were carried out to compare rates of landsliding on recently logged land with those on lands logged more than 30 years ago. However, “background” rates are ordinarily understood to refer to those present under pre-management conditions. It is generally accepted that rates of erosion on lands cut during the 1950’s and 1960’s are likely to remain higher than those in old-growth forests. Rates of landsliding measured for second-growth forests older than 30 years thus will overestimate the actual background rates, so the calculated 9.6-fold increase in landsliding rates on recently cut land relative to lands cut more than 30 years ago actually represents a *minimum* estimate of the increase over background rates.

7. Concerning the statement that “field identification of shallow landslides is not difficult”: It is very true that it is not difficult to identify a landslide once it has occurred; the problem is identifying those slides *before* they occur. Many sites are stable with vegetation present but are not stable when vegetation is absent; it is the removal of vegetation that makes those sites unstable. There thus will be no evidence of instability at the site before the vegetation is removed.

Dr. Opalach notes that hillslope moisture conditions can provide an indication of instability. Moisture is indeed a very important influence on slope stability, and removal of vegetation through logging, burning, and herbicide application alters the hydrologic regime of hillslopes by increasing the moisture levels. The hydrologic conditions evident before a site is logged thus do not necessarily reflect the conditions that will be present after the site is logged. A competent geologist can do a very good job of recognizing earthflows and deep rotational slides that are continually in an unstable state, and these are the kinds of failures that are marked by the “pistol-butting” and “jack-straw” trees that Dr. Opalach cites. These are also the failures that exhibit hummocky topography, headwall escarpments, open tension and radial cracks, compression ridges and other evidence of past or continuing slope movement.

Such indicators are not useful, however, if the slope has not yet moved because it has not yet been destabilized. The same geologist would not be able to recognize sites that are completely stable before they are logged, but fail suddenly as shallow debris avalanches after logging has occurred. The standard approach for recognizing site types *that will become unstable* after logging is to examine the site types that became unstable after past logging. This information is available only through observation of the outcomes of past storms on managed slopes.

8. Concerning the issue that most inner-gorge slides are within 400 feet of the creek: Unfortunately, it tends to be the largest landslides that extend the farthest from the creek. The headscarp of the largest recent landslide in Bear Creek watershed is located more than 500 feet from the nearest stream, for example, and sediment delivery was essentially 100%. It is possible that confusion surrounding the actual locations of the Bear Creek slides arises from PWA's apparent practice of plotting the slide location according to the approximate center-point of the slide, rather than according to the headscarp location. (It should be noted that this practice is reasonable as long as it is consistently applied and the protocol for plotting is described). True head-scarp locations must be determined from planimetric corrections of the aerial photography, rather than from the locations indicated on the PWA map.

9. Concerning the low number of landslides on stream-side slopes less than 65%: Without knowing the proportion of stream-side slopes greater than 65%, the number of landslides occurring on such slopes does not provide a basis for assessing whether stream-side slopes of less than 65% are more or less landslide-prone than steeper stream-side slopes.

10. Concerning selection of a period containing major storms for evaluation of landslide frequencies in North Fork Elk River watershed: First, it is the major storms that reveal the influence of land-management activities on slope stability; it is pointless to study the distribution of storm-generated landslides during a period without storms. Second, because the analysis is carried out in terms of relative rates between recent and old logging, the storm distribution during the period is not particularly important *as long as the period contains sufficient large storms to generate a large sample-set of landslides*. If a 20-year period of no storms and no landslides is appended to the sampled period, there would be no change in the calculated results.

Reference cited

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Appendix 3

Discussion of Dr. James Patric's review of SYP/HCP comments by Reid

In early January, PALCO staff solicited a review of the SYP/HCP comments by Reid (1998) from James Patric, and on 29 January 1999 that review, entitled "Review of the sustained yield plan/habitat conservation plan for the properties of the Pacific Lumber Company, Scotia Pacific Holding Company, and Salmon Creek Corporation," was submitted to the California Board of Forestry. Eleven points were raised by Dr. Patric, and these are discussed below.

1. Dr. Patric states that "Had the document been directed to lumber companies named in its title rather than a congressman, it might qualify as an attempt to produce useful methods or processes." It is not clear why the objectivity of the document is expected to be contingent upon the entity requesting the document. The review was prepared by a federal scientist at the request of the Environmental Protection Agency and Congressman George Miller, so it was directed to the agencies and individuals requesting the review. It is assumed that the lumber companies involved have access to the review, and it is hoped that those companies will find the information useful in designing future land management practices.

2. Dr. Patric uses the report's statement that "To the extent that today's stands fall short of supplying the quantity and quality of wood contributed by original forest conditions, channel habitat and downstream floodplain conditions will fall short of those upon which the downstream beneficial uses depended" as evidence of an "anti-timber harvest bias." However, this statement simply presents the concept that there is a continuum of impact intensities possible: impact intensities will be low if conditions are similar to those in which the species evolved, while impact intensities will be high if conditions are radically changed. This principle is a well-established precept of Pacific Northwest fisheries biology, and is the basis for the establishment of reference woody debris loadings in the NMFS matrix (NMFS 1996) and PACFISH guidelines (USDA and USDI 1994). See also discussions by Bisson et al. 1997, Frissell and Bayles 1996, Frissell et al. 1997, National Research Council 1996, Reeves et al. 1995, and FEMAT 1993. Dr. Patric's interpretation that "To me, this sentence suggests that nothing short of forest conditions as per pre-European colonization can suffice" is not supported by the original statement.

3. Dr. Patric notes that only a small amount of water is trapped behind woody debris during storms, thereby arguing that the overall influence on flood peaks would not be large. However, the primary influence of concern is the role of roughness imparted by debris: flow is slowed in rough channels. It is a common engineering practice to clear debris from channels to increase flood conveyance (e.g. Linsley et al. 1982, Dunne and Leopold 1978), but it is also a common engineering concern that doing so increases flood hazard downstream (Dunne and Leopold 1978), where flood waters converge with increased efficiency from the cleaned channels upstream.

4. Dr. Patric first indicates that he does not understand the mathematical basis for the woody debris calculations. He then cites a November 5, 1989 article in the Houston Post which contained an error in a mathematical analysis of the eruption of Mount Saint Helens. Dr. Patric then appears to conclude that the calculations presented in the HCP comments are wrong because the calculations in

the Houston Post were wrong. However, the fact that errors have been made by others in the past is not valid grounds for assuming that all other calculations are erroneous.

5. Dr. Patric states that the “deep, porous soils conducive to efficient infiltration of rainfall” characteristic of forested areas “can exacerbate debris avalanching on forested land.” However, data from Bear Creek, Jordan Creek, and North Fork Elk River indicate that this is not the case on those areas of PALCO land that have been evaluated; in each case, debris avalanche rates on forested land are lower than on logged land.

6. Dr. Patric supplies data from Helvey (1967), which indicate that a 60-year white pine stand intercepts 26% of the annual rainfall. He then states that, on the basis of Helvey’s work, the rate of interception for the 60-year-old redwood stands in Freshwater Creek was “likely about half of the speculative 22.5%.” Dr. Patric does not explain this conclusion, but it would seem that the results of Helvey’s work would suggest that the rate expected of Freshwater should be on the order of 26%.

7. Dr. Patric indicates that “foliage interception never drops to zero and lush re-vegetation characteristic of humid climate forest soon begins to restore interception loss rates.” However, interception on a burned or herbicided surface may be very close to zero. Second, recovery is accounted for in the calculations through the assumption of a 15-year period for complete hydrologic recovery. Measurements by Helvey (1967), as quoted by Dr. Patric, now suggest that the assumption of a 15-year period for hydrologic recovery severely underestimates the actual recovery period; white pine forests were no more than 73% recovered 35 years after cutting. Thus the calculations included in the HCP comments may significantly underestimate the impact of recent logging on increased flood frequencies in Freshwater Creek.

8. Dr. Patric first notes that “a surprising amount of intercepted water evaporates during storms.” He later states that losses during the storm constitute only “the tiny amounts evaporated from wet vegetation.” Measurements in a redwood forest at Caspar Creek quantify the amount of intercepted water that evaporates during storms: of the 3.34 inches of rain that fell during a 24-hour storm, 16.8% of the water never hit the ground (J. Lewis, USDA Forest Service, personal communication). Even if initial storage is assumed to account for the *maximum* of 0.15 inch quoted by Dr. Patric, the amount that evaporated during the storm amounted to 3.7 times that initially trapped on the foliage. In coastal California, then, most of the interception loss appears to occur throughout the period of the storm; it is not restricted primarily to the early stages of the storm. Considerable interception loss therefore occurs during the portion of the storm most relevant to generation of flood peaks.

9. Dr. Patric states that “Dr. Reid’s speculations as to sediment production and transport hinge on expected increases in flood magnitude and frequency resulting from decreased interception of rainfall.” However, this statement does not reflect the actual contents of the comments. The 16 November comments indicate, instead, that the description of observed increases in sediment production and transport hinge upon documented field observations in the areas under question.

10. Dr. Patric cites the occurrence of an 1861 flood as an indication that restoration of pre-European settlement forest conditions will not solve hydrologic problems in the redwood forest. No one, however, has argued that floods did not occur under natural conditions. Instead, the 16

November comments 1) raised the issue that large floods are now occurring with increased frequency and are doing more damage to hillslopes and channels than occurred under natural conditions, and 2) presented evidence that the increase in frequency and damage appears to be associated with rapid rates of logging in the affected watersheds.

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Appendix 4

Discussion of memo from PALCO concerning analysis of flooding in Freshwater

On about 2 February 1999, staff of PALCO presented a letter entitled “Response to Dr. Leslie Reid’s Analysis of Flooding And Sediment Aggradation in Freshwater Creek” to the California Board of Forestry. Five points were raised by PALCO staff, and these are discussed below.

a. PALCO staff state that existing evidence shows that the Freshwater channel is not aggrading. First, the reply notes that “PALCO staff have observed areas of exposed bedrock in the main channel, an indicator of stream downcutting, not infilling.” However, the location of the bedrock was not described. If it was in the portion of the channel upstream of the aggradational reach, it is not relevant to the flooding problem. Second, bedrock is exposed in the channel banks at several locations in the aggradational reach. This is characteristic of channels that, in the past, encroached laterally into a bedrock terrace; it implies nothing about the current trend of channel change. Third, at locations where aggradation is occurring by accumulation of fine sediments on banks, even exposures of bedrock in the channel bottom would not constitute evidence to refute the occurrence of aggradation. We thus need to know the locations and circumstance of the bedrock exposures to be able to interpret what they indicate.

The response also notes that CDF concluded “only minor channel aggradation may have occurred in the lower gradient reaches of Freshwater Creek.” CDF also provided the data to demonstrate that this “minor” aggradation amounted to about 30% of the 1975 channel cross-section for 20-year recurrence interval flows. Subsequent analysis, described in the comments by Reid (1998) on the draft SYP/HCP, demonstrated that this decrease in cross-sectional area would correspond to a 2-fold increase in the frequency of a 20-year flood. Thus, the issue here appears to be whether a doubling of the frequency of major floods constitutes a “minor” change. CDF had characterized the change before the effects of the changes on cross-sectional and flood stage had been calculated.

The response noted that the CDF study also found that the channel has degraded at least two feet at the upper-most cross section. The likely reason for this was discussed in the Reid (1998) comments: “This section is located 300 feet downstream of a seasonal dam constructed after the Army Corps survey (W. Stringer and R. Langlois, personal communication). Battens are emplaced between the concrete abutments in early summer of each year to provide a swimming pond at Freshwater Park. At the time of the original survey, similar ponding was effected through annual construction of an earthen embankment. Examination of the existing structure indicates that the new abutments constrict the original high-flow channel cross section, suggesting that hydraulic damming may occur during flood flows, thus raising the water-surface gradient in the reach immediately downstream and promoting local incision.” It should also be noted that this cross section is located upstream of the reach in which aggradation and increased flooding are of concern.

b. The response notes that the cross sections were not relocated precisely and implies that they should therefore not be relied on for calculations of resulting impacts. However, the problem of precise location of cross sections was discussed in the comments by Reid (1998): “The exact locations of the original cross-sections could not be determined, but the 1998 sections were estimated to be within about 100 feet of the originals along reaches of relatively uniform character (P. Cafferata, personal communication), and vertical control was provided by matching surveyed elevations of topographic features that were not likely to have changed, such as stable terrace surfaces (Cafferata and Scanlon 1998). Comparisons between the two sets of cross sections thus will not be exact in detail, but general patterns of change are expected to be reliably represented...”

Following submission of the 16 Nov comments, two floods occurred in Freshwater for which peak-flow stages could be measured at the three cross section locations, thus allowing the 1975 recurrence intervals for those floods to be calculated at each location. Recurrence intervals at the upstream site are calculated using the 1998 cross sections to account for recent incision at this “control” location. Results are shown in Table A-3.1, and demonstrate that moderate floods (6-year and 7-year floods at the cross section located upstream of the aggradational reach) are causing the impact of 35- to 100-year floods along downstream reaches. These calculations are not contingent upon the accuracy of the remeasured cross sections except for the adjustment of the recurrence interval for XS-7; without this adjustment, results would show an even larger disparity between upstream and downstream recurrence intervals. A proportion of this increased flood frequency is likely due to the decreased flood conveyance caused by the measured aggradation at the lower reaches.

Cross section	Location	21 November 1998		6 February 1999	
		stage (ft)	RI (yr)	stage (ft)	RI (yr)
upper	above Graham Gulch	bankfull	2?	<bankfull	<2?
XS-7	upstream of aggradation	69.2	7	68.9	6
XS-5	above Little Freshwater	49.1	100	48.1	35
XS-3	area of primary flood concern	38.5	70	37.1	35

Furthermore, measurements of aggradation on banks immediately upstream of XS-3 showed 5 to 14 inches of deposition during the 21 Nov flood and 2 inches during the 20 February flood, indicating that future flood hazard will be increased above the calculated levels.

c. The response states that I “depended upon a personal communication with a Freshwater resident as to how much aggradation had occurred from 1975-1998. Use of such anecdotal information, from someone with an interest in the outcome of the analysis, is representative of many instances in which Dr. Reid’s methods fail to meet normal standards of scientific rigor.” The response provides no other examples of failure “to meet normal standards of scientific rigor.”

First, I did not rely upon the landowner’s observations. Instead, as described in the 16 November comments, I measured the amount of aggradation above surveyed-in benchmarks: “Three survey benchmarks emplaced at grade in 1992 were disinterred at the 37-foot level on the floodplain; mean depth of burial in the 6-year period was 6.2 cm with a range of 5.5 to 7 cm; a single benchmark at the 39-foot level showed burial of 3 cm. The depth of aggradation is expected to be greater lower on the floodplain and additional aggradation is expected to have occurred prior to 6 years ago, but an average aggradation of 6.2 cm is assumed in the following calculations. The resulting calculations are thus expected to underestimate the actual change in cross-sectional area for flood stages greater than 35 feet at this section.” Thus, the PALCO memo misrepresents factual information by inaccurately reporting the content of the analysis. The accusation of “failure to meet normal standards of scientific rigor” is unfounded.

Second, measurements are not “anecdotal” information.

Third, it is distressing that PALCO would assume that residents of Freshwater cannot be trusted. It is interesting that PALCO staff equate “having an interest in the outcome of the analysis” with a willingness to misrepresent factual information; some might construe their statement as a confession rather than as an accusation.

d. The reply explains that "rainfall associated with a single storm is often a very poor predictor of streamflow" so that estimates of recurrence intervals for rainfall can not be used to determine the recurrence interval for streamflow. However, the analysis under discussion was not based on such an assumption. Instead, the analysis employs the assumption that if flood-producing storm conditions recur twice as often, the flood associated with those conditions will recur twice as often; the *proportional* change is the focus of interest. Thus there is no need to make an assumption that a 10-year storm will produce a 10-year flood, for example. Furthermore, the confounding variables such as soil saturation levels and storm timing are either not affected by logging and so would not affect the results (i.e. storm timing would have the same influence before and after logging), or would themselves increase the influence of logging on storm peaks (i.e. decreased interception loss during small storms will increase the antecedent soil moisture during large storms, further increasing the flood peaks). This effect was discussed in the 16 November comments: "Foliage interception loss, in contrast, is important throughout the year when rainfall occurs, and is also influential in maintaining lower levels of soil moisture by decreasing soil moisture recharge from storms of all sizes."

e. PALCO staff make the unsupported assertion that "More troubling, in many cases Dr. Reid's assumptions and analytical approaches bias her results toward maximizing the impacts attributable to logging." However, it is very evident that in each of the cases discussed above, the assumptions made actually decrease the calculated level of impact due to logging: 1) a minimum depth of aggradation was assumed for the floodplain based on measurements made at a location inundated only by the highest flows and only representing 6 years of aggradation, thus underestimating the actual increase in flood heights to be expected at the cross section, and 2) antecedent soil moisture calculations were not included in calculations of the influence of rainfall interception on flood peaks, thus underestimating the actual increase in flood heights to be expected. In both cases, these assumptions were discussed in the text of the comments.

Further, the statement that "most scientific work in hydrology contradicts Dr. Reid's finding" shows either an unfamiliarity with recent hydrologic literature or a highly selective survey of the literature.

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Appendix 5

Discussion of memo from PWA concerning Bear Creek review by Reid

On 10 November 1998, Dr. William Weaver and Mr. Danny Hagans of PWA sent Dr. Dan Opalach of Pacific Lumber Compnay a report entitled “Response to Dr. Leslie Reid’s review comments of the PWA Bear Creek Sediment Source Investigation and Sediment Reduction Plan.” Numerous points were raised, and are discussed below.

General comments

Review comment 1 – The major issue of the review comment was that there was no explanation given in the report for the selection of 1% to 2% yield from surface erosion. The response reaffirms that “We did not estimate ‘surface and rill erosion rates,’” and explains that the estimate was based on the fact that there are many landslides in the watershed.

Review comment 2 – The response states that the topography and geology of neighboring old-growth areas in Bull Creek would not allow a valid comparison of landslide rates between logged and unlogged lands. However, geologic maps indicate that the headwaters portions of several unlogged Bull Creek tributaries are on the same bedrock as most of Bear Creek, and topographic maps indicate that although average slopes are lower in the Bull Creek tributaries, the range of slopes represented would allow comparison on the basis of landslide rate per unit area of slopes of different gradients.

Review comment 3a – I very much appreciate the correction: road-related sediment inputs indeed accounted for 8% of the total sediment input, rather than 8% of the anthropogenic sediment input. To calculate the percentage of anthropogenic sediment input, it is necessary to calculate the proportion of the landsliding and debris torrent activity that is anthropogenic. The calculation proceeds as follows:

Assumptions:

the 9.6-fold increase in landsliding measured for recent logging also holds for earlier logging
all bank erosion is “natural”
torrent track erosion is also increased by a factor of 9.6 over background rates

Calculation for 1966:

the total debris slide input of 1,027,400 yd^3 represents the combination of that which would have occurred naturally (x yd^3) and that which occurred because of logging (9.6x yd^3):

1,027,400 yd^3 = $x + 9.6x$;   so x = 96,900

Thus, 930,500 yd^3 of the 1966 input were anthropogenic, and 96,900 were “natural”
The same calculation for torrent tracks produces 89,000 yd^3 for anthropogenic, 9,300 yd^3 for “natural.”

Calculation for 1974 and 1994:

All sources but road erosion are assumed “natural” for this period, as no information is provided concerning reactivation of sources.

Calculation for 1997:

The total debris slide input of 277,900 yd^3 represents the combination of that which would have occurred naturally over the entire watershed, plus that which occurred because of logging of 37% of the watershed:

277,900 yd^3 = 1.0 x +0.37*(9.6 x); so x = 61,000

Thus, 216,800 yd^3 of the 1997 input were anthropogenic and 61,000 were "natural."
The same calculation for torrent tracks produces 24,600 yd^3 for anthropogenic, 6,900 yd^3 for "natural."

Results:
Calculations thus result in the following distribution:

Year	Debris slides (yd^3)	Torrent track scour (yd^3)	Road-related erosion (yd^3)	Total (yd^3) anthropogenic
1966	930,000	89,000	65,000	
1974+1994	0	0	25,000	
1997	217,000	25,000	54,000	
Total	1,147,000	114,000	144,000	1,405,000

Dividing the total road-related sediment delivery by the total anthropogenic sediment delivery thus indicates that 10% of the total anthropogenic sediment delivery was contributed by road-related erosion rather than the 8% that I had mistakenly stated in my review of the PWA report.

However, the central issue of the original comment remains: if the road related input amounts to 10% of the anthropogenic input, the fact that 90% of the anthropogenic input does not originate from road-related erosion would suggest that the bulk of the sediment control effort should be directed toward non-road-related sediment sources.

The response states that "landslides which are going to occur on hillslopes in the watershed, whether or not they are harvest-related, are (for the most part) not amenable to control or prevention." However, future logging-related landslide rates are easily controlled through modifications of logging practices, just as future road-related sources are amenable to control through modification of road design.

Review comment 3b – I agree with the description of an approach to identification of slide-prone areas that the response outlines. It is unfortunate that experience with the mass-wasting avoidance strategy, as realized under the pre-permit agreement, has not been found to result in the kind of analysis that is so clearly described by the response.

Assessment of major findings

Review comment 1 – The response first states that PWA were "unable to provide the clear picture of causative links between specific silvicultural practices and landsliding described by Dr. Reid." However, I did not describe a "clear picture of causative links between specific silvicultural practices and landsliding." Instead, I pointed out that PWA had provided the evidence to link silvicultural practices—in whatever combination was applied at Bear Creek—to a 9.6-fold increase in landsliding rate.

Second, the response states that "Slopes in Jordan Creek were nearly equally divided between those logged less than 15 years ago and those that were harvested over 30 years ago. In spite of this, and in direct contrast to landsliding in Bear Creek, 85% of the landslides occurred on the older harvested areas." This response was written by 10 November 1998, approximately 80 days before the

Jordan Creek report was actually released to the public and to agencies for review. Oddly, the report itself is in conflict with PWA's characterization of their own results. According to the report, "In Jordan Creek, 60% of the landslides and 77% of the landslide sediment delivery came from 50% of the watershed which had been harvested within the last 15 years" (PWA 1999, p.27)." In other words, the Jordan Creek data show the same pattern as the Bear Creek data: landslide sediment input is substantially higher on recently logged lands than on lands logged more than 15 years ago.

Review comment 3 – The response here misrepresents the content of the comment. First, a major concern of the comment was that the length of time represented by landsliding visible on the 1947 photos was guessed; no evidence was provided for how it was determined that the landslides observed had occurred during the previous 50 years. This comment was not addressed in the response.

Second, a second major concern raised by the comments was that the analysis of relative landslide rates between pre-1947 conditions and 1997 conditions was based on the observation that "In 1997, the frequency of large and very large landslides triggered by the 1996/97 storm had dropped to levels close to that seen in the 1947 photograph" (PWA 1998, pg.17 par.3). I thus pointed out that comparison of the number of large landslides that were guessed to have been produced over the 50-year period preceding 1947 with the number of large landslides produced by a single storm in 1996/97 was not a valid procedure for comparing rates. I suppose it is gratifying that the response concurs that this is not a valid approach. However, it is puzzling that the response implies that I made the calculation, rather than noting that it was I who pointed out that the report's calculation was invalid.

In trying to understand the basis for the response, I begin to wonder if there was a misunderstanding based on the text. Is it possible that the statement "...the frequency of large and very large landslides triggered by the 1996/97 storm..." is simply a misprint, and the intended comparison was *not* between the result of the single storm and the outcome of the 50-year period? I had assumed that the "levels close to that seen in the 1947 photograph" referred to a comparison between the 10 slides/50 years = 0.20 slides/yr and the 8 slides/31 years = 0.26 slides/yr, as suggested by the reference to "large landslides triggered by the 1996/97 storm. I had assumed that the comparison was not between 0.20 slides/yr and the 11 slides/31 years = 0.36 slides/yr, which would not constitute "close" levels.

The response then presents results of the Jordan Creek report to support the statement that landslide patterns evident in Bear Creek cannot be applied elsewhere. However, examination of the Jordan Creek report indicates that the response misrepresented the actual results from the Jordan Creek study: in this case, too, rates of landslide sediment production were considerably higher on recently logged lands than on lands logged more than 30 years before.

Review comment 6 – The response again misrepresents results of the Jordan Creek report, presenting these results as the only evidence that results from Bear Creek cannot be applied to inform management decisions on areas with similar geology, topography, and management history in nearby watersheds. However, as described above, the actual results of the Jordan Creek report support the general patterns of landslide distribution that had been described in Bear Creek.

Specific comments

(note that most responses to specific comments provide useful discussion and supplementary information; there are only a few that require further discussion, as presented below:)

17/2 "Statistical measures were not used to test for differences in mean landslide dimensions.... Management reports designed for regulatory agencies and for land owner use are not typically written with descriptive statistics." When descriptive statistics—in the form of calculated means—are presented as results, then it is very useful to include the confidence interval for the means to provide an indication of whether a comparison of means actually means anything. Because almost all of the information needed to calculate the 95% confidence interval has already been entered in order to calculate the mean, the subsequent step is an inconsequential addition to the time required for analysis. If regulatory agencies and land owners are *not* supplied with the minimal information needed to determine whether reported conclusions are valid, then regulatory and management decisions will necessarily suffer.

22/4 The calculation that a 2% proportion of total sediment from road surfaces would require 1.5' of lowering of roads provides a very useful insight—I wish I'd thought of doing that calculation. Measured road-surface erosion rates (Reid and Dunne 1984) indicate that rates of lowering on in-use road surfaces can amount to 1" to 2" per year on roads surfaced with 8 inches of competent rock. Thus, lowering over a 50-year period on poorly-rocked roads might be expected to amount to well over 1.5'. On unsurfaced roads, observation of rill depths developed over half a winter season on a seasonally unused road suggests that lowering rates are at least on the order of 0.4 cm/yr; sheet erosion on the inter-rill areas would increase this value further.

It should also be noted that road drainage connectivity in 1997 does not necessarily reflect that which would have been present in 1966, especially in view of the presence of riparian roads along the main channel during the first cycle of logging. Finally, observations during moderate to large storms indicate in the area suggest that widespread overland flow in unchannelled swales is quite capable of delivering highly turbid water from rolling dips and water bars to channels located more than 50 meters away from the road. The actual proportion of the road surface that contributes sediment to streams may be considerably higher than the 14% noted here.

27/2 The response implies that I suggested that treatment of road related erosion be ignored. This is not the case. The point of the comment was that because road-related erosion constituted a minor portion (now calculated to be 10%) of the anthropogenic sediment input, more effort should be placed on controlling the controllable anthropogenic sources that are responsible for the remaining 90%.

The response states that "Most road-related erosion can be identified before the erosion occurs and it can be prevented by pro-active implementation measures. In contrast, once forests are harvested...there is literally nothing that can be done to immediately remediate the conditions and prevent future slope failure." This, however, is not a valid comparison. The point of the comment was that, like road-related erosion, most logging-related erosion can be identified before the erosion occurs and it can be prevented by pro-active implementation measures. Those measures, however, are contingent upon a good understanding of the relation between landslide frequency and silvicultural practices, and those measures involve modifying the silvicultural strategies used on slopes. Reports such as those prepared for Bear Creek and Jordan Creek could have provided some of the information needed to plan appropriate silviculture, had this goal been an identified priority for the reports.

31/6 The response mistakenly construed the comment that "The practice of diverting road surface water onto hillslopes was discontinued 20 years ago on Washington State forest lands on the Olympic Peninsula when it was found that the diverted water caused landslides" as meaning "...Dr. Reid would encourage the use of long uninterrupted ditches to drain the entire roadbed and cutbank directly to stream crossing culverts." Clearly, other solutions than "long uninterrupted ditches" are available, just as it is clear that indiscriminate use of rolling dips and waterbars is not a prudent measure on the steep, marginally stable slopes on which many of the roads in this region are constructed. It is also clear that PWA have enough expertise and experience not to use such measures indiscriminately, but it is also very clear, given observations of landsliding below waterbars and rolling dips in this region, that the practice is being used in places that it should not be.

37/3 The response states that the relevant calculations had not been presented. However, they can be found in the comment concerning 18/4, where the issue had first been discussed: "The implication here is that the rate of landsliding on recently cut slopes is 9.6 times greater than on slopes last cut 30 years before (85%/37%=2.3 compared to 15%/63%=0.24)."

The response alleges that my statement constitutes "a serious scientific mistake to interpret an apparent association between variables (such as recent harvesting and landsliding) as a 'direct' cause and effect relationship without the data and proof to make such a claim." The response overlooks the fact that the report itself produces the data and the means required to infer causality through a simple statistical test. First, the association between landslide distribution and recently logged areas is easily shown by a chi-square test to have a less than 1 in 10,000 chance of being produced randomly. If the distribution of recently logged areas is characteristic of the rest of the watershed in terms of potential to generate landslides, then causation can be inferred: the landslides are where they are because that is where the logging occurred.

That lands on which the recent logging occurred are characteristic of the remainder of the watershed was first evident on the basis of examination of air-photos, topographic maps, and geologic maps, the "qualitative analysis" referred to by PWA in an earlier response. This conclusion was supported by the DMG review of the Bear Creek report: "For assessment purposes, there is no substantial difference between harvested areas in subwatersheds with channel inner gorges and the subwatersheds where harvesting has not occurred within the past 15 years" (Spittler 1998, p.3 par.5), and that "The functional homogeneity of the geology and geomorphology of the Bear Creek watershed allows for a more simplified assessment of the role of timber management on landslides...." (Spittler 1998). The means to statistically test the representativeness of the recently cut lands is provided by the map of landslides present in 1966, when essentially the entire basin had been logged. If the landslide frequency in the 37% of the watershed that would later be cut in the 15 years preceding 1997 is the same as that in the 63% of the watershed that would not be cut again by 1997, then the two areas are functionally the same with respect to the potential for landslide generation. A chi-square test for landslide distribution in 1966 demonstrates that landslide frequencies did not show a significant difference between the two areas at the 95% confidence level. Thus, the difference in later landslide rates can be attributed to the treatment variable, the logging.

Conclusion

As I said before in the original comments, "The report has done a very good job of assessing the contribution of a variety of sediment sources over the period since first-cycle logging began in the 1940s. The information provided about sediment sources is very useful and appears to be well-founded." I regret that staff of PWA interpreted my comments to be "hypercritical" and to be "more interested in questioning our judgment, objectivity, and professionalism than in the basic data and interpretations we presented." This was certainly not the intent on my part, and on rereading the comments I remain puzzled about which parts were considered offensive. It may simply be that there's a difference in cultures here—researchers are accustomed to fielding concise (read: blunt) commentary in the process of preparing journal articles for publication. I apologize if this style was interpreted as a questioning of judgment, objectivity, and professionalism; at least the authors can rest assured that I did not accord them special treatment.

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Appendix 6

Assessment of chronic turbidity levels in several project-area streams

Introduction

Wild salmonid populations have decreased throughout the southern portion of their range during the last century, and several evolutionarily significant units of coho salmon, chinook salmon, and steelhead trout have been listed as threatened or endangered under Federal or state endangered species acts. Considerable expense is now devoted to habitat restoration in an attempt to halt the population decline. These efforts have concentrated primarily on establishing a physical habitat form considered by biologists to be desirable for the species in question.

Casual observations of undisturbed and disturbed tributaries, however, reveal that the most pervasive visible distinction between the two is not differences in channel form, but differences in the turbidity of the water during and after storms. Preliminary results of a turbidity monitoring program are here used to compare conditions in several Humboldt County streams against the levels of turbidity documented to harm salmonids and against levels of turbidity found to occur in streams draining old-growth redwood forest.

Turbidity and salmonids

Lloyd (1987) and Newcombe and MacDonald (1991) summarize research carried out to define levels of suspended sediment and turbidity that adversely affect salmonids. Results of the summarized studies are reported most frequently in terms of suspended sediment concentration. However, both the concentration of suspended sediment and the opacity of the water are important as agents of injury. In the case of gill abrasion, for example, suspended sediment concentration is likely to correlate most strongly with measures of injury because the injury is caused by contact with the particles. In contrast, turbidity is likely to be the most useful predictor for evaluating changes in feeding efficiency because increased opacity of the water decreases the distance over which salmonids can detect prey (Barrett et al. 1992). In most areas turbidity and sediment concentration are related to one another by either a power function or linear relation.

Newcombe and MacDonald (1991) note that the severity of impact from contact with pollutants depends on both the magnitude of the concentration and the duration of the exposure; they thus construct an index of exposure *(EI)* to suspended sediments:

$$EI = \log_e (C_s\, T) \qquad (1)$$

where C_s is the suspended sediment concentration (mg/l) and T is the exposure duration (hours). They then define a scale of impacts (Table 1), and find that the exposure index explains 64% of the variance in observed impact severity:

$$\text{impact severity} = 0.738\, EI + 2.179 \qquad r^2 = 0.638 \qquad (2)$$

Additional variance is undoubtedly associated with differences in water temperature and life stages of the fish.

Direct mortality begins to occur at an impact severity rank of 10, corresponding to a 24-hour exposure to 1670 mg/l, a 2-week exposure to 120 mg/l, or a 1-month exposure to 56 mg/l. Lower levels of exposure do not kill the fish directly but decrease their level of fitness and thus are likely to decrease ocean survival, reduce reproductive success, and increase mortality indirectly through increasing the organisms' susceptibility to infection and disease. Sigler et al. (1984) document reduced growth rates for coho salmon at moderate levels of chronic turbidity, for example, and

Holtby et al. (1990) find that ocean survival rates decrease for coho as the size of the out-migrating smolt decreases. The sub-lethal levels of exposure are the primary focus of the present study.

Field sites and methods

To determine the extent to which logging on the lands in question has increased levels of turbidity, it is necessary to identify the levels of sediment loading encountered on the landscape under undisturbed conditions and under conditions representative of existing land-use distributions. Data to answer this question were provided by the "Watershed Watch" monitoring program. Volunteers took repeated water samples and measured discharge at approximately 45 sites in western Humboldt County during the 1997-98 wet season. Among the sites monitored by Watershed Watch are 7 old-growth redwood tributaries in Humboldt Redwoods State Park, 7 tributaries along nearby Shively Road, and McCready Gulch, a tributary of Freshwater Creek, located approximately 40 km north of the other sites. Both McCready Gulch and the Shively Road tributaries drain property considered by the FEIS/FEIR. Portions of the Shively Road watersheds had been logged previously, but intensity of logging has increased since 1991, with 18% of the combined watersheds' area logged since 1995. The recent logging is predominantly by clearcutting.

One of the Shively Road tributaries had been lightly selectively cut in about 1991 but had not been significantly disturbed since; most of the road length in the watershed was associated with the 1991 logging. A second watershed also was last entered in 1991, but this catchment contains a significant length of road undergoing frequent use.

Grab samples were collected from well-mixed portions of each stream in glass or plastic bottles, shaken well, and placed in a Hach portable turbidimeter (model 2100P) to measure turbidity levels in Nephalometric Turbidity Units (NTU). Turbidities of greater than 1000 NTU were determined by diluting the samples to a level that reduced the turbidity to less than 1000 NTU, and the original value was calculated by assuming a linear relation between dilution and turbidity. Relationships between turbidity and suspended sediment concentration were defined during the 1998-99 wet season by filtering and weighing the sediment in a series of grab samples from each of the locations after turbidity had been measured.

Ambient turbidity levels

The data from the old-growth tributaries in Humboldt Redwoods State Park occupy a consistent field in a plot of turbidity against discharge (Figure A6-1), and they share the field with the forested tributary along Shively Road. The Wildcat bedrock present at the forested Shively Road site is expected to be slightly more erodible than Franciscan sedimentary rocks and Yager Formation bedrock typical of the Humboldt Redwoods tributaries, but the colinearity of the relations for the undisturbed tributaries and the least disturbed tributary along Shively Road suggests that the

Table A6-1. Ranking of effects of suspended sediment on fish (from Newcombe and MacDonald 1991)

Rank	Description of effect
14	>80 to 100% mortality
13	>60 to 80% mortality
12	>40 to 60% mortality, severe habitat degradation
11	>20 to 40% mortality
10	0 to 20% mortality
9	Reduction in growth rates
8	Physiological stress and histological changes
7	Moderate habitat degradation
6	Poor condition of organism
5	Impaired homing
4	Reduction in feeding rates
3	Avoidance response, abandonment of cover
2	Alarm reaction, avoidance reaction
1	Increased coughing rate

difference is not significant for undisturbed to lightly disturbed sites. On this basis, data from Humboldt Redwoods State Park are assumed to provide an estimate of background rates for the Shively Road tributaries. Data from the other six Shively Road tributaries plot half an order of magnitude higher and fall into two groups (Figure A6-2). Three statistically indistinguishable sites define a high-turbidity group, and three others define one of moderate turbidity. Data from the forested tributary define a statistically distinct curve at a lower turbidity level. Membership in the groups appears to be independent of the kind of bedrock present in the watershed.

The turbidity rating curves for the moderate turbidity group from Shively Road and that for Humboldt Redwoods State Park (Figure 1 of main report) can now be used to calculate the distribution of turbidity levels for a sample watershed throughout a year. A prototype watershed of 8.9 km^2 was selected for modeling on the basis of its utilization by coho salmon; watersheds of this size provide important habitat both for spawning and rearing of this species (T. Roelofs, Humboldt State University, personal communication). Because the watershed to be modeled is substantially larger than those monitored along Shively Road, data from Shively Road were compared to those from 4.9-km^2 McCready Gulch to ensure that the form of the rating curve does not change with larger watersheds. Results from McCready Gulch are not statistically different at the 95% confidence level from those of the moderate-turbidity group from Shively Road.

Annual hydrographs were measured by the U.S. Geological Survey in 15.7-km^2 upper Jacoby Creek watershed in the late 1950’s and early 1960’s. The ordinates of hydrographs for 1961-1964 were multiplied by 8.9/15.7 to provide an estimate of hydrographs expected from the 8.9-km^2 prototype watershed, and the turbidity rating curves were then applied to the calculated discharges to estimate daily average turbidity for the 3-year period.

Results suggest that under the moderate-turbidity regime measured at Shively Road, a level of 100 NTU would be surpassed for 2 to 3 months of each of the years modeled. Under old-growth conditions, this level would have been surpassed for 2 days during the 3-year period (Figures A6-3 to A6-4). A turbidity of 100 NTU corresponds to a suspended sediment concentration of about 50 mg/L. At this concentration, an exposure duration of 2 months would lead to an impact severity rank of 8, associated with “physiological stress and histological changes,” while an exposure duration of 2 days would lead to an impact severity of 4, associated with “reduction of feeding rates.”

Associations with land use

To assess the likely landscape-scale patterns of chronic turbidity, it is necessary to identify the land-use practices with which high turbidities are associated. The distribution of logging and roading intensities in the Shively Road tributaries allows a crude test of whether high sediment loadings are associated with logging or with roads.

Each of the turbidity rating curves shown in Figure 1 (main report) was used to estimate a sediment yield per unit area for the respective watershed during the 1963-64 water year. These values then allowed the seven Shively Road tributaries to be ranked in order of their sediment yield. Because of the strong dependence of the magnitude of the sediment yield on the slope of the turbidity rating curves, rankings were considered to be a more robust basis for calculations than were magnitudes of sediment input. Each watershed was also ranked according to the percent of the watershed logged and the total road density.

Comparison of the rankings indicates that unit sediment yield generally increases with both percent logged and road density, even though the rankings of these two variables are not well-correlated with one another (Figure A6-6). Sediment yield ranking is best correlated with the mean of the rankings for logging and roads, suggesting that both roads and logging are influential in controlling the unit sediment yield. The three high-turbidity watersheds have the highest level of disturbance by roads and logging, while the three that define the moderate-turbidity group have undergone an intermediate level of disturbance by roads and logging.

Conclusions

The combination of monitoring and modeling results described above suggests that (1) chronic turbidity exposures capable of causing tissue damage are achieved in the 9 km^2 prototype watershed under conditions representing a moderate level of management; (2) These exposures are not achieved in a 9 km^2 watershed under undisturbed conditions; and (3) The highest turbidities are associated with both high road densities and high proportions of watersheds recently logged.

Further, the level of turbidity demonstrated by the monitoring results indicates that sediment conditions are already far above those specified in Basin Plan objectives: turbidity levels are not simply 20% above background levels, but exceed those levels by more than 400%.

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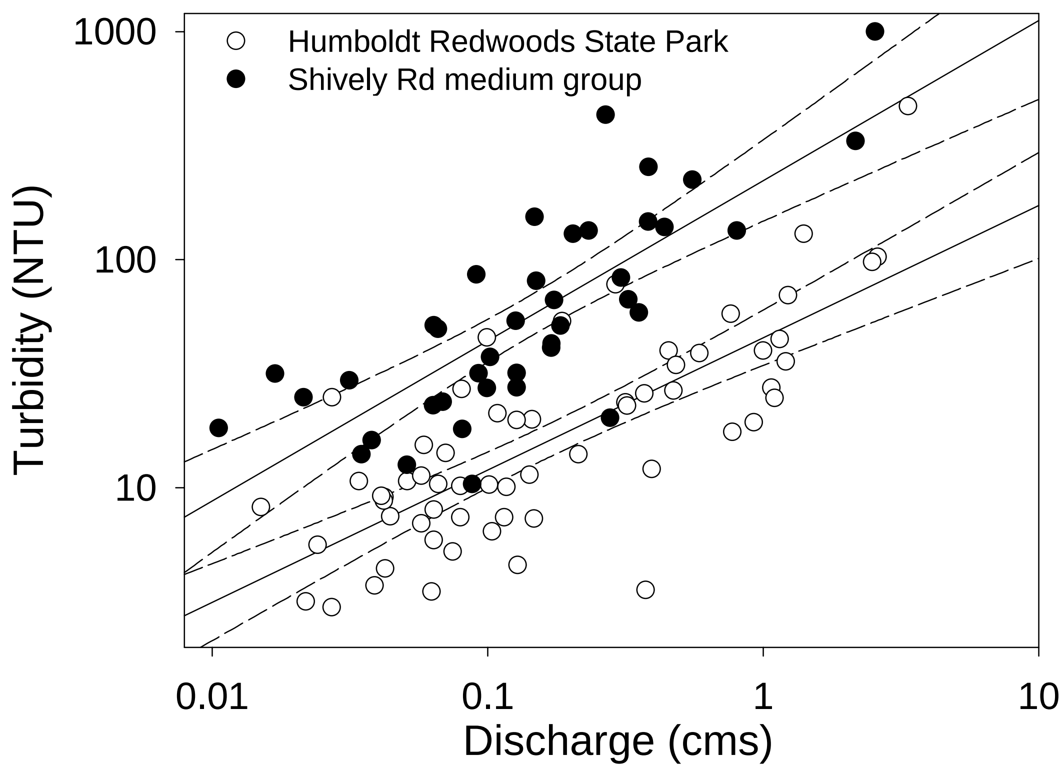

Figure 1. Comparison of turbidity rating curves for logged (Shively Road) and unlogged (Humboldt Redwoods State Park) conditions

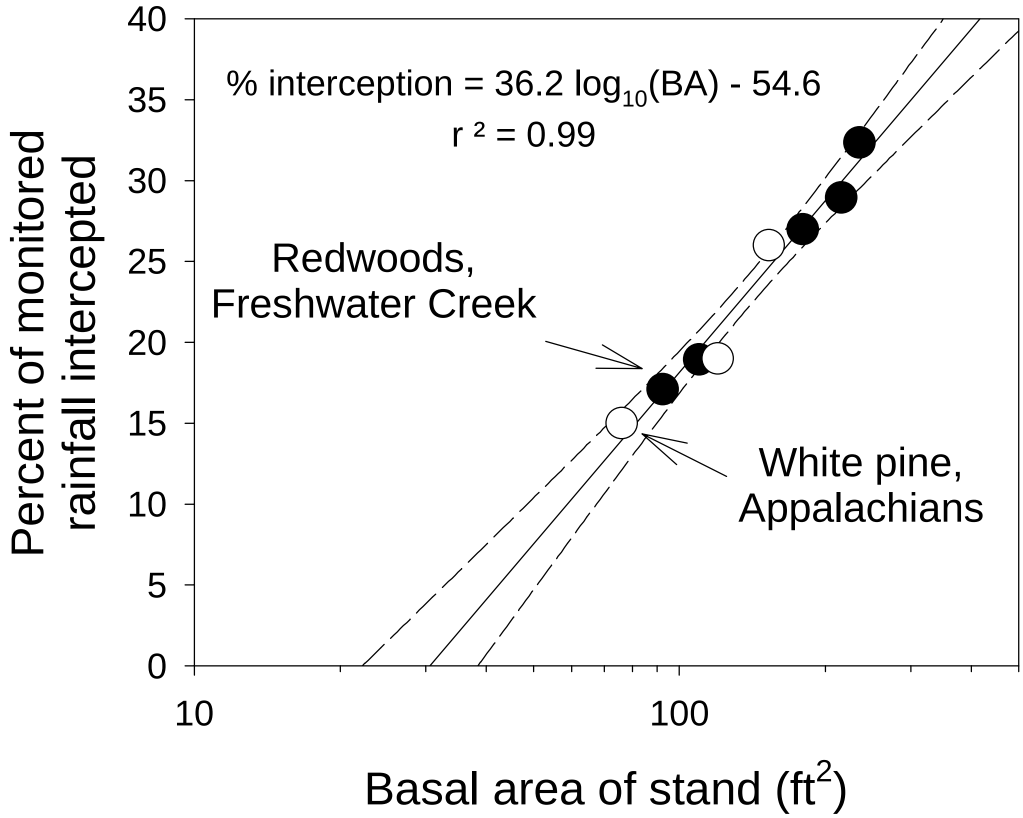

Figure 2. Relationship between stand basal area and percent interception

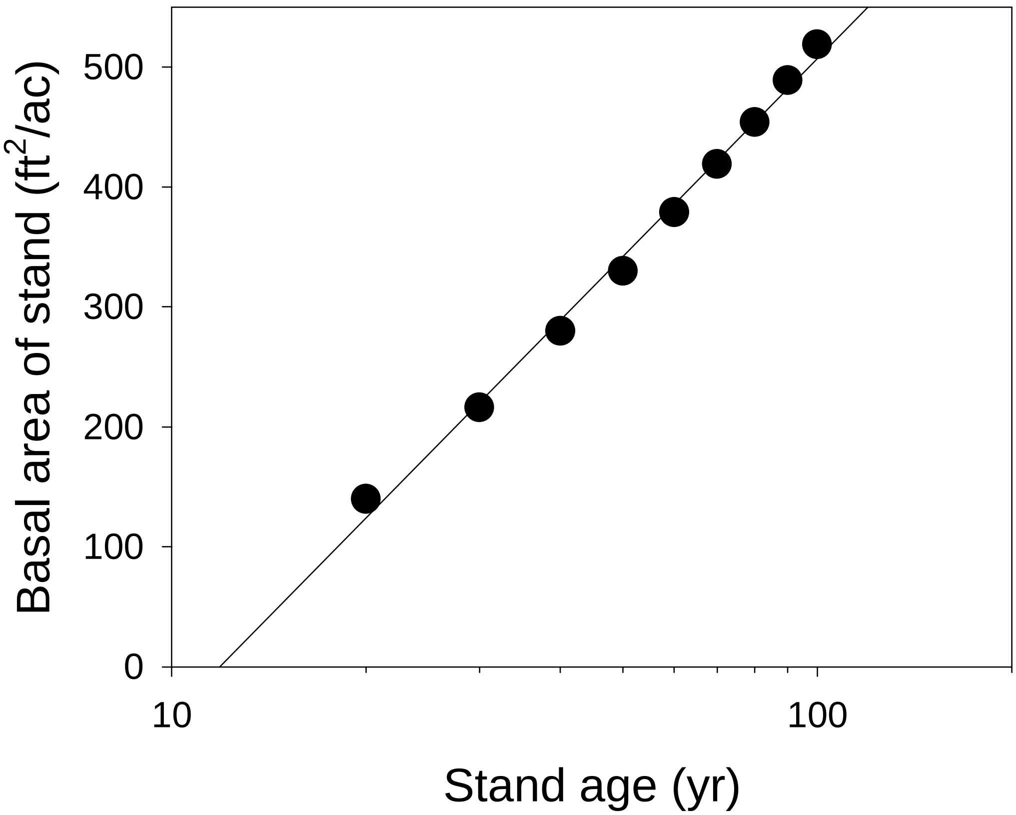

Figure 3. Basal area as a function of stand age for redwoods on site index 160 lands

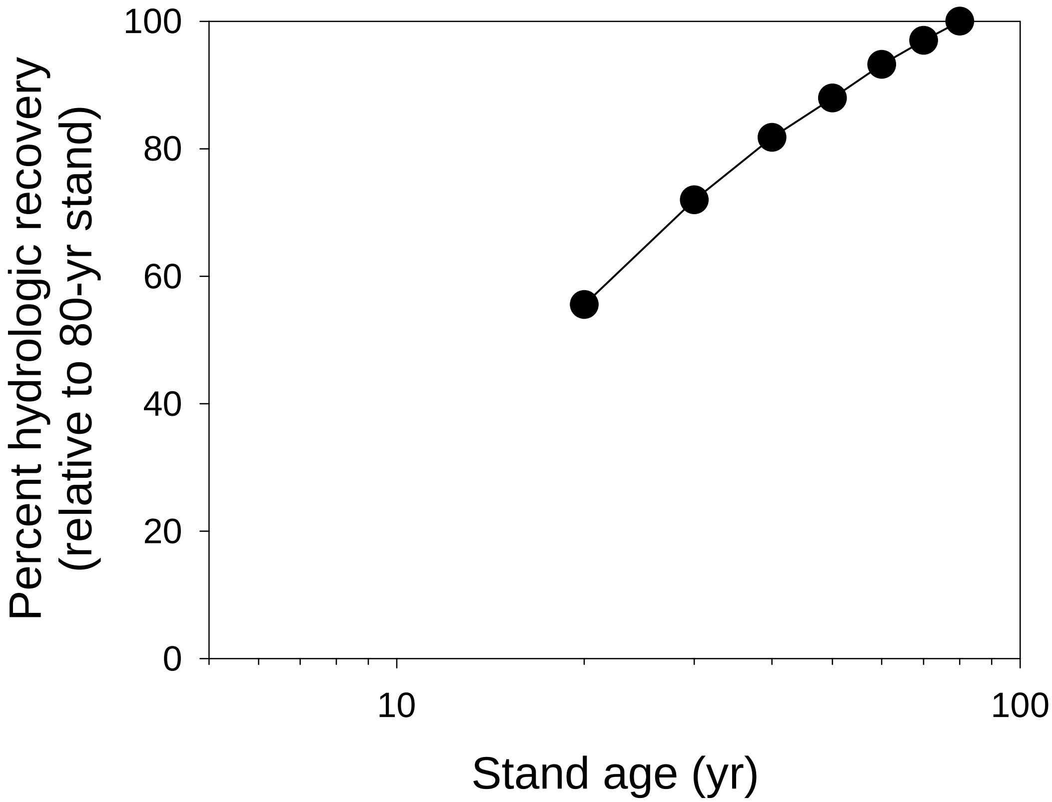

Figure 4. Estimated hydrologic recovery as a function of stand age

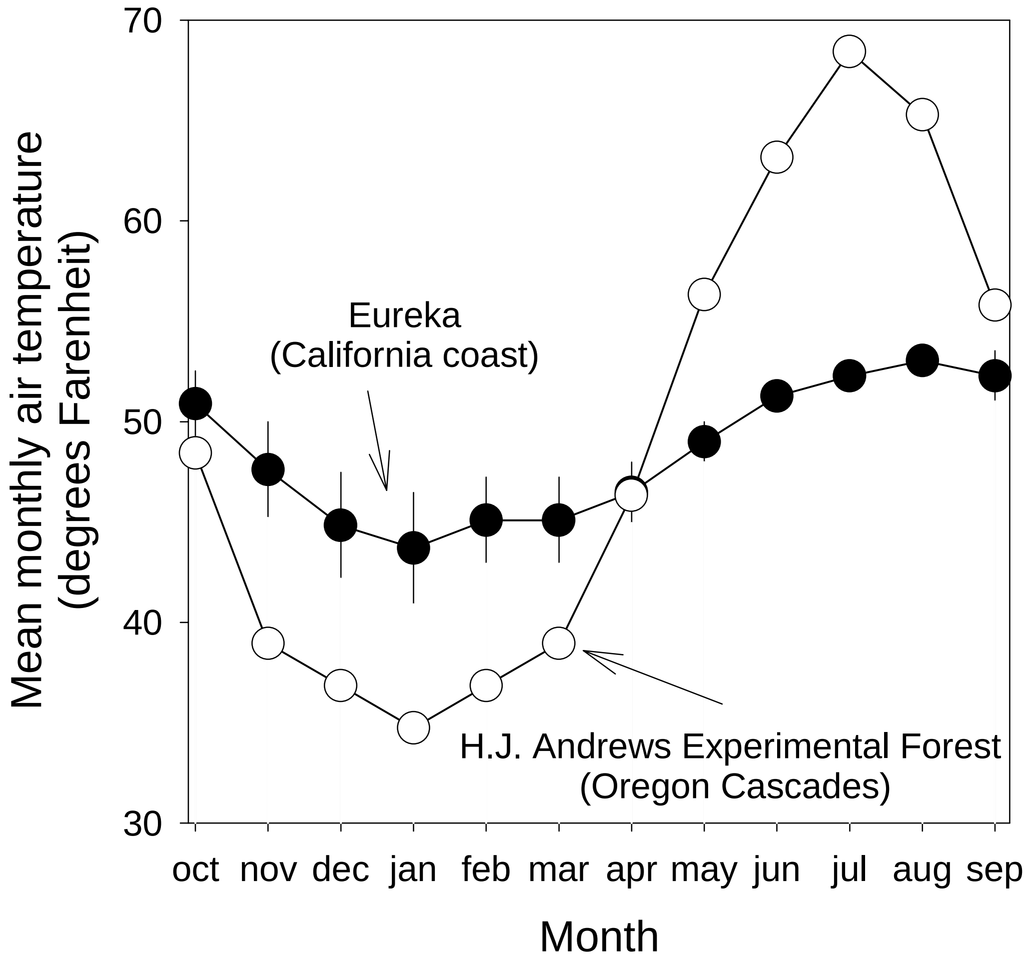

Figure A1-1. Average monthly temperatures for Eureka and for H.J. Andrews Experimental Forest

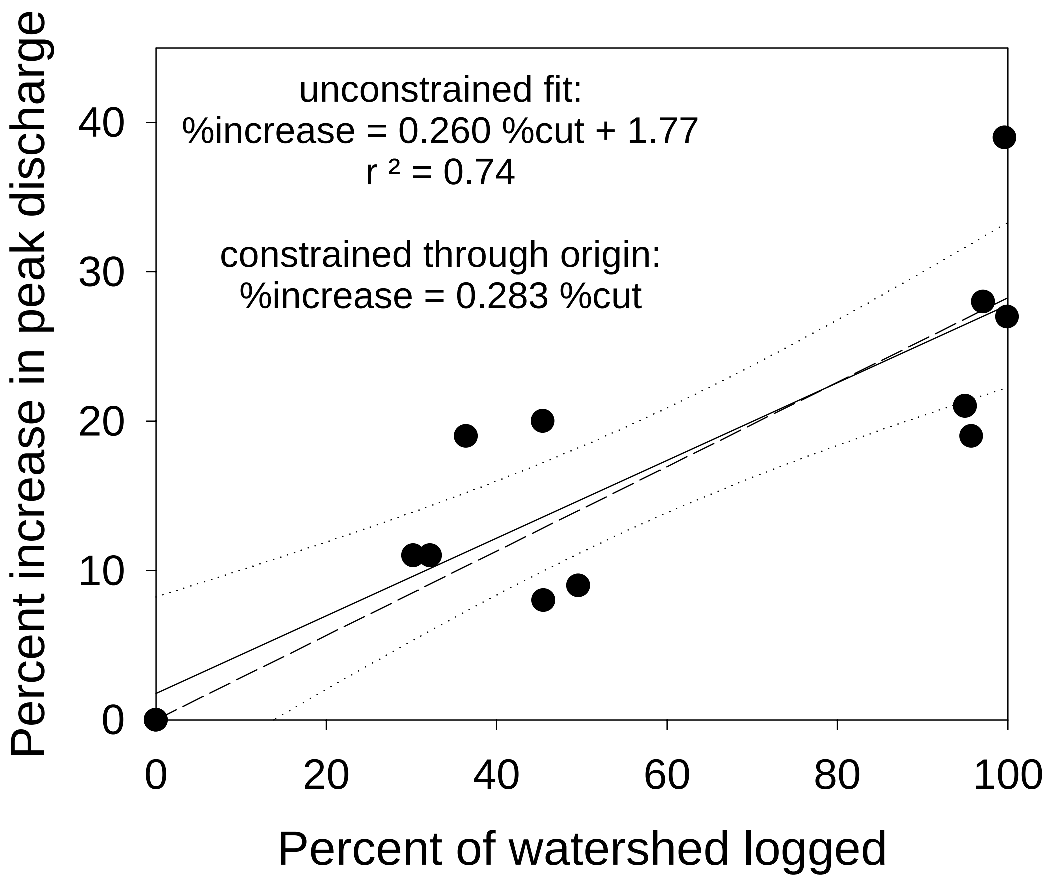

Figure A2-2. Relationship between peak-flow change and percent of watershed logged at Caspar Creek (Figure 6 in Reid (1998a))

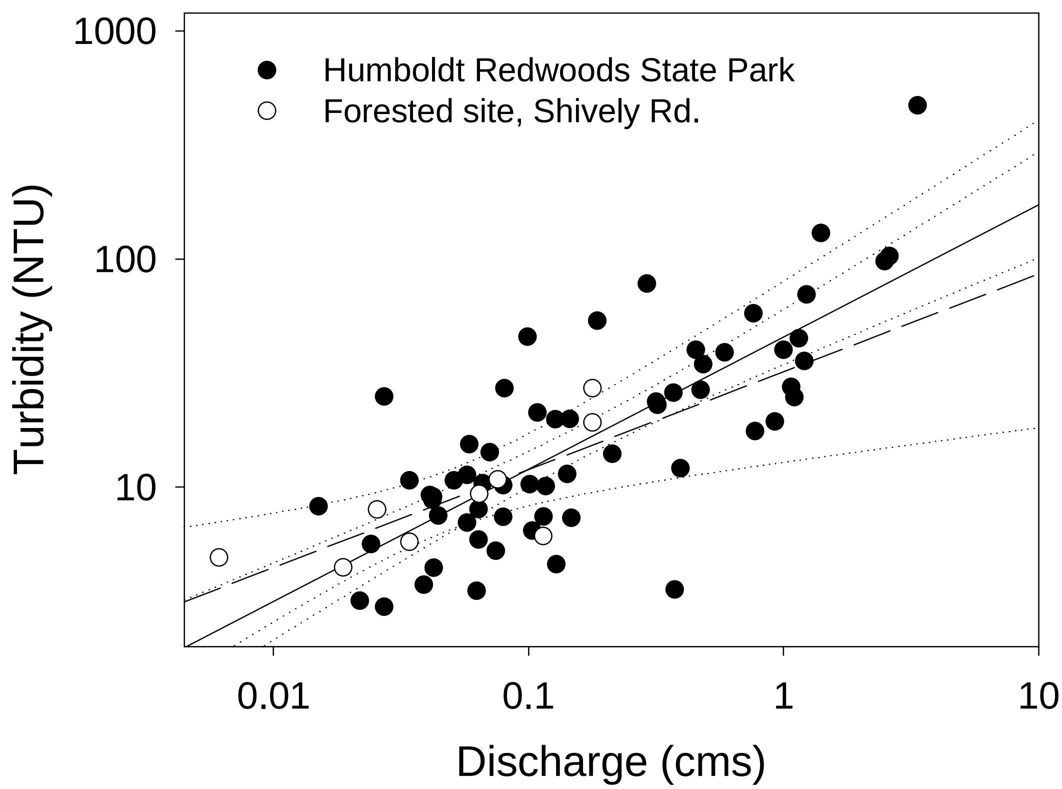

Figure A6-1. Turbidity rating curve for old-growth redwood forest (Humboldt Redwoods State Park) and for a not-recently-logged tributary along Shively Road

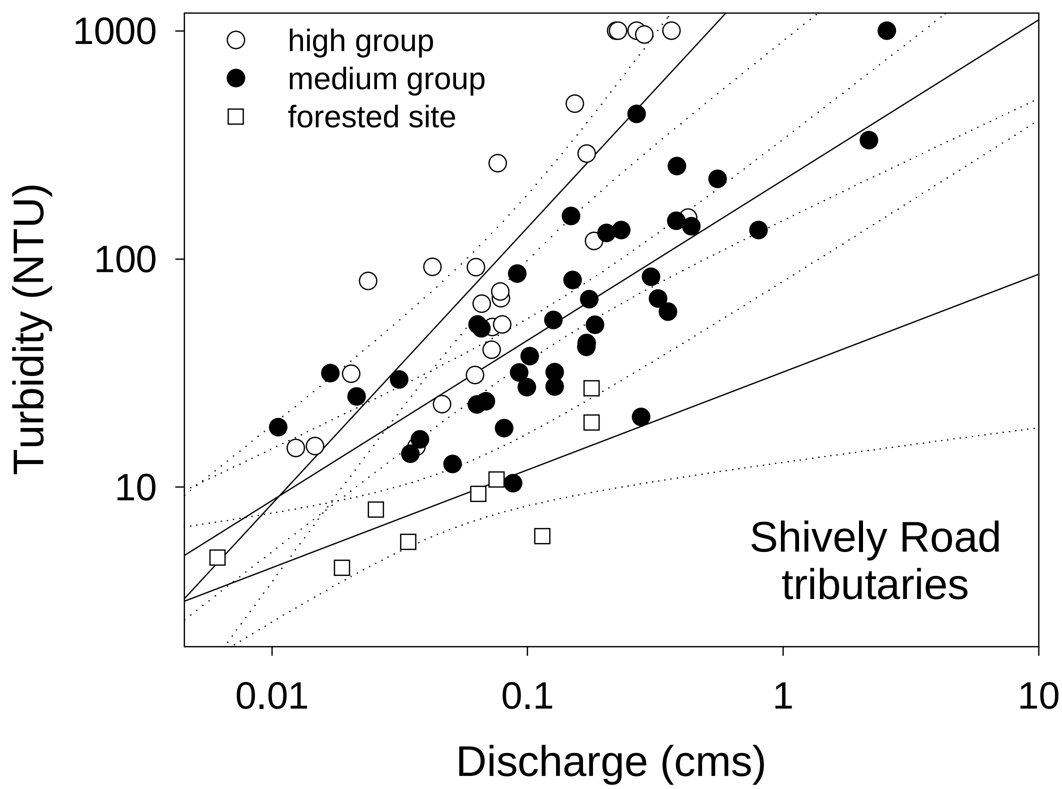

Figure A6-2. Turbidity rating curves for 6 tributaries along Shively Road which have undergone recent logging and one which has not ("forested site")

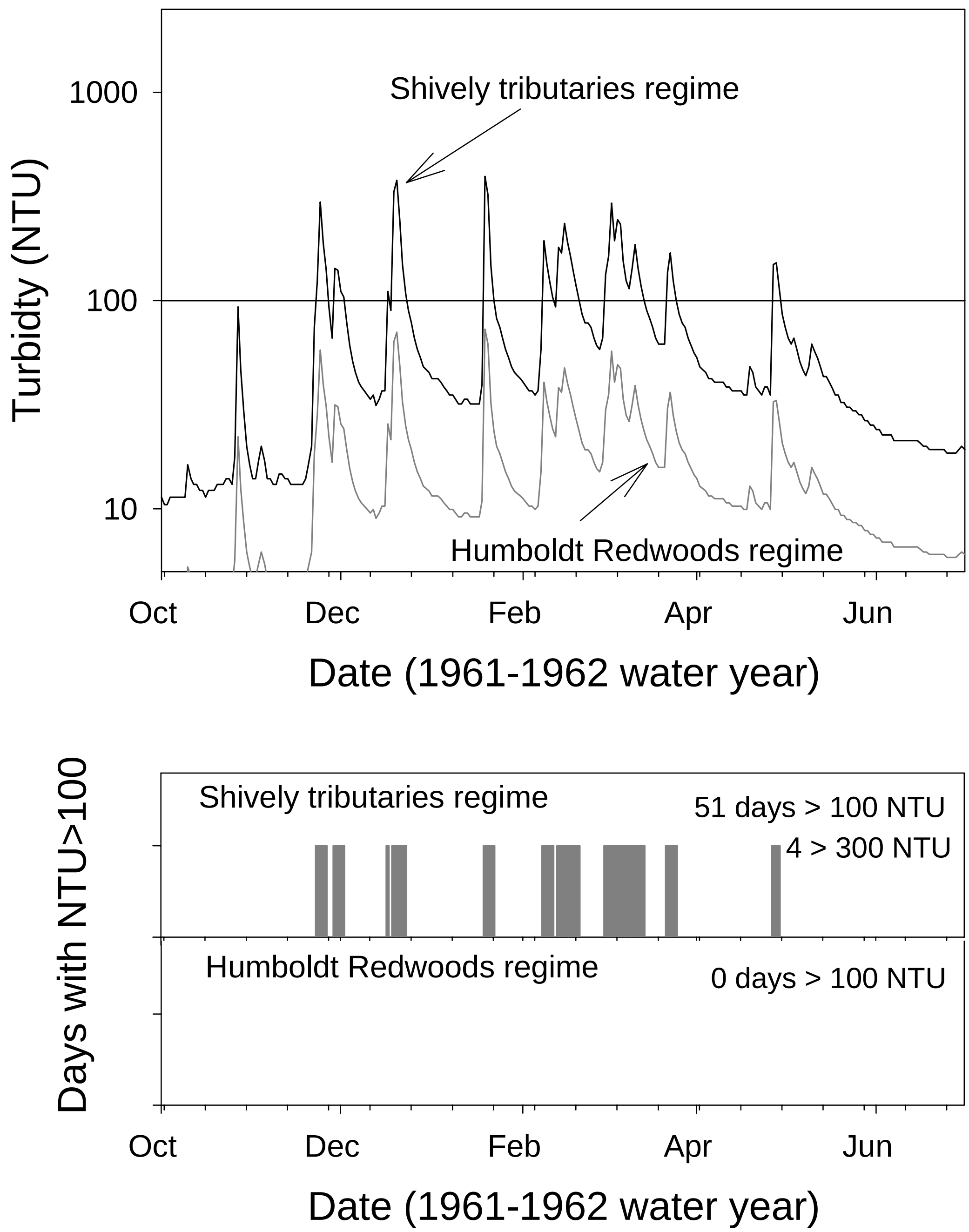

Figure A6-3. Calculated annual turbidity graph: 1961-62

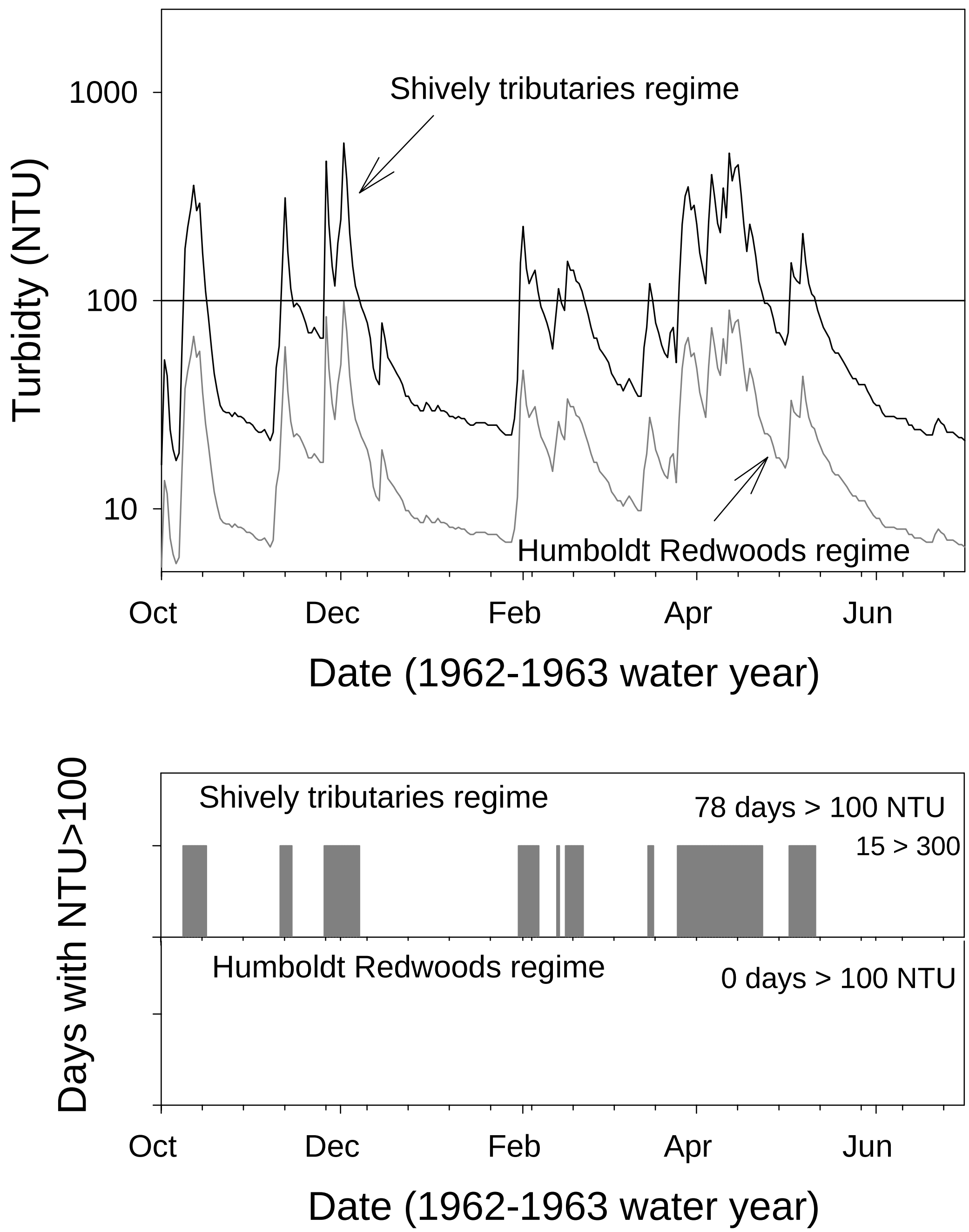

Figure A6-4. Calculated annual turbidity graph: 1962-63

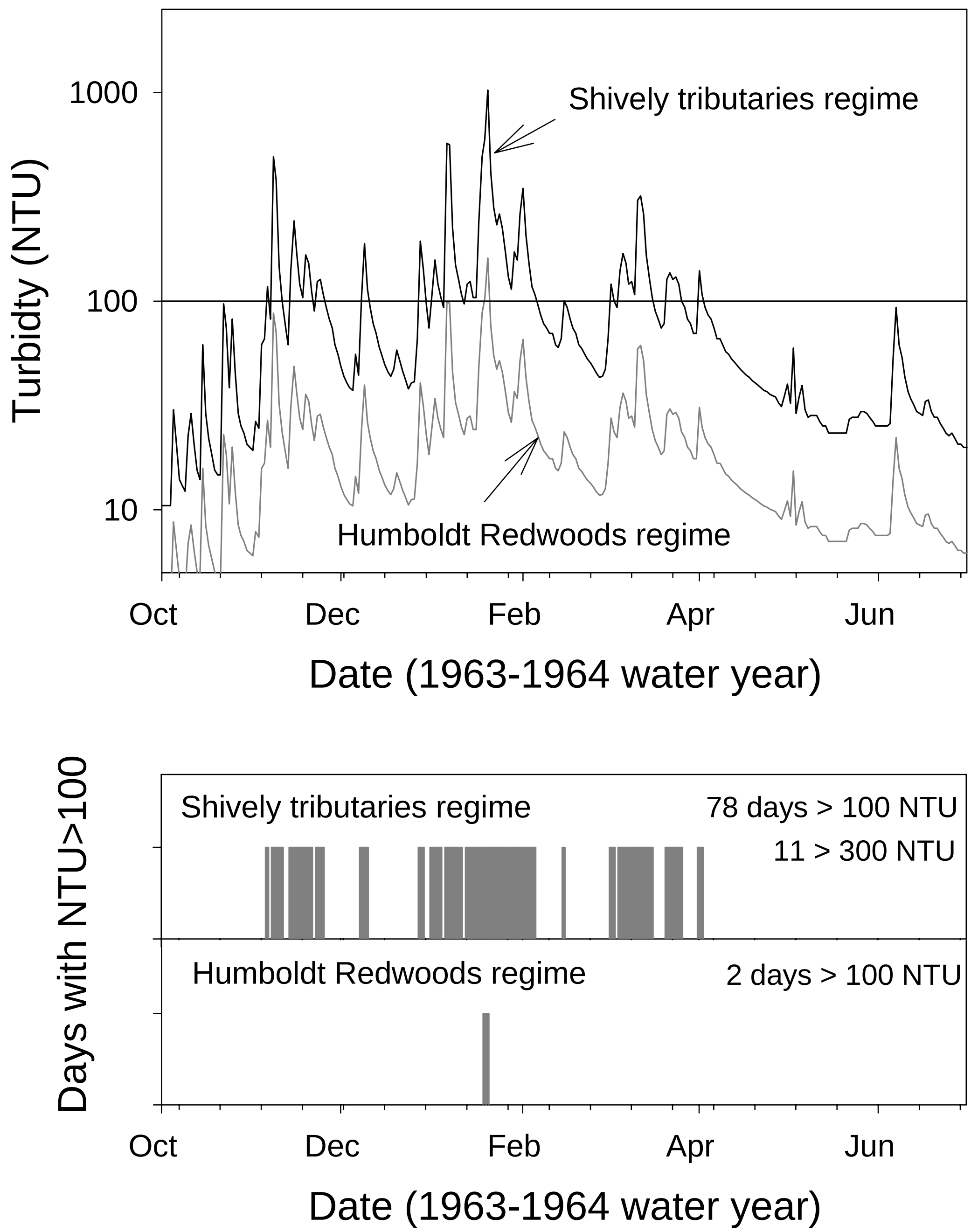

Figure A6-5. Calculated annual turbidity graph: 1963-64

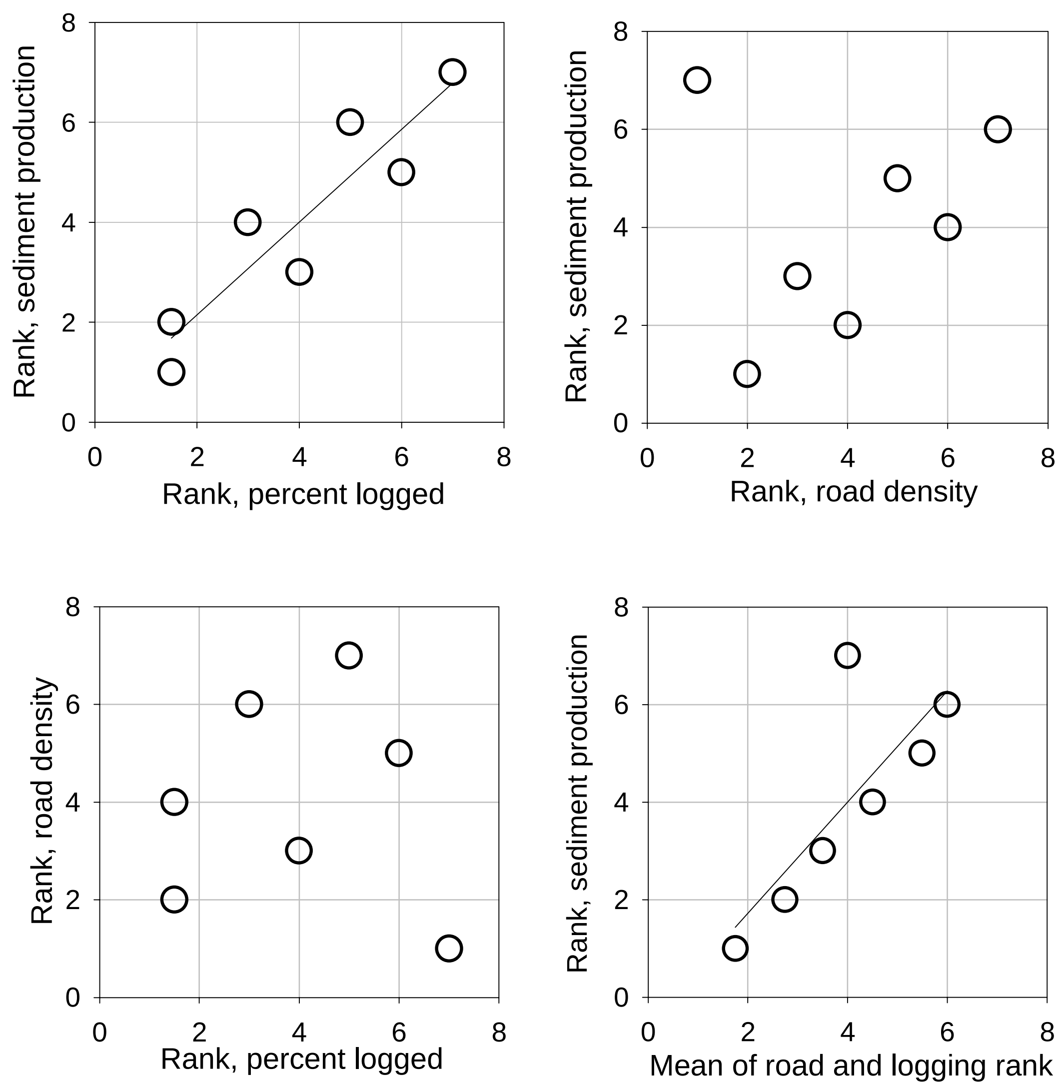

Figure A6-6. Comparison of rankings for sediment production, road density, and percent logged for Shively Road tributaries