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Initial Comments on the Peer Review Draft

1. INTRODUCTION

In 2013 I was contracted by Humboldt Redwood Company (HRC) to provide a scientific review of some of their monitoring activities, specifically with respect to best management practices, and to review their watershed analysis of the Elk River Basin. I also was contracted by HRC and Green Diamond Resource Company to provide a scientific review of the Peer Review Draft: Staff Report to Support the Technical Sediment Total Maximum Daily Load for the Upper Elk River (NCRWQCB, 2013). The purpose of this memo is to provide specific, science-based comments on the Peer Review Draft. Hence this memo will focus on the points where—in my professional opinion—the underlying science is either not clear, or does not seem to support some of the statements made in the Peer Review Draft. In the interest of time and brevity I have not spelled out all of the detailed arguments or provided many citations to support the following statements, but prior to or after our initial discussions I may be able to provide more references on selected topics if this would be helpful.

I want to explicitly acknowledge that the Peer Review Draft represents a tremendous compilation of information collected over many decades, and I recognize and appreciate all of the effort that has gone into developing this document. I also recognize that no study or document is “perfect”, and that any effort to synthesize a large amount of information and use this for guiding future management is an iterative process given: 1) the inherent complexity and variability of sediment production, delivery, and storage in time and space; 2) the difficulty in measuring and separating the current, legacy, and natural causes of the observed high turbidities and sediment loads; and 3) the fact that we can never have a full knowledge of the underlying processes and exactly how these will be affected by both future management and natural events, such as large rainstorms. Given the scale and complexity of the Peer Review Draft plus all the underlying reports and related data, my comments should be taken as a work in progress. More specifically, time constraints limited the discussion of some issues, and other issues still need further investigation before being identified as an issue of concern. My plan is to submit any additional or updated comments at least a week before the meeting scheduled for 4 February, and at this time I also will suggest a list of the most important topics and fundamental as the initial focus of our discussions.

Finally, on a more personal basis, please note that my professional life has been devoted to trying to understand the effects of land use and other disturbances, such as fires, on runoff and erosion, and then use this information to guide management. The overall goal is that with better information our society can make better management decisions, and try to further reduce the adverse effects of human activities on ecosystem sustainability and water resources at both the site and watershed scale. This means that the following comments should be taken not as a critique, but as a means to further refine and improve the proposed TMDL. In this way future regulations and restoration efforts can be of maximum efficiency in terms of reducing the observed problems while still allowing for activities that yield important resources and economic benefits (e.g., timber harvest, fishing, and the use of water for domestic and agricultural uses). Since the exact balance between these various uses and activities is ultimately a political decision, my hope is that this document and our discussions can help lead to a better understanding of the key issues and a broader consensus on how to move forward from here. I trust that these comments will be taken in this spirit, not only for the Elk River watershed, but by extension to other coastal watersheds under the jurisdiction of the North Coast Regional Water Quality Control Board (NCRWQCB).

Please note also that some of the comments are grouped as they generally relate to a single overall issue, and other comments are more independent. At this point the comments are not in order of priority, or in the same order as the chapters in the Peer Review Draft, and this is partly why a suggested order of discussion topics will be provided before our meeting.

<u>1. Setting of TMDL loading at 120% of natural.</u>

1.1. Since each TMDL is unique to its watershed, it is not clear how the list of loading capacities for other North Coast sediment TMDLs (Table 5.5.1, which should probably be corrected to Table 5.1) is relevant. Should the Upper Elk River be more similar to the Garcia River, where the TMDL was set at 341%, or to the South Fork of the Trinity River at 108%? A simple average of all of these TMDLs relative to natural loadings has no physical meaning.

1.2. The calculated sediment yields for the reference watershed (Little South Fork) are extremely questionable due to the very poor data. This uncertainty is greatest for the higher flows that transport most of the sediment, as these flows have the greatest uncertainty in the stage-discharge relationship as well as the relationship between field-measured turbidity and suspended sediment concentrations. The uncertainty in the assumed natural sediment yield has important implications for the allowable increase in sediment yields due to management.

1.3. Related to the previous point, it is not clear how the Staff calculated the sediment yields for the LSF presented in Table 5.2. The annual equations listed in Table 1 of Appendix 5A (relating turbidity to suspended sediment concentrations) are very different from the equations developed by HRC using the valid field data, and the calculated annual sediment yields are much lower than the values calculated by HRC.

1.4. The short record from LSF also means that the mean sediment yield is probably underestimated, as both sediment production and sediment yields are typically highly skewed over time and tend to have a log-normal rather than normal distribution. Hence most of our measurements are made in the low-to-moderate years rather than the biggest storms and biggest rainfall years that generate much of the long-term average sediment production and sediment yields, and the mean value from a short-term record will—on average—be less than the mean value from a long-term record (e.g., Kirchner et al., 2001). Landslides are a prime example of a very sporadic process that will result in highly skewed sediment production and sediment yield values, particularly in smaller and unmanaged watersheds where they are both less frequent and proportionally more important to the overall sediment yield.

1.5. The draft TMDL assumes that the ratio of sediment yields (or implicitly sediment production) between managed and unmanaged basins is constant over time. Conceptually, this assumption may not be valid, as the ratio is highly dependent on the absolute value of the denominator (the reference watershed). If the absolute value of sediment yields in the reference watershed is very small, the ratio will be very high in the low years as the managed basins will still be producing a relatively large amount of sediment, while the ratio may be lower in the bigger years as the proportional increase in sediment yields is likely to be greater in the reference basin. But if the absolute sediment yields in the reference basin are higher, the ratio of managed to unmanaged sediment yields may be smaller in the dry years and higher in the wet years because the proportional increase in sediment yields might be greater in the managed basins due to their greater susceptibility to increased erosion. The data from LSF and other watersheds with better, longer-term records should be examined to determine the variation in the ratio of managed to unmanaged watersheds in dry and wet years, respectively.

1.6. The draft TMDL does not explicitly consider the magnitude of sediment storage either under current conditions or from legacy sources. Storage is mentioned primarily in the context of the mainstem channels, but sediment delivery almost always sharply declines with increasing watershed area (Walling, 1983), and there is considerable field evidence for storage in the lower-order channels.

Similarly, the draft TMDL does not consider the amount of bedload in the sediment source analysis and the potential for both bedload and suspended load to be stored rather than delivered to the downstream gaging stations. The ratio between sediment sources (i.e., sediment production) and downstream delivery (sediment yield) cannot be implicitly assumed to be 1.0. A release of sediment from legacy sources (e.g., channel incision into stored sediment) may be part of the natural recovery process and the appearance of a higher sediment delivery ratio (yield divided by current production) and higher yields at the downstream stations. Conversely, the storage of sediment from current sources will result in a lower sediment delivery ratio.

In the absence of any explicit consideration of sediment storage, the empirical sediment budget approach described on page 4-5 is not a sediment budget, but actually a method of characterizing sediment inputs. The implicit assumption of the loading estimates developed for the Upper Elk River is that the inputs are directly translated downstream to affect the designated beneficial uses. Yet the 2004-2011 loading summarized in Table 5.5 has management-related instream stored sediment as 5.6 times larger than the management-related upslope sediment loading. This storage does not even include any of the sediment stored in lower-order channels; those who have worked in the lower-order headwater channels have noted the large amounts of sediment being stored throughout the channel network *(e.g., D. Manthorne, W. Weaver, pers. comm., 2013)*. The potential changes in this storage need to be considered in relation to the magnitude of the hillslope sources, and the extent to which a given reduction in the different hillslope sources will or will not affect downstream conditions.

1.7. The allowable increase in sediment yields was calculated by first developing annual nonlinear relationships between turbidity and suspended sediment yields and using these to calculate annual sediment yields (Table 1 in Appendix 5A). Twenty percent was then added to the turbidities in accordance with the Basin Plan (equation 2 in Appendix 5A), and the suspended sediment yields were recalculated (Table 2 in Appendix 5A). Again it is not clear how the staff obtained the turbidity and suspended sediment data used to develop equation 2, and there is no indication of the fit of the observed data to the equations provided in Table 1, the resulting uncertainty of these equations in terms of the standard error of the parameters, or the resulting estimates of suspended sediment yields. In the absence of this information one cannot evaluate the validity of the estimated annual sediment loads being calculated, or the ratio between the two sets of values as summarized in Table 6A.3. As an aside, no units are provided for the suspended sediment loads in Table 2 in Appendix 5A.

1.8. Figures 3.26 to 3.30 plot the frequency of the SEV (severity of ill effects) values greater than 4. The reference station (534) is plotted for HY2003-5 and HY2007. The validity of the data from the reference watershed is questionable, as there was no effort to calculate suspended sediment loads in HY2003, and the data for the other years are very limited and noisy, particularly for HY2005. Data for station 534 are not shown for HY2006, and this is consistent with the lack of data, but suspended sediment yields were calculated for HY2006 (see Table 5.2).

For accuracy, these and other plots should note that at many of the monitoring stations turbidity is only measured from roughly mid-October to mid-May, so the time percentages for the SEV values are only for this period. Assuming minimal turbidity values due to the lack of rainstorms over the remainder of the year, the SEV values should either be multiplied by 7/12 to get annualpercentages, or explicitly acknowleged as wet season values.

1.9. The draft TMDL states on p. 5-8 that "The existing estimate of natural sediment loading is based on a long-term average for the period of 1955 to 2011, as is necessary for establishing the total maximum daily load." If this is the case, why are none of the turbidity and sediment yield data from 2008 to 2011 for the reference and other watersheds included in the analyses?

<u>2. Validity of drainage densities</u>

The difference in drainage density between managed and unmanaged watersheds is an important control on several sediment sources, including soil creep, bank erosion, streamside landslides, and headwater channel incision. The drainage density for unmanaged areas was derived from the nearly pristine Little South Fork of the Elk River (LSF). The Peer Review Draft references Appendix 4C as the source for the drainage density in LSF, but the data and analysis underlying this Appendix apparently come from Chapter 2 in Buffleben (2009).

For unmanaged areas the drainage area for a channel head is defined solely by area, yet the data for the four channel heads in the ULSF presented in Figure 2.6 in Buffleben (2009) show a very clear dependence on slope. If all the data in Figure 2.6 are considered then there is no relationship between area and slope, but it is **not** valid to lump all the data as the data from the managed watersheds are from statistically significant different populations. Given both the theoretical and empirical relationship between slope and area for channel head initiation (e.g., Montgomery and Dietrich, 1988), the channel heads and associated drainage density for unmanaged conditions should be plotted as a function of slope **and area** using standard GIS tools and the high quality DEM for the entire watershed. The resulting drainage density should then be compared to the current drainage density to determine the actual change in drainage density. Only then can soil creep, bank erosion, and streamside landslides be more accurately calculated for managed and unmanaged conditions. (Note also that the current use of the median drainage area to define the drainage density is not appropriate, as the data are skewed and the mean drainage area has to be used to obtain a spatially accurate average drainage density.)

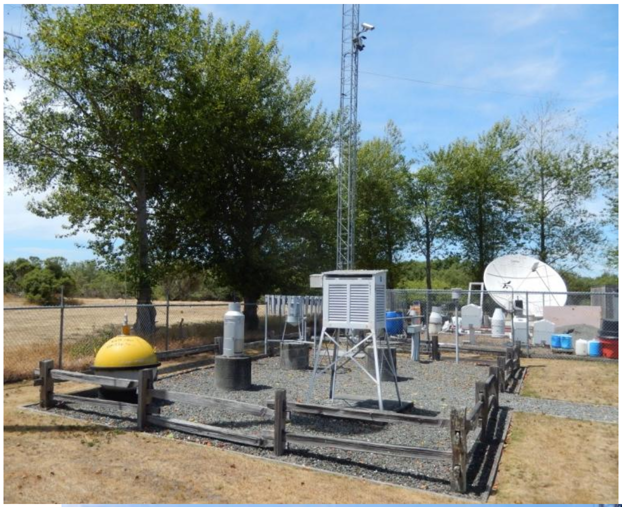
There are other issues associated with exactly how the median drainage area was determined for channel heads in unmanaged areas (4.22 ha) and the two managed watersheds, and these can be discussed in more detail as desired with the NCRWQCB staff.

3. Validity of targets

3.1. On p. xvii the Peer Review Draft states that "The sediment loading associated with instream deposits within the low gradient reaches of Upper Elk River consumes the load allocation. Staff propose that the entire volume of instream stored sediment must be remediated over a 10 year period to achieve the TMDL." Without a better estimate of the volume of instream stored sediment and the stored sediment that might be accessible to the stream under high flows, it is not clear how this recommendation can be justified, or how the 10 year figure was developed.

3.2. The detailed pilot hydrodynamic modeling indicated that a 75% reduction in the measured 2003 suspended sediment concentrations would result in localized channel scour and lead to "some form of channel recovery" (p. 5-8). The next paragraph in the Peer Review Draft explains why staff cannot be confident that a 75% reduction in sediment loading will result in attainment of water quality objectives, but in my view: 1) the uncertainty in this 75% value does not appear to be greater than many of the other data presented in this report; and 2) the model could readily be run with different assumptions of rainfall and streamflows as well as sediment loadings, and these runs could help determine the robustness of the results and the extent to which they may be more generally applicable. The key point is that this sophisticated hydrodynamic modeling indicates that a smaller reduction in sediment loading could lead to many of the desired changes in downstream sediment deposition and nuisance flooding, but these results do not appear to be used for setting the TMDL.

3.3. Some of the data on particle-size distributions (e.g., Elk River in Figure 3.19) and pool depths (Fig. 3.25) do suggest an improving trend in some cross-sections or reaches. This would be expected if the sediment sources are declining relative to the transport capacity, and would suggest that changes in management are starting to have some effect on channel conditions. It would be very helpful to more fully examine the relative frequency and significance of the trends (or lack thereof) at each location, why certain cross-sections or reaches are showing a trend relative to other locations, and how these data are or are not correlated with rainfall and streamflows. It also should be noted that use of the recent rainfall data from the official Eureka rain gage is questionable, as the recent growth of trees adjacent to the official rain gage may well be causing an increasing (but unknown) bias in the data (Figure 1).



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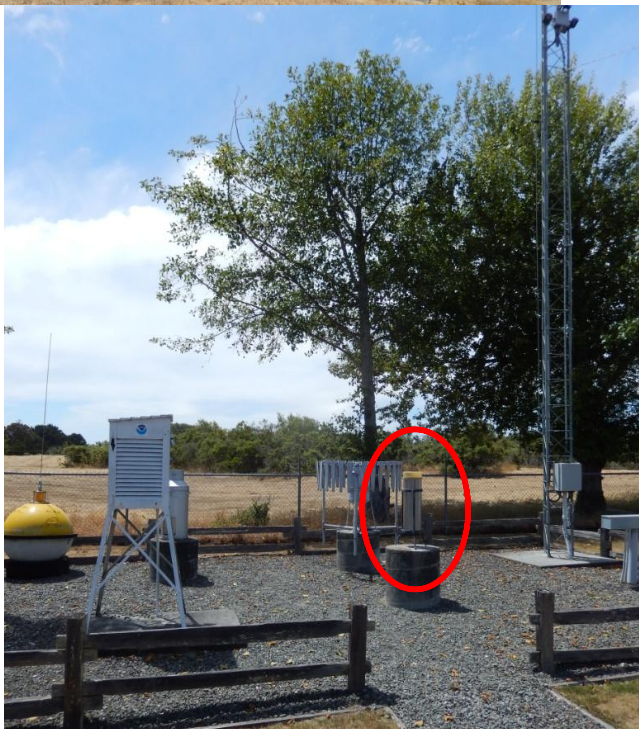

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Figure 1. Pictures of the weather station outside of the NOAA office in Eureka taken in summer 2013. The official Eureka rain gage is located just behind the rain gage with the Alter shield, so it is to the left of the wooden temperature shelter in a), and to the right in b). This shows how the adjacent trees are almost certainly having an increasing effect on the official rainfall measurements.

3.4. The natural rate of soil creep (p. 4-7) used Lehre's (1987) value, and this references Buffleben's Ph.D. dissertation (2009). The use of Lehre's value is questionable, as it was derived in a much drier, predominantly grassland area in Marin County, and it appears that the maximum depth of soil creep was just 0.4 m. Creep data from Redwood Creek are available and have been used in other North Coast TMDLs. These indicate a much greater depth for soil creep, but the determination of a "best" value using the Redwood Creek data is difficult because of sampling and other issues (Buffleben, 2009). Nevertheless, these data are still much more applicable than Lehre (1997) due to the much greater similarity in processes, vegetation, soil depths, and climate.

Both personal and indirect communication with staff from the California Geologic Survey (CGS) indicates that they also believe that the background creep rate used in the Peer Review Draft is not applicable and probably too low. They note that aerial photos are not a particularly sensitive way to determine whether a given deep-seated landslide is moving, and that there are almost certainly more than two active deep-seated landslides in the Upper Elk River watershed, especially given the continuing tectonic uplift. Since it only takes a few millimeters of movement a year from a moderate number of deep-seated landslides to deliver much larger volume of sediment than identified in the Draft Peer Review, both the suggested creep rate and the value for deep-seated landslides should be discussed with professional geologists from CGS and adjusted accordingly.

3.5. The total allocation of 1.4 yd^3 mi^{-2} yr^{-1} in Table 5.5 to the sum of post-treatment sediment discharge sites, road surface erosion, and harvest surface erosion is unrealistically low. First, it is not realistic to limit post-treatment sediment discharge sites to the minuscule value of 0.25%. There has been a consistent improvement in the treatment of problem sites and a corresponding reduction in post-treatment sediment production, but extensive earthwork on an existing crossing will almost inevitably deliver more than 1/400th of the crossing fill volume; treatment data from the Six Rivers National Forest and HRC indicate that one can expect an average of about 3-5% of the fill volume will be released during treatment. Hence this target needs to be adjusted to allow for a small (e.g., <5%) transient post-treatment sediment pulse in exchange for the long-term reduction in sediment production and risk. A better approach for these targets is to set up a strategic system for post-treatment monitoring, and to immediately address any problems, or potential problems, that are observed at a given sediment discharge site, road, or harvest unit.

Second, it is not realistic to disconnect all road segments from watercourses. Road densities are typically around 6 mi mi^{-2}, but can reach 10 mi mi^{-2} in some watersheds (Figure 4.17). If the drainage density (according to the Peer Review Draft) is about 12-17 mi mi^{-2} depending on geology (Table 4.1), there will be numerous stream crossings and a certain length of road has to drain directly into the stream. The length of the road segment draining into a crossing should be minimized and sediment production reduced by rocking, but some sediment inevitably will be produced and delivered from an active road network. Legacy roads immediately adjacent to the stream (e.g., those within 50 or 100 feet) also will contribute some sediment, and these should be removed to the extent possible. The allowable sediment yield from roads should be calculated from a survey of road crossings and the use of a calibrated road erosion model, such as SEDMODL2 or WEPP:Road, and used to help generate a more realistic target for road surface erosion.

The third component, harvest surface erosion, can and should be reduced to near zero by requiring at least 65-70% surface cover throughout the harvest unit, especially on skid trails and cable rows. In some particularly sensitive areas consideration should be given to ripping the skid trails to increase infiltration, or strategically placing compacted slash. This should minimize, if not largely eliminate, surface runoff and erosion. Such treatments, when combined with the use of buffer strips as required by California's Forest Practice Rules, should then capture the surface runoff and sediment being generated from harvest units.

3.6. The change in drainage density with management over time is questionable. First, Table 4.1 (drainage density by decade) in the Peer Review Draft shows no difference in natural drainage density with geology. However, this table does show that the effect of management on drainage density is identical for Wildcat and Yager lithologies, while the management-induced increase in drainage density is shown to be 29% less for Franciscan lithology. If lithology affects drainage density in managed areas, shouldn't lithology also affect drainage density for unmanaged conditions?

A second major concern with drainage density is with the extension of channel heads over time and the underlying cause(s) of any increase in drainage density. Table 4.1 in the Peer Review Draft shows that 75% of the increase in channel density occurred by 1950-59, and a consistent 5% increase in drainage density for each subsequent decade, and in 2000-2009 reaching 100% or the current drainage density of 16.5 mi mi^{-2} for Wildcat, Yager and Hookton geology, and 11.7 mi mi^{-2} for Franciscan geology. The assumed 5% rate of headward channel extension per decade is not clearly justified, especially given the changes in timber harvest practices over time and how these might affect the different processes that control channel initiation (infiltration-excess overland flow, seepage erosion, buried channels and macropore flow). Unpublished results from the nearly completed Beck's BMPEP monitoring project have shown no headward channel extension as a result of recent management activities (D. Manthorne, HRC, pers. comm., 2013). Buffleben (2009, p. 38) states "Most channel heads in the managed watersheds are associated with some type of management feature, the most common of which are skid trails." A more detailed, process-based analysis is needed to explicitly determine what proportion of channel heads are due to changes in runoff processes and runoff concentration (e.g., interception of subsurface flow by roads and skid trails, or the generation of surface runoff by infiltration-excess overland flow) as opposed to the potential change in peak flows due to timber harvest that tends to be emphasized in the Peer Review Draft. The relative importance of these different processes will then affect whether the existing channel network is likely to decrease over time as road and skid trail initiation points are eliminated and the extent to which current timber harvest and the change in canopy density is likely to increase or decrease drainage density. More importantly, this understanding is needed to guide and improve future forest management activities.

3.7. The draft TMDL indicates that there should be a decreasing length of unstable channels in order to reduce bank erosion and streamside landslides. The problem is that it is not clear how one can define an "unstable channel", as conceptually stream channels are inherently unstable and are rarely in complete equilibrium (P. Wilcock, Borland Lecture in Hydraulics, 2013) due to the effects of large storm events or droughts, disturbances such as the input and movement of large woody debris, soil creep leading to bank erosion, the shifting of meander bends over time, uplift rates, and changes in base level. Nevertheless, extremely unstable channels generally can be recognized through the field judgment of experienced fluvial geomorphologists and repeated monitoring because of their higher than expected percentage of unstable banks and unusual magnitudes of channel incision or aggradation in response to **moderate** flows. With more investigation this instability generally can be attributed to a large shift in the amounts of--or balance between--flows and sediment, a change in the type or amount of bank and floodplain vegetation, a reduction in the amount of large woody debris, or other causes. As stream instability decreases it becomes increasingly difficult to separate natural vs. management-induced instability. It should also be recognized that different stream types have inherently different levels of "instability", and it would make more sense to quantify bank erosion and streamside landslides by near-stream slope and geomorphic stream type than by the standard Forest Practice Rule classification of Class I, II and III. (For stream type I would suggest using the more process-based Montgomery-Buffington [1997] classification rather Rosgen's [1994] classification system.)

I would therefore suggest that the numeric target should be to minimize upslope sediment sources, minimize bank disturbance, minimize the inputs of surface runoff and sediment from roads and skid trails, and set targets for large woody debris. If upslope erosion rates from management activities are largely controlled, surface runoff is not concentrated and delivered into swales or channels, and the channels are not directly altered, there is no clear process to drive a significant increase in the amount of instability in lower-order channels. And if lower-order channels are not subjected to significant management-induced increases in sediment or peak flows compared to the effects of varying rainfall, then downstream channels also should not be subjected to an increase in instability. Note that one cannot necessarily expect a high degree of channel stability given the legacy effects of prior management activities, and the associated effects of these legacy practices on drainage density, wood loading, channel incision, and downstream sediment deposition.

Longer-term monitoring should be done on selected stream reaches of different orders (or preferably stream types) in the Little South Fork to quantify the temporal variations in bank erosion, streamside landslides, and channel cross-sections over time. Similar measurements should be made in representative reaches in managed watersheds. These would only be done at relatively wide intervals, such as every three years, and after exceptionally large storm events. Some of this is being done as part of the aquatic monitoring under HRC's Habitat Conservation Plan and the Peer Review Draft presents some cross-sectional change data in Figures 3.9-3.13 and 3.15-3.18, but these are only for downstream areas. More monitoring in lower-order channels is needed to characterize the changes in both managed and unmanaged areas.

<u>4. Limit harvest-related peak flow increases in Class II and Class III watercourse catchment areas to 10% in 10 years.</u>

Much of the justification for this limit appears to come from the desired to limit the predicted increase in peak flows to a value that will result in no more than a 20% increase in suspended sediment yields. Appendix 6B presents the logic that was used to determine what increase in peak flows is allowed, and there are several basic issues with the approach developed here. First, the basic equation used to relate timber harvest to peak flows was developed from Caspar Creek, and there already is an extensive report critiquing the use of this model (Dhakal and Sullivan, 2006; 47 pp. plus appendices).

Second, the basic equation used to predict how much change in peak flow is needed to cause a 20% increase in sediment yields is questionable (the pooled regression equation in Figure 1 in Appendix 6B). This equation is based on 51 storms from HY 2004-7 in ULSF (station 534) (note the incorrect caption, which states only HY 2004-5), with 7 storms in HY2004, 10 storms in HY 2005, 20 storms in HY2006, 8 storms in HY 2007, and 6 storms in HY 2008 (Table 1 in Appendix 6B). Each data point appears to represent the total load for a given storm along with the associated peak flow. Given the extremely poor quality and relationships of the field-measured turbidity and suspended sediment records, particularly for HY2005, HY2006, and HY2008 (R^2 of <0.04 for HY2008), it is again not clear how the data in Table 1 were calculated, especially since the data for these three years account for over 70% of the total dataset. For the pooled data the regression slope of the logged data is 1.248 and the standard error is 0.155, so the 68% confidence interval is 1.09-1.40, and the 95% confidence interval is 0.94 to 1.56. If one takes into account: 1) the underlying uncertainty in the data, 2) the uncertain validity of the underlying peak flow model, and 3) the uncertainty in the relationship between peak flows and suspended sediment, then it is highly questionable whether this approach can be used for setting an allowable increase in peak flows.

The slope of 1.25 for ULSF is only about half of the slope of the relationship from Caspar Creek (2.52). Assuming the units are the same, this means that sediment yields in Caspar Creek are much more sensitive to changes in peak flows than the sediment yields in ULSF. For example, a change in peak flows from -0.25 log units or 0.56 cms to 0.5 log units or 3.16 cms increases sediment yields from 900 to 7800 kg, or just less than eight times. If the slope is set to 1.88, then the same flows change the calculated sediment yields to range from 630 to over 16,000 kg, or more than 25 times. The use of an average value is only appropriate if all of the underlying sources, transport processes, sediment storage components, and particle size distributions are similar. The large discrepancy between the estimated slope from ULSF and Caspar Creek suggests that there are some substantial differences in the transport relationships between these two sites, and there is not a clear physical basis for using the average of the two slopes. Using a steeper slope leads to a smaller increase in allowable peak flows to keep the increase in sediment yields to just 20%.

A more fundamental issue is the extent to which timber harvest is: 1) having a significant effect on the larger peak flows that have clearly drive sediment yields; 2) whether the harvest-induced peak flows have any geomorphic significance relative to the much greater variations in peak flows due to the variations in the amount and intensity of rainfall; and 3) the extent to which any changes in peak flows will rapidly diminish over time and with increasing watershed area. The forest hydrology literature (NRC, 2008) and an analysis of data from western Oregon and Washington (Grant et al., 2008) consistently show that forest harvest has a progressively smaller effect on the size of peak flows with increasing size of the peak flow, and a decreasing effect over time. At Caspar Creek logging 37% of the North Fork watershed produced an estimated 9% average increase in the 2-year peak flow, while selection logging increased the 2-year peak flow by an estimated 14% in the South Fork (e.g., Cafferata and Reid, 2013). A larger increase of 27% was observed for the 2-year peak flow in clearcut sub-watersheds, but these peak flows neared pre-treatment levels after 10 years. One would expect these changes to be smaller with improved logging practices, particularly given the improvements in the layout and treatment of skid trails. Grant et al. (2008) only found changes in the size of peak flows for recurrence intervals of 6 years or less, and that roads appeared to be a very significant contributor to the observed increases in the size of peak flows.

With respect to basin size, Grant et al. (2008) noted that the magnitude of any peak flow increase in response to forest management diminishes with increasing basin area because of channel resistance, flood-plain storage, and transmission losses. They also noted that no hydrologic mechanism exists (I would limit this to rain-dominated basins) by which percentage increases in peak flow can combine to yield a higher percentage increase in peak flows in a larger basin. These basic principles mean that control of hydrologic changes at the site or small watershed scale will effectively preclude cumulative hydrologic effects at larger scales.

More importantly, the magnitude of the observed changes in the size of peak flows are relatively trivial compared to the much larger interannual variability in peak flows, and any harvest-induced changes in peak flows will have a correspondingly low geomorphic effect. To quote from the abstract for Grant et al. (2008) "When present, peak flow effects on channel morphology should be confined to stream reaches where channel gradients are less than 0.02 and streambeds are composed or gravel and finer material." Furthermore, as spatial scale increases the magnitude of any increase in peak flow will rapidly drop to non-detectable, as the forest hydrology literature consistently indicates that 15-20% of a watershed must be cut in a very short time period in order to detect any change in annual water yields.

On pages 6-2 to 6-3 the Peer Review Draft argues that watershed-scale changes in hydrology are an important component in preventing and recovering impaired water quality. Three hydrologic modifications are noted: 1) "the prevalence of soil piping, in Upper Elk River poses a unique sensitivity to hydrologic modification which has contributed to their collapse, subsurface erosion, creation of in-channel sediment sources, and sediment delivery downstream."; 2) Deposition of fine sediment resulting in an increased magnitude and frequency of overbank flooding; and 3) "Hydrologic modification to the runoff patterns and drainage network in Upper Elk River has contributed to more rapid runoff and less infiltration of surface water into the soil mantle altering the natural groundwater recharge pattern. The decrease of base flow during the summer period adversely affects beneficial uses of water, including salmonid habitat and water supplies."

With respect to the first issue, there are few or no data on the extent to which current management practices are or are not affecting the presence and size of soil pipes, or the magnitude of the sediment that could be produced either from the erosion of soil pipes or changes in their size and frequency. Any practices that cut into the soil, such as road construction, can convert subsurface stormflow into overland flow, and this certainly has happened in the past with roads and also poorly constructed skid trails. This could contribute to gullying and channel incision, but it is not clear that this is currently happening given the limitations on new road construction and the changes in harvest practices. References on pipe erosion are limited, and the relevance of the reference on seeps and bank erosion at Goodwin Creek in Mississippi (Fox et al., 2007) to the steep hillslopes in the Elk River is questionable. From a process-based perspective, the conversion of subsurface flow to overland flow is always a concern, but the Peer Review Draft does not clearly show how the relatively small changes in flow from forest harvest will significantly affect pipeflow erosion and sediment yields at the hillslope or small watershed scale.

The deposition of fine sediment in downstream areas and associated increases in overbank flooding is well documented in the Peer Review Draft. However, this is basically a sediment production, storage, and delivery issue rather than a change in flow issue.

The third watershed-scale hydrologic change is basically arguing for an increase in overland flow and an associated reduction in infiltration, resulting in a decrease in base flow. There are a series of problems with this argument, as the forest hydrology literature generally shows that forest harvest **increases** base flows as a result of the decrease in summer evapotranspiration (and decreases interception if there are summer rainstorms). Less groundwater recharge could occur if there are large compacted areas that generate infiltration-excess overland flow, but this is almost certainly not the case for the Upper Elk River watershed. Again, any potential reductions in baseflows will rapidly diminish in the downstream direction, and would be dwarfed by the interannual variations in precipitation. There also is an inherent contradiction in that this hydrologic modification argues for less infiltration, which would reduce pore water pressures and the incidence of shallow landslides in harvest areas, yet the Peer Review Draft also points to forest harvest as increasing shallow landslides due to the reduction in interception and associated increase in soil moisture and pore water pressures.

Appendix 6A provides very little relevant support for the hydrologic modification argument, as most of the references pertain to more extreme hydrologic modifications such as urbanization, vegetation conversions, channelization, and surface water diversions. There is no mention of any of the forest hydrology literature after Jones and Grant (1996), but Appendix 6A does acknowledge that forest harvest typically increases baseflows, which contradicts the claim made with respect to the third hydrologic modification on p. 6-3. In short, the quality and relevance of the material in Appendix 6A is substantially below the rest of the Peer Review Draft, and provides very little support to the points being made.

5. Cumulative watershed effects.

Cumulative watershed effects are discussed in Chapter 6 and Appendix 6A. Appendix 6A defines cumulative watershed effects (CWEs) as “significant, adverse influences on water quality and biological resources that arise from the way watersheds function, and particularly from the ways that disturbances within a watershed can be transmitted and magnified within channels and riparian areas downstream of disturbed areas”. This definition, and its emphasis on magnification is quite different from the typical definition of cumulative effects drawn from the Council on Environmental Quality as a restatement of the principles expressed in the National Environmental Policy Act (e.g., Reid, 1991). Quoting from Reid (1991):

> “Cumulative impact” is the impact on the environment which results from the incremental 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.”

This definition was designed to clarify what needs to be considered in an environmental impact assessment, but the basic concept is that a cumulative effect results from the aggregated effects of natural sources and anthropogenic activities over time and/or space.

The Clean Water Act set up a discharge permit system for point sources, and the primary mechanism to deal with nonpoint sources, such as silviculture, was the establishment of Best Management Practices (BMPs). The iterative development, assessment, and improvement of BMPS has been one of the great success stories in terms of reducing the adverse impacts of forest management activities. As noted in the Peer Review Draft, the Independent Science Review Panel (ISRP) noted that "The best that could be done is to postulate a plan based on the best available information; continually test the plan using a combination of compliance, effectiveness, and trend monitoring; and revise the plan in a timely and appropriate manner based on monitoring results." (pp. 6-16 to 6-17). This is essentially an adaptive management approach, as it recognizes the uncertainty in quantifying values such as sediment sources and yields, and then linking the sources to either a specific water quality parameter or designated beneficial use.

The Draft Peer Review appears to contradict this basic approach by setting very specific targets that may not be readily attainable, and presuming that these targets (e.g., a 97% reduction in management-related sediment sources) are necessary to sustain the desired beneficial uses and water quality goals in terms of coldwater fish, nuisance flooding, etc. The Draft Peer Review argues against the use of an iterative adaptive management approach using BMPs because of: 1) "lack of effectiveness monitoring of individual BMPs", and 2) "it is difficult to rely solely on BMPs to ensure cumulative watershed effects are avoided and water quality standards are achieved".

With respect to the first point, there has been extensive monitoring of management practices and different sediment sources on both a formal and an informal basis, and some of the formal data have been cited in the Peer Review Draft. As one example of the informal monitoring, there has been a large, ongoing effort to treat sediment discharge points such as stream crossings, and the informal monitoring of these efforts has led to changes that have successively reduced the frequency and amount of sediment generated post-treatment. It is exactly this type of monitoring that is most efficient in reducing sediment sources. Other examples of the BMP approach include road-induced landslides and road surface erosion, road stormproofing, and efforts to reduce erosion from skid trails and harvest units. The large changes in management practices have greatly reduced management-related sediment production from the initial tractor logging after the Second World War to the present-day. The data from Caspar Creek show clear recovery over time following logging, and the trends documented by Sullivan et al. (2012) generally show a reduction in sediment yields over time from managed watersheds. The ongoing Beck's BMP effectiveness monitoring project also indicates that the careful implementation of BMPs is able to reduce management-related sediment sources to nearly non-detectable levels.

The Draft Peer Review cites the paper by Klein et al. (2011) as evidence that the BMP approach is not sufficient. The conclusions of this paper: 1) identify the rate of harvest as the primary concern; and 2) use this to conclude that present BMPs are limited in their effectiveness. Appendix 1 to this document presents a detailed explanation as to why Klein et al. (2011) cannot be used to justify a broad-scale restriction on the rate of timber harvest. Instead, a field- and process-based approach should be used to identify which current management activities are generating and delivering sediment to streams, and which BMPs need to be further altered to minimize management-related sediment inputs.

Appendix 1 also provides a detailed critique of the weighting coefficients used to calculate clearcut equivalent area in Klein et al. (2011). These coefficients are presented in Table 4.28 as "canopy removal coefficients", and these values may be approximately valid for canopy removal. The problem is that these coefficients inherently presume that the change in interception is the primary driver of management-induced increases in peak flows and sediment production rates rather than the change in infiltration or any of the erosion processes operating in the Elk River watershed. Grant et al. (2008) note the disproportionate effect of roads on peak flows relative to forest harvest, and this is primarily due to their negligible infiltration rate and associated generation of overland flow rather than the reduction in interception. For surface erosion the unit area coefficients for roads would generally have to be at least an order of magnitude higher than for harvest units due to the much lower infiltration rate and the reduction in surface cover, and this would be clearly evident if the sediment loading values for roads and harvest units in Table 4.32 were adjusted according to the area occupied by roads and harvest units, respectively. Similarly, on a per unit area basis, roads have a much greater effect on the frequency of landslides than clearcutting or other forms of forest harvest (e.g., Sidle and Ochiai, 2006), and again this view is strongly supported by the relative magnitudes of road-related versus open-slope shallow landslides in Table 4.32 should these values be normalized per unit area of a given activity.

From a process-based perspective, the effect of a given management activity is likely to be diminished in the downstream direction due to storage, dilution, dispersion, and transformations (MacDonald and Coe, 1997). This means that if the different sediment sources are minimized at the source, it is difficult to come up with a process-based argument for the generation of significant downstream cumulative watershed effects (MacDonald, 2000), or "magnification" as stated in the definition of cumulative watershed effects in Appendix 6a (note that a citation was desired, but none was entered). If the staff want to propose the numeric hillslope target "*Within a Class I subbasin, and within an individual ownership, the maximum average annual timber harvest rate is 1.5%*" (p. 6-19), this must be first be justified at the site scale with process-based arguments showing a substantial change in hydrology or erosion rates under current practices. The effects at the site scale then need to be routed downstream and shown how they will collectively result in a significant cumulative watershed effect in terms of precluding recovery, causing nuisance conditions, or adversely affecting cold freshwater habitat.

6. No new management discharge sites.

The draft TMDL calls for a target of "*No new management discharge sites created*." (p. 6-11, italics in the original). A better target would be no **net** increase in discharge sites. The rationale is that in some cases it may be better to move a streamside road to a mid- or upslope position, but doing this could easily create additional watercourse crossings. Such crossings will inevitably produce some small amount of sediment as discussed under section 3.4. However, the logical presumption is that shifting a road to a less sensitive location will result in an overall net reduction in sediment loading over time. Similarly, the target for sediment production should be formulated in a way that would allow for some new sources as long as there is substantial net decrease over time.

7. Validity of target values for streams and restoration goals.

Table 6.2 sets forth a set of instream habitat indicators and target conditions for sediment. Among others, this specifies that: 1) for all wadeable streams and rivers with a gradient of <3%, ≤14% the bed sediment should be less than 0.85 mm and ≤30% should less than 6.4 mm; 2) for streams with a slope of 1-4% the median particle size (D_{50}) should be 65-95 mm; 3) percent embeddedness in all wadeable streams and rivers should be no more than 25%; and 4) an increasing trend in the number of reaches in all wadeable streams and rivers with at least 40% of their length in primary pools. The primary reference for these indicators and target conditions is Fitzgerald (2006), but I was not able to review this as it was not listed in the references. My concern is that many of these targets may be unrealistic, and therefore unattainable, for many of the reaches to which they are presumed to apply. Several studies have also questioned the usefulness and accuracy of embeddedness measurements.

According to the report by Paternaude (2004), the channel substrate at the cross-section established by the USGS in 1958 varied from mud to gravel, with finer material in winter to gravel and stones in summer. According to the Peer Review Draft, the dominant substrate observed in the 3000 m of streams inventoried for bank erosion in the Little South Fork was "primarily sand-sized particles with minor amounts of cobble and gravel" (p. 4-8). Given the lithology within the Upper South Fork and the poor competence of most of the rocks, it is not clear to what extent one can realistically expect ≤14% coarse sand, or ≤30% finer than 6.4 mm. Similarly, the expectation of at least 40% of the length of a stream to be in pools is very high unless many, if not most, of the pools are formed by large woody debris. All of these criteria are highly dependent on gradient and stream type, but there is no explicit recognition of the extent to which these criteria may or may not be applicable to each of the different stream types in the Upper Elk River watershed. If the bed material in the reference watershed is primarily sand-sized particles and there are plane-bed streams in some portions of the watershed, what are the realistic expectations for coldwater fish habitat?

The role of large woody debris for fish habitat, affecting bank erosion, and storing sediment is largely ignored in the Peer Review Draft. There also is no target for large woody debris. Yet the Peer Review Draft acknowledges that there was splash damming, and it is highly likely that much of the large wood was removed from the channels during the early logging period. The presumed loss of large woody debris may have had a profound effect on channel incision, bank erosion, and streamside landslides, but typically the role of large woody debris is only loosely acknowledged in the discussion of the possible controlling causes for these different processes, and how the rates might be reduced.

8. Use of significant figures.

The Peer Review Draft often implies a much greater accuracy and precision in the values presented in the report than can be justified by the data. This implies a much greater certainty in the results than is actually the case. Numerous examples can be provided, but the calculations of stored sediment exemplify the problem, as the text on pages 3-17 to 3-18 explicitly note the difficulty in calculating the volume of instream sediment deposits, but Table 3.3 shows values with six significant figures (e.g., 259,428 yd^3). The use of six significant figures indicates that we know the volume to the 0.5 yd^3 or to within 1/259428, or 0.0004%. Many more examples can be cited, such as the bank erosion loadings in Table 4.8 being reported to

0.01 yd^3 mi^{-2} yr^{-1}, which converts to an accuracy of approximately ±10 kg per square mile. Values should be rounded to reflect their uncertainty, and in nearly all cases we don't know the true values to within any greater accuracy than ±1%, and in most cases to only ±10% or worse. The use of significant figures throughout the Peer Review Draft will greatly help in evaluating whether a difference is possibly a "true" difference or just noise, and which values are large enough to deserve more attention.

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Use of Klein et al. (2011) for Justifying Restrictions on Timber Harvest Rates

1. INTRODUCTION AND OVERVIEW

The 2011 paper by Klein et al. ("Logging and turbidity in the coastal watersheds of northern California") provides a useful and interesting assessment of the statistical relationships between a set of watershed and management variables on the one hand, and 1-3 years (depending on the analysis) of 10% exceedence turbidity values for 28 coastal watersheds in northern California. This paper is being extensively used by the North Coast Regional Water Quality Control Board (NCNCRWQCB) to justify the numeric hillslope target for the rate of timber harvest as follows: "*Within a Class 1 subbasin, and within an individual ownership, the maximum average annual timber harvest rate is 1.5%.*" (pp. 6-17 to 6-19, NCRWCB, 2013).

The purpose of this white paper is to evaluate: 1) the extent to which the results of Klein et al. (2011) can or cannot be used to justify this harvest rate threshold; and 2) suggest how additional analyses and follow-up studies could further assess the relationship between the rate of timber harvest and watershed-scale turbidity levels, and use these results to guide future management. The focus is on the broader validity of Klein et al. (2011) for understanding the effects of timber harvest and guiding future management, rather than a broader evaluation of the validity of the cumulative watershed effects section in the Peer Review Draft (pp. 6-16 to 6-19, NCRWQCB, 2013). In providing this commentary I fully appreciate that no study is perfect, and that every study has constraints in terms of time, data availability, and statistical uncertainties. Hence there is no intent to criticize the three authors of Klein et al. (2011), but simply to point out some of the underlying scientific issues that—in my professional opinion—limit the extent to which the paper by Klein et al. (2011) can be used to understand the linkages between harvest rate and turbidity, and to justify the proposed limits on the rate of timber harvest.

There are four main issues that constrain the use of Klein et al. (2011) for justifying the proposed limit on the rate of timber harvest. These are briefly summarized below and then discussed in more detail in the following sections. First, the weighting factors used to quantify the combined effect of different management activities by calculating equivalent clearcut areas are only appropriate for the estimated change in canopy cover. The relative weighting factors listed in Table 3 are not valid to represent the differences in other key processes, including infiltration, surface erosion, and landslide rates, respectively. As acknowledged by the authors (p. 143 in Klein et al., 2011), the weighting factors also implicitly combine both the relative effect on erosion and the relative likelihood that this sediment will reach the stream. Yet these differences in the likelihood of sediment delivery do not appear to be considered in the tabulated values of the weighting factors (see p. 143 in Klein et al., 2011). Finally, there does not appear to be any consideration of how these weighting factors should change over time; for example, the weighting factors for timber harvest should decline over time, whereas roads will not have the same reduction over time. The values of these weighting factors over time are important because these directly affect the calculation of equivalent clearcut areas (ECA), and any change in ECA values is likely to alter the statistical results, specifically the conclusion that 10-15 year ECA is the key control on 10% exceedence turbidities.

Second, the watershed-scale data and statistical analyses in Klein et al. (2011) do not provide any definitive links to the underlying causal processes. The paper does find a statistically significant correlation between mean annual equivalent clearcut area (ECA) for 1990-1994 and 10% turbidity exceedance values, but the underlying cause of this correlation can only be inferred. The value of the study for setting management guidelines is severely constrained by the lack of information or inability to quantify key watershed variables (such as geology, landslide rates, or longer-term legacy effects), the cross-correlation between the various management variables listed in Table 2, and the inherent assumptions and problems associated with the calculation of the ECA values. In the absence of any supporting data on the relative importance of different sediment sources over time (e.g., roads, channel erosion, shallow landslides, deep-seated landslides, legacy effects), the presumed dominant role of harvest-related shallow landslides due to canopy removal can only be inferred. The proposed imposition of a limit on harvest rate necessarily converts this statistical inference to a known and primary cause.

The same concerns apply to the other key result (Figures 2 and 3; Table 5), where the watersheds are classified into four management-related groups. It appears that most of the differences between the low and high harvest rate categories are due to relatively high turbidity values in three or four watersheds. These same watersheds may also be strongly influencing the results of the multivariate analysis. To evaluate the more general validity of the relationship between rate of harvest and turbidity, one should first determine whether there is anything particularly unique about these watersheds (e.g., geology, legacy effects, high rainfall events) and the most likely potential causes of the observed high turbidity (sediment source analysis). Similarly, the watersheds classified as high harvest with relatively low turbidities (57% of the 14 high harvest data points in Figure 2) should be investigated to determine why these watersheds are more resistant or resilient to forest management activities in terms of having lower 10% exceedence turbidities. These additional analyses may be beyond the scope of the original paper, but there is a considerable amount of data for many of the watersheds used in the study. This more detailed assessment of the high harvest watersheds also is needed to determine the extent to which three or four of these watersheds are driving the statistical results, and why there is such wide variability in the turbidity values (e.g., compare Whitlow and Inman Creeks to the South Fork of Elk River). This understanding of the variations in watershed response is needed before one can justify imposing universal restrictions on the rate of harvest.

Third, only the 2005 data were used for the multiple regression analysis (p. 139)). A recent study of 17 watersheds in Elk River and Freshwater Creek analysed the relationship between the 10-15 year ECA and 10% exceedence turbidity on a year-by-year basis from 2003 to 2011 (Sullivan et al., 2012). This showed that there was a statistically significant relationship between 10-15 year ECA and 10% exceedence turbidity only for 2004 and 2005, which are the two years that provided most of the data in Klein et al. [2011]), while the relationship was non-significant for each of the other seven years and progressively weakened from 2005 to 2009 (Table 16, p. 110 in Sullivan et al., 2012). The results of a multivariate analysis with multiple years of data showed that both basin area and the 10-15 year ECA were statistically correlated with the 10% exceedence turbidity, but in contrast to Klein et al. (2011) the 10% exceedence turbidities were **negatively** correlated with the 10-15 year harvest rate (Table 15, Sullivan et al., 2012).

The point is that the relationship between the 10-15 year ECA and 10% exceedence turbidity is not consistent over time, and both the significance and the direction of the relationship appears to depend on which watersheds and years are being analysed. Hence the relationship between ECA or harvest rate and turbidity—while true for 2005—cannot be more broadly generalized without further investigation, and therefore should not be used as a basis for regulating the rate of timber harvest.

The fourth main limitation is that the conclusions: 1) identify the rate of harvest as the primary concern; and 2) use this to conclude that present BMPs are limited in their effectiveness. If the statistical analysis is flawed by the weighting factors used to calculate the ECA or the results are only valid for certain years, the first conclusion may be incorrect. The second conclusion is not supported by the body of the paper, as no data are presented on the extent, implementation, or effectiveness of BMPs. In this context it should be mentioned that changes in forest management have greatly reduced sediment production rates over time (Sullivan et al., 2012; NCRWQCB, 2013; Cafferata and Reid, 2013). The BMP approach is clearly an iterative process (MacDonald and Coe, in press), but given the legacy effects in the Elk River watershed, the time lags associated with sediment storage and delivery, and the lack of a more direct physical linkage between different sediment sources and the observed turbidities, the authors cannot conclude that the BMP process has, or is, failing, or what specific BMPs are most in need of revision. (Note that most of the citations here are more recent than the paper by Klein et al. [2011], but two of these are compilations of studies and data that were mostly available before the paper was revised and resubmitted in October 2011.)

2. WEIGHTING FACTOR FOR DIFFERENT MANAGEMENT ACTIVITIES

With respect to the first point (the weighting factors listed in Table 3), the values should be selected to represent the relative impact of each activity on the most important processes that produce and deliver sediment to the stream network. The values of these weighting factors have been justified by presuming that the relevant process is the change in forest canopy, which then affects rainfall interception and the amount of net precipitation. However, turbidity is generated by erosion and the delivery of the resulting sediment to the stream, and this can come primarily from changes in: 1) peak flows, which would increase turbidity through channel scour and streamside landslides; 2) surface erosion (rainsplash, sheetwash, rilling, and gullying); and 3) the frequency and/or size of mass movements. In a perfect world we would have comparative data on how each of these three sets of processes are affected by forest management, and the weighting factors would be set according to the known or estimated changes in each set of processes. So for the change in peak flows the weighting factors should be set according to the net change in interception, reduced infiltration leading to infiltration-excess or saturation overland flow, delivery of subsurface stormflow, and increased soil moisture due to reduced transpiration. For the change in surface erosion the weighting factors should primarily reflect the differences in the amount of infiltration-excess (Horton) overland flow and the amount of bare soil (e.g., roads would have a much higher coefficient than clearcuts). For mass movements the weighting factors should reflect the net change in processes that lead to slope instability (primarily reduced root strength, increased pore pressures due to road runoff or increased soil wetness, and mechanical hillslope weakening or overloading to due road cuts and fills).

Extensive literature and data from areas such as Caspar Creek (see Cafferata and Reid, 2013) and the Elk River watershed show that the weighting factors for both surface erosion and landslides should be much higher for roads on a per unit area basis than for clearcuts or other types of timber harvest. The sediment source analysis as summarized in Table 4.32 in the Peer Review Draft (NCRWQCB, 2013) shows that road surface erosion was about 70-190 tons mi^{-2} yr^{-1} from 1955-2003 and most recently (2004-2011) about 24 tons mi^{-2} yr^{-1}, while surface erosion from timber harvest units has always been less than 10 tons mi^{-2} yr^{-1} (note that these values are NOT normalized for the area of each activity, so the weighting factors for roads, if expressed per unit area of activity, should be even higher since roads occupy a much smaller proportion of the watershed than harvest units). Skid trails, which act more like roads but for practical purposes generally have to be included in harvest unit erosion, are most recently estimated to contribute 21 tons mi^{-2} yr^{-1} but in the past have ranged from 5-36 tons mi^{-2} yr^{-1} (NCRWQCB, 2013). For mass movements, Sullivan et al. (2011) noted that about half of the landslides from the 1997 storm were from roads, and half from clearcuts; Sidle and Ochiai (2006) also noted that roads have a much greater effect on landslides than harvest units. The Peer Review Draft estimated that open-slope shallow landslides generated 7 tons mi^{-2} yr^{-1} from 2004-2011 as compared to previous values of 8-280 tons mi^{-2} yr^{-1}, while the current (2004-2011) estimate for road-related landslides are is seven times higher at 35 tons mi^{-2} yr^{-1} (Table 4.32, NCRWQCB, 2013). Again roads generally occupy far less watershed area than harvest units, so roads must have far greater weighting coefficients than clearcuts for landslides as well as for surface erosion. Yet the weighting factors for roads and clearcuts in Klein et al. (2011) were both set at 1.0, and the factors for sanitation salvage and seed tree seed cut were set at 0.75, or only slightly less than roads despite the expected large differences in infiltration and exposure of bare ground between roads and these two types of timber harvest. The weighting factor of 0.5 for selection harvest and group selection also could be considered as too high for most sediment production and delivery processes, and Klein et al. (2011) appear to acknowledge this in their discussion on page 143.

Hence the respective weighting of clearcuts and roads in Table 3 in Klein et al. (2011) could only be considered valid if the change in peak flows is the dominant sediment source, and the change in peak flows is solely due to the change in interception rather than a change in infiltration or the routing of water on the hillslopes. Problems with the weighting factors in Table 3 have been explicitly acknowledged by the authors (e.g., p. 143 in Klein et al., 2011).

In the absence of any specific process-based knowledge of the causes for the observed turbidities, one possible approach is to generate three sets of weighting factors to more accurately represent the generalized changes in peak flows, surface erosion, and mass movements, respectively. Some of the weighting coefficients also should vary with easily determined site characteristics; for example, one might apply the weighting coefficients for mass movements only to those areas with a certain, empirically-defined slope gradient (see Sidle and Ochiai, 2005). The weighting factors for different management activities also need to account for both the relative change in erosion rates and the differences in connectivity between the different management actions and the stream network. As one example, surface erosion from group selection might be much less likely to reach a stream than surface erosion from a road. The weighting coefficients used by Klein et al. (2011) implicitly combine both the relative effect on erosion and the relative likelihood that this sediment will reach the stream, but in fact the difference in connectivity does not appear to influence the weighting factors in Table 3 despite the authors noting that “Erosion from cut units is less likely to reach a stream” and “logging road stream crossings are perhaps the most prominent sources of delivery of sediment to streams” (p. 143). As noted earlier, the higher connectivity of roads to streams relative to clearcuts argues for even lower weighting factors for harvest units compared to roads. It also is of interest that the paper cites the number and length of breakthroughs of runoff from harvest units to streams by Rivenbark and Jackson (2004) for the southeastern US, but omits any reference to the comparable work by Litschert and MacDonald (2008), who found much fewer and shorter erosion features (rills or sediment plumes) from recent harvest units, and much fewer breakthroughs. While the MacDonald and Litschert paper is from the northern Sierra and southern Cascades rather than the North Coast and was limited to USDA Forest Service lands, it may still be more relevant given that the BMPs in the Rivenbark and Jackson (2004) study are almost certainly weaker than the BMPS being applied on private industrial timber lands in California.

The weighting factors for different management factors also need to have their respective decay curves defined. For example, the coefficient for surface erosion from clearcuts should decline relatively rapidly while surface erosion from roads might have little or no recovery over time except after construction or grading. The three sets of coefficients and their respective recovery curves should then be used to calculate three separate sets of management indices for each watershed and each year. The multivariate analysis could then be rerun with each set of coefficients to determine which set is most highly correlated with the 10% exceedence turbidity, and the relative results could then suggest which management actions are of greatest concern, and which generalized erosion process is best correlated with the 10% turbidity exceedence value. Or an expert panel could come up with a single set of weighting factors through some kind of Delphi method. Only after these steps could one start to infer how management might be altered to minimize the observed increases in turbidity, and what type and magnitude of management restrictions and BMP improvements would be most likely to help achieve water quality standards over what time period. This inference would then need to be verified by a more in-depth evaluation of the sediment sources in the watersheds with the highest turbidities.

An increase in the relative weighting of roads versus clearcuts is likely to increase the importance of roads with respect to increasing turbidity while decreasing the importance of clearcuts and other types of timber harvest (i.e., the much greater area in clearcuts than roads may have led to a much higher importance of clearcuts in driving the results when the road and clearcut weighting factors were both set to 1.0). If the weighting factor for roads is much higher than for clearcuts, then one might expect road length, area, or density per unit watershed area to become a more significant explanatory variable. Klein et al. (2011, p. 142) explicitly noted that road densities were closely correlated with the amount of forest harvest (r=0.80 for the correlation between road density and 15-year mean harvest rate), so it is not too surprising that roads added little explanatory power.

In summary, the weighting factors in Table 3 are not valid for all the key processes that cause forest management activities to increase in erosion and turbidity. A reanalysis using different sets of values representing different sets of processes may lead to very different results, and it would provide (somewhat) better insight into the underlying causes of the observed differences in turbidity. Different results would have different implications for management guidelines and the relative effectiveness of current BMPs. In presenting these points, I fully recognize that the development and testing of different weighting factors would require substantially more time and effort, so this is not a direct criticism of the results, but simply an explanation of the limitations associated with the interpretation of the results in Klein et al. (2011).

3. LACK OF A PROCESS-BASED EXPLANATION

As suggested above, a major limitation in applying the results of Klein et al. (2011) is that the paper found a statistical relationship between 10-15 year ECA and 10% exceedence turbidities, but this relationship cannot be tightly coupled to a primary causal process. Klein et al. (2011) represents a useful first step in terms of a providing a broad geographic perspective and an interesting correlation between management and turbidity, but the statistical results cannot be used to set management guidelines or thresholds because the underlying causal processes have not been sufficiently identified.

Erosion studies and watershed analyses have been conducted in a number of these watersheds, and this information should be used to more rigorously identify the primary causes of the turbidities observed in the different watersheds (particularly the higher turbidity watersheds in Table 4 and Figure 2). Dr. Kate Sullivan has stated that the six highest points in Figure 2 come from just four watersheds, and it is only these six out of 14 points that appear to cause the significant difference in turbidities between low and high harvest watersheds (Figure 2). By combining the data in Figure 2 with the 2005 turbidities in Table 4, the four key watersheds (in declining order of turbidity) can be identified as the South Fork of the Elk River, the North Fork of the Elk River, the South Branch of the North Fork of the Elk River, and Freshwater Creek at HH bridge. Given the studies conducted by the different landowners and other parties, a first step would be to use this known information to identify the likely cause(s) of the high turbidities in these four watersheds (e.g., the number and causes of landslides, number of unimproved roads and road crossings). (Note that any change in the coefficients in Table 3 will affect the calculated ECA values, which might alter which watersheds are classified as low and high harvest, respectively.)

Legacy effects are dismissed as important relative to modern logging practices (p. 143, Klein et al., 2011), and this is based on the comparisons in Table 5 and Figure 2. The data in Figure 2 indicate that the significant difference in turbidities between the high harvest watersheds and the legacy watersheds (Table 5) are primarily due to the eight of the 14 turbidity values from the watersheds in the high harvest category; the other six points or 43% of the data either overlap with, or are not much higher than the maximum turbidities observed in the legacy watersheds. This means that in some cases the high harvest watersheds did NOT have unusually high turbidities, again showing that the relationship between harvest rate and turbidity is not consistent. And if the relationship is not consistent, can the results be used to justify a universally-applied restriction on the rate of timber harvest, or would it be more effective to focus on those management activities that alter the primary causal processes?

Klein et al. (2011, p. 143) also state that "the extent of water quality degradation in actively harvested watersheds, wherein modern BMPs are used, cannot be solely attributed to residual effects of more destructive harvesting practices of the past". I think that everyone would agree with this statement, as no one is likely to claim that current water quality degradation is **solely** due to residual effects of more destructive harvesting practices in the past. However, the use of mean rather than median values tends to overemphasize the importance of the very high turbidities from the problematic watersheds like the South Fork of Elk River.

A second step would be to use existing studies to document the most likely causes of the high turbidities in the eight or so watersheds with particularly high turbidities (Table 4), and why three of the watersheds classified as high harvest only have 10% exceedence turbidities of 26-33 NTUs (Tables 1 and 4). A short table with the associated references could be prepared that would summarize the dominant sediment sources for these watersheds. With a little more effort this effort could be expanded to all of the watersheds that have readily available data (e.g., the North and South Forks of Caspar Creek). Such a compilation would help to relatively quickly determine if the amount or rate of harvest is indeed closely coupled with high turbidities, or whether geology, roads, or legacy sources are a key cause. This more explicit analysis is another of the logical next steps beyond Klein et al. (2011) to help determine the underlying cause(s) of the observed correlations.

4. LIMITED TURBIDITY DATA OVER TIME

The analyses and results from Klein *et al.* (2011) are based on a relatively limited sample of turbidity data over time. From Table 1 one can calculate that their primary data set includes turbidity data from 11 watersheds for 2003, 24 watersheds for 2004, and 27 watersheds for 2005. They found that the 10-15 year ECA was statistically related to the 10% exceedence turbidity in a multiple regression analysis, and that there was a significant difference in turbidity levels between the watersheds classified into "high harvest rate" and low harvest rate. However, this harvest "rate" appears to be the mean annual ECA for a five-year period rather than a changing rate over time. Klein et al. (2011) did examine other time periods, but the strongest relationship was for the ECA for 1990-94. Hence their primary result uses the common approach of substituting space for time. However, if one wants to more rigorously prove that turbidity is directly related to harvest rate, a stronger test is to document how turbidities change over time within a watershed as harvest rates change over time, as this will eliminate the effect of watershed variability in turbidities (which are a function of geology, slope, etc.) and the varying sensitivities of different watersheds to a given set of management activities. A tracking of turbidity and harvest rates over time from multiple watersheds, similar to Figure 23 in Cafferata and Reid (2013), would more rigorously evaluate the strength of the linkage between timber harvest rates and turbidity.

The difference between these two approaches may seem academic, but the effect of different approaches can be shown by comparing the results of other analyses using longer datasets. To test the validity of the results in Klein et al. (2011), regression models were developed between the 10% exceedence turbidities and the 10-15 year ECA for about 17 watersheds in the Elk River basin for each year from 2003 to 2011 (Sullivan et al., 2012). The results confirmed a statistically significant relationship between the 10-15 year ECA and 10% exceedence turbidities for 2004 ($p=0.037$, $R^2=0.491$) and 2005 ($p=0.015$, $R^2=0.714$), supporting the results of Klein et al. (2011). However, the results for 2003 were not significant with a p-value of 0.362, and the p-values from 2006 to 2011 ranged from 0. 105 to 0.421, again indicating no significant relationship. According to Sullivan *et al.* (2012), the lack of a significant relationship for the other years could not be attributed to an increase or decrease in the rate of harvest, as half of the watersheds had an increase and half had a decrease. When Sullivan et al. (2012) used a multivariate approach similar to Klein et al. (2011), both basin area and the 10-15 year ECA were significantly related to the 10% exceedence turbidity, but in contrast to Klein et al. (2011) the 10% exceedence turbidities were **negatively** correlated with the previous 10-15 year ECA (Table 15 in Sullivan et al., 2012).

These findings first indicate that the significant relationship between the 10-15 year ECA and 10% exceedence turbidity from Klein et al. (2011) may not be consistent if additional years of data were included. Of course Klein et al. (2011) could only use the data that were available to them, but the conflicting results from these two studies indicate that the relationship between the 10-15 year ECA and 10% exceedence turbidity is not very robust, and that the significant results in Klein et al. (2011) may somehow be a statistical artifact of the data from 2005. A more extensive analysis is clearly needed to determine which points are driving the positive relationships found by Klein et al. (2011) and Sullivan et al. (2012) for 2004 and 2005, the lack of a significant relationship for other years (2003, 2006, 2007, 2008, 2009, 2010, and 2011), and the negative relationship found by Sullivan et al. (2012) for the entire dataset.

Contrasting results are often encountered in scientific studies, but at a minimum the results from Sullivan et al. (2012) call into question the presumed cause-and-effect relationship between harvest rate and 10% exceedence turbidity from Klein et al. (2011). In summary, the relationship between harvest rate and turbidity should not and can not be generalized without further work, and should not be used for regulating forest harvest rates until the relationship between the rate of timber harvest and turbidity is proven to be more consistent over time and space rather than an artifact of any particular year(s) or watersheds.

The logical step to resolve these issues is to assemble as large and complete a dataset as possible of watershed variables, management variables, turbidities, and sediment yields, and then conduct a series of analyses across time and possibly different subsets of watersheds. Focusing on a smaller geographic area, such as Elk River and Freshwater Creek, could be beneficial in terms of reducing between-watershed variability. This would effectively be an entirely new study, as it would require assembling a consistent turbidity record from a relatively large number of datasets, conducting the analyses, and writing up the results, but it would be the best way to clarify these conflicting statistical results (the physical causes of the high turbidities would still need to be confirmed to provide the associated management guidance).

Of course a number of methodological issues would have to be resolved in such a study, such as the fact that timber harvest practices have changed substantially over time, and that the effects of a given activity are highly site dependent. The first point is means that the effect of a 1990s clearcut may be very different than current clearcuts using practices such as shovel logging, and the second is that a clearcut on a steep slope may have a very different effect on sediment production than a clearcut on a flatter ridgetop. Similar issues apply to characterizing roads or any other management activity, as a carefully designed ridgetop road in 2011 should have a very different effect than a midslope road built in the 1970s. One also would need to account for the differences in rainfall over time, as rainfall is the dominant control on annual sediment yields and presumably turbidity (Sullivan et al., 2012). The WY2005 precipitation variables included in Klein et al. (2011) are useful given the wide geographic range of their study watersheds, but a more extensive and rigorous study would need to consider the variations in both rainfall amount and intensities (or erosivities) over time for each watershed. The difficulty of quantifying the combined effects of different management activities over time is one of the major limitations to analyzing and predicting cumulative watershed effects (MacDonald, 2000), and any changes in practices over time make it problematic to use past relationships to justify changes in current management practices.

5. EFFECTIVENESS OF BMPS

With respect to BMPs, it is self-evident that they are "neither perfectly conceived nor perfectly implemented" (Klein et al., 2011, p. 143). However, numerous studies have shown that BMPs generally have a very high degree of implementation and effectiveness (e.g., Ice et al., 2010; HRC Roads and BMPEP report, 2012; California's Forest Practice Rules Implementation and Effectiveness Monitoring Program). I also agree that a continuing review of BMPs and water quality data is essential, and that the development of BMPs is an ongoing, iterative process of adaptive management (MacDonald and Coe, in press). However, a simple correlation between 10-15 year ECA and 10% exceedence turbidity is not sufficient to question the overall effectiveness of the BMP approach for meeting water quality standards. Klein et al. (2011, p. 141) note that "Other models using just harvest rate (including annual mean harvest rate 0-15 years prior to the turbidity record) also performed well." This suggests that recent sources of sediment also are contributing to the observed turbidity, and that harvest-induced landslides due to a reduction in root strength almost certainly is just one part of the explanation for any relationship between forest management and turbidity.

Strong BMPs are clearly warranted in more erosion-prone terrain, and current regulations require a calculation of erosion hazard based on slope and soil type, and more stringent BMPs in more erosion-prone areas. Special restrictions also should apply in those areas that are mapped as unstable or potentially unstable. Recent data suggest that sediment from harvest-induced landslides is already being reduced by changing harvest practices, particularly in susceptible terrain (Sullivan et al., 2012; NCRWQCB, 2013), and the data in the Draft Peer Review indicate a reduction in sediment sources over time (Table 4.32, NCRWQCB, 2013. This means that BMPs, changes in management, natural recovery, and restoration efforts are reducing sediment production and delivery to streams.

In their conclusions Klein et al. (2011, p. 143) state that "severe degradation of water quality can occur despite use of BMPs in watersheds where too much of the land base is harvested over too short of a time period." While the first part of this statement may be true in some cases, they do not provide sufficient evidence to claim that "severe degradation of water quality" is due to "too much of the land base is harvested over too short of a time period" due to the limitations of their study discussed previously and they cannot directly identify the cause(s) of the measured high turbidity levels. Similarly, the conclusion that "limiting the rate of harvest in erosion-prone terrain . . . could do much to close the gap between what regulatory programs desire to achieve and actual water quality conditions in streams" is a presumption rather than a statement that can be directly supported by their results. Their conclusions with respect to the validity of **current** BMPs are also undermined by the fact that the strongest relationship between 10% exceedence turbidity was with ECA from 1990-1994, but forest practices and BMPs have changed substantially over the intervening twenty years (see Sullivan et al., 2012; NCRWQCB, 2013; California's Forest Practice Rules from 1990 and 2013). In the absence of any specific data on the implementation and effectiveness of **current** BMPs, Klein et al. (2011) cannot be used as proof that current BMPs are inadequate.

As one suggestion, it might be useful to conduct a systematic assessment of current BMP implementation and effectiveness, as this would provide more guidance with respect to the adequacy of current practices. Humboldt Redwoods is conducting relatively intensive studies of how intensive forest management is affecting sediment sources, turbidity, and sediment loads in two sub-basins, Beck's Gulch and Railroad Gulch, but these are relatively expensive, longer-term studies. An example of a more rapid assessment is the work of an interagency panel to assess the potential for water quality impacts from areas managed for high-yield timber production, and this found that the highest magnitude of sediment delivery was from poorly located roads or roads with poor BMP implementation rather than clearcuts (The California Resources Agency, 2011). A somewhat similar approach could randomly select recent Timber Harvest Plans from key watersheds such as Elk River and Freshwater Creek, and then select random harvest units, road crossings, and other management actions for auditing. An interdisciplinary team could then go out and determine: 1) if the BMPs were implemented as planned; 2) if the BMPs appeared to be effective; and 3) what changes in BMPs would be justified given the results of the audit. Ideally this would be done after a large storm event as well as during the dry season.

6. CONCLUSIONS AND FUTURE WORK

In short, the paper provides a useful jumping off point, but cannot be used to justify the strict, broad-scale regulation on the rate of harvest as suggested in the Peer Review Draft (NCRWQCB, 2013, p. 6-19). The last paragraph in Klein et al. (2011) argues for "a working, continually updated knowledge of the complexities of natural and anthropogenic factors and their interactions" and "a watershed approach…that addresses the full spectrum of potential effects of timber harvesting". I agree, but with the caveat that we not only need to look at the effects of timber harvesting, but also the effects of all the associated activities, particularly roads, skid trails and cable rows, and road crossings. The problem is that the paper by Klein et al. (2011) is being used to support a limitation on percent harvest over time when the paper does not provide the explicit, process-based data needed to show that **current** harvest rates (or more accurately, ECA) can be explicitly and consistently linked to a current degradation of water quality in the form of higher turbidities. In the absence of a proven and consistent relationship between current practices and turbidities, the paper cannot be used to justify any specific new management regulations to minimize the effects of current forest management activities on turbidity and sediment loads.

As discussed above, there are two approaches that could be used to provide more insight into the statistical linkages between turbidity and forest practices, both past and current. The first, less costly approach would be to do some additional analyses using the same turbidity data and many of the same variables as in Klein et al. (2011). The first set of analyses would involve several steps. 1) A small group could be convened to develop one or more new sets of weighting factors based on key processes. These would then be used to recalculate the integrated effect of roads and timber harvest, and then evaluate the relationships between turbidity and the recalculated management indices. 2) Determine the extent to which the key results depend on: a) a relatively few watersheds that have intensive management and high turbidities, and b) determine how many of the intensively-managed watersheds have the opposite relationship (i.e., high management and low turbidities). This will help indicate the robustness of the relationship between management and turbidity. 3) Compile existing information on the watersheds identified in a) and b) in order to determine what processes are driving the high turbidities in some of the intensively managed watersheds, and why some intensively managed watersheds have much lower turbidities than other watersheds with similar management intensities. The combined results would then provide much more insight into the strength, consistency, and causes of any linkage between management and turbidity. This analysis and associated write-up might require around 2-3 months of time by scientists and statisticians, and the provision of reports and other information from the primary land owners. This approach also could be simplified by eliminating some of the watersheds that are further and possibly less directly relevant to the Elk River watershed, but the trade-off is a reduction in sample size.

The second and more costly approach would be to conduct a similar study with a smaller geographic focus but using more years of data ("Klein-like study"). Sullivan et al. (2012) has compiled the data for 2002 to 2011 from 22 hydrology stations in Elk River and Freshwater Creek, and these data plus possibly a few other stations would provide the primary water quality dataset. These data would need to be peer-reviewed, and the management variables for each watershed would have to be compiled and weighted. With a more limited geographic scope and the associated data on sediment sources, it might also be possible to come up with some additional watershed variables, such as a geologic sensitivity factor. Ideally sediment yields also could be calculated for each watershed for each year. The relationships between turbidity, sediment yields, management, and watershed characteristics could then be analysed on a year-by-year basis as well as in a multi-year analysis. Existing information, such as sediment source studies and watershed analyses, also would have to be compiled to explain—to the extent possible—why particular watersheds have unusually low or high turbidities. By incorporating additional years and watersheds within a narrower geographic area, this second approach should provide a much more insightful understanding of the relationship between forest management and turbidity. Perhaps more importantly, the compilation of existing sediment source and watershed information would provide much more reliable guidance as to the causes of high and low turbidities relative to management, and how management (and restoration) activities could be adjusted to improve water quality. This second approach might require around 7-10 months of time from scientists and statisticians, and should lead to a peer-reviewed journal article.

A complementary or alternative approach would be to conduct systematic field assessments of best management practices to assess their implementation and effectiveness. These could be done on specific watersheds that are identified as particularly problematic, or a more randomized approach to assess specific practices in proportion to their estimated importance for sediment production and delivery. These would provide a more direct link between management activities and water quality, and more direct guidance for minimizing the adverse effects of different management practices on water quality.

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