Lee MacDonald
27 March 2014

Informal Additional Comments on Peer Review Draft: Elk River Sediment TMDL

The following are my additional comments on the Peer Review Draft (PRD). These are drawn from my notes while reading the PRD, and they are being submitted in my personal capacity as a scientist. The sole intent is to help you formulate a stronger, more explicit, and more defendable technical TMDL for the Upper Elk River. There should be relatively little overlap with the comments I had submitted earlier, but I trust you will excuse any inadvertent redundancy. They are organized in the order that they occurred in the PRD, and grouped according to the structure of the PRD.

As always, you are most welcome to contact me if you have any questions, suggestions, or comments!

COMMENTS

PRD consistently uses units such as $yd^3/mi^2/yr$. Mathematically the use of two slashes would convert this to $yd^3\ yr/mi^2$. It would be much clearer and more accurate to use superscripts, so $yd^3/mi^2/yr$, for example, should be written as $yd^3\ mi^{-2}\ yr^{-1}$. This would be consistent with normal scientific convention and avoid any ambiguity or misinterpretations.

Summary

On p. xii the draft states that "Canopy intercepts rainfall and acts as storage for water through evapotranspiration"; presumably this water is lost primarily through evaporation rather than transpiration?

Turbidity is limited to 20% above background; how is this determined--on a continuous basis, an annual average, or ?? How is background determined? (maybe this does not need to be in the TMDL, but I had to wonder….)

Chapter 1

A Technical TMDL does not have an implementation or monitoring plan, but pursues alternative approaches to implement the actions. By "alternative approaches" does this mean the WDRs? This is not clear, so maybe you can be more specific about the possible approaches?

Chapter 2: Overview of the Upper Elk River Watershed

The mean annual precipitation map in Figure 2.1 is pretty crude, but maybe there is nothing better? HRC and GDRC should have more data to adjust this? Rainfall is a key driver of many of the underlying processes, so it would be useful to know how this varies across the Upper Elk River watershed.

This chapter refers to Patenaude's report, and references some Caltrans pictures 50 years apart that show a shift from native grasses to blackberry and willows (and trees?). I would think that the original, pre-European vegetation in this area was floodplain or low terrace redwood forest, so is it really appropriate to “restore” the vegetation to native grasses? Were the grasses due to people converting the forest to pasture for grazing, rather than “natural”?

RCAA 2003 not in refs, nor is Lewis 2011 (p. 2-5)

LiDAR data were used to develop a 1-m DEM, and this was used to determine percent area in different slope classes. While only 12% of the watershed is less than 15% slope (Table 2.2), slope should affect sediment delivery to the stream and many of the erosion processes (e.g., landslides). Can the sediment production estimates and sediment delivery be adjusted according to the detailed slope information available from the LiDAR data? The effect of slope on sediment production and delivery should at least be acknowledged.

Subsurface erosion due to soil pipes is stated to be prevalent in the Upper Elk River, at least in the Wildcat group (PWA, 2000; Buffleben, 2009). The PRD argues that collapse of pipes can lead to gullying, and eroded material can clog pipes and increase pore water pressures, and cites a 2007 paper by Fox *et al.* in *Earth Surface Processes and Landforms*. This paper seems to focus on seeps and bank erosion at Goodwin Creek in Mississippi, and notes that the seeps observed at Goodwin Creek are not similar to those in the literature. Hence it is not clear that this paper can be cited to support the arguments in the PRD for subsurface erosion by soil pipes.

The PRD makes reference to “a splash dam” (HBWAC, 2005) that was used prior to the railroad. Presumably there were multiple splash dams, not just one, as this was a common log transport technique in much of the western US. The use of splash dams would be expected to have a substantial impact on the stream channels, and this should be at least acknowledged. Similarly, logs may have been dragged down some of the smaller channels, and again this would have had a significant effect on the channels, which then may be affecting the current channel conditions, particularly bank erosion and streamside landslides. It would be helpful to have some assessment as to the likely magnitude of these practices on current channel conditions and erosion rates. Note also that HBWAC 2005 is not in the references.

“mycroryzal” on p. 2-14 should be correctly spelled.

Chapter 3: Problem Statement

What has happened to channel bed elevations since October 2003 (see Fig. 11 in Pastenaude’s 2004 report)? It would be quite helpful to know if there is any trend, and if so, in what direction (i.e., is there any “recovery”?).

PRD argues that blackberry is increasing roughness (p. 3-11), but will this shrub really stand up to high flows, or will it just lie down? And should the comparison with roughness be relative to the grass present in about 1958 according to the USGS, or the old growth redwood that presumably was there pre-European per my earlier comment?

Channel armoring as mentioned on p. 3-11 and 3-34 appears to be misunderstood, as armoring usually refers to the development of a coarse surface layer, either on hillslopes or in a stream channel. If a channel is composed of fines (sand, silt, or clays), particularly a channel the size of the lower Elk River, it is not clear how this can be defined as armored. The formation of an erosion-resistant, cohesive clay bed is a different process than classical armoring.

The cross-sectional changes (Figures 3.9-3.13) are all presented in absolute terms. For understanding and context these need to be presented as percent change, as a 60 ft^2 change may be trivial in a very large channel, but “significant” in a small channel.

If I look at Figures 3.9-3.13, I see a general but not universal trend of a reduction in channel cross-section, while other cross-sections show no trend. There also seems to be a difference in trend between ATM 162 and 214 (no trend for the latter). This begs some explanation.

Salmon Forever began monitoring cross-sections in 2002, and the PRD calculates a weighted average deposition of 0.5-0.75 ft of deposition on banks and floodplains, but this weighted average is greatly affected by a few high values (e.g., XS2 in Figure 3.15). I think the median value would be a much better indicator of overall trends as opposed to a mean, which can be greatly affected by a few high values (if Bill Gates moves to Arcata, then the average income would show that there is no more poverty!).

Fig. 3.19 shows the percent fines <0.85 mm as determined from shovel samples that were then sieved. Overall this figure shows a general downward trend for most of the ATM sites. Since bed material particle size is one of the most sensitive indicators for the effects of land use (Montgomery and MacDonald, 2002), this would seem to indicate that overall some recovery is taking place. Yet the text states “None of the stations demonstrated steady trends of improvement over the period of record.” One cannot expect “steady” trends, as it becomes harder and harder to get a decrease in the amount of percent fines, especially as one approaches the lower values, and this principle is supported by the greater decline in percent fines for the ATM sites with higher initial values. The TMDL should provide a more accurate characterization of the data.

It also would be useful to know whether there is any relationship between the change in particle size over time and stream gradient, location, precipitation, or some other factor?

There is much less of a trend for percent fines <6.35 mm (Fig 3.20); is this due the fact that this threshold is much closer to the “natural” bed material particle size?

The text notes that two of the channel bed deposits had >55% gravel (Fig. 3.22). Which two stations and why?

Similarly, Station 162 was the only one with an upward trend in D_{50} (Fig. 3.24); why?

Residual pool depths generally show a small increase, with a decrease in 2008-9 (Fig. 3.25). Again, does this suggest some recovery, and why should there be a decrease from 2008-9?

Overall, it seems that the extensive data on cross-sections, bed material particle sizes, and pool depths are trying to tell something of a story, as in some cases there seem to be clear trends and some recovery, while for other locations there is no trend. Can we understand more if the data are plotted and analysed longitudinally, or against stream gradient, or contributing area, or other potential controlling factors? Some simple geomorphic context and relatively simple additional analyses could greatly help clarify what these data actually mean, and if there is already a trend towards improving fish habitat. If this is the case, then it suggests that all of the efforts to reduce sediment from timber harvest and roads are having an effect. Any indication of recovery, or the spatial distribution of trends, then has important implications for how much more effort is needed to further reduce sediment sources from management activities.

The justification for deep pools in the PRD is to provide cool water and cover, but my understanding is that the water temperature data does not indicate that water temperature is a problem. And wouldn't the amount of coarse woody debris (CWD) be an important control on the amount of cover (as well as the complexity of habitat)? The PRD currently provides very little information on CWD loading (my understanding from the literature is that there is a relatively direct positive relationship between the amount of CWD and fish populations).

Are SEV values in Figs. 3.26 to 3.30 only for winter, or entire year? This should be explicit. If the SEV values are only based on October to May (8 months), then it should be noted that the annual values would be 33% lower than the values shown (assuming that there are no significant events that increase turbidity from June to September).

With respect to the SEV values in Figures 3.26 to 3.30, Station 522 on Corrigan Creek has high harvest rates, yet the SEV values are lower than most other sites (Fig. 3-28). The SEV values also were higher in HY 2005. Taken together, this would suggest that SEV values vary more with rainfall than with management, as there were few SEV>4 values for HY 2004 and HY 2005. The TMDL should provide some acknowledgement and explanation as to why there is so much difference between SEV values for the same watersheds between different years; is this a harvest effect, landslides, precipitation, or some other factor? I realize that the TMDL is not a research paper, but a more process-based analysis should provide more useful insights rather than trying to simply generalize across all sites.

I also should note that the recent rainfall record at Eureka is a problem due to the growing effect of the nearby trees per my earlier written comments.

CHAPTER 4: SEDIMENT SOURCES

Typos on p. 4-3, 4-6.

The second of the three approaches is characterized as an empirical sediment budget to assess sediment production of specific land classes. But sediment storage is typically the largest piece in a sediment budget, so a sediment budget has to include all three components: 1) inputs, 2) outputs, and 3) changes in storage. The storage part is not addressed here, so the "empirical sediment budget approach" should be relabeled as an assessment of inputs rather than a sediment budget.

The sub-basin approach uses the South Branch of the North Fork of the Elk River and Corrigan Creek as the prototypes for managed watersheds, and the Little South Fork of the Elk River as pristine background. Ideally more information could be provided to indicate the extent to which these three basins are representative and sufficient for generalization to the entire watershed.

The last paragraph on the bottom of page 4-5 talks about "modeling watershed sediment production", but the empirical approach stratifies a watershed into land classes and develops sediment production estimates using empirical coefficients. In my view this is not really a "model", but a classification, characterization, and extrapolation.

On p. 4-6 the PRD claims that timber harvesting and skid trails lead to groundwater interception, soil compaction, and drainage diversion. The effects of timber harvest and skid trails should be discussed separately, as there are probably order of magnitude differences in infiltration rates and the potential for intercepting subsurface flow between a skid trail and a harvested area that was not subjected to repeated (or any) disturbance by tracked or wheeled skidders.

From a process-based perspective, one would expect that soil creep rates, bank erosion, and streamside landslide volumes should vary with stream size, stream type, and the steepness of the adjacent hillslopes. Have any of the data from the various studies been analysed to look at these relationships? It seems a bit simplistic to apply the rate measured from certain inventories to all streams. Soil creep in particular, and hence bank erosion, is not likely to be uniform in space as currently assumed (see p. 4-9).

It also is not clear to what extent the values of bank erosion and streamside landslides might vary according to the amount of precipitation and high flows that occurred prior to the surveys. I recognize that the surveys did try to estimate the approximate age of the features being measured, but there did not seem to be any attempt to normalize these estimates according to precipitation or high flows. One cannot expect a nice clean relationship between precipitation (or maximum peak flows) and the amount of bank erosion and streamside landslides, but bank erosion and streamside landslide rates can vary by orders of magnitude amongst years, and it would be helpful to know that the relative magnitudes of high-intensity rainfall events or peak flows during the different monitoring periods. The TMDL also should recognize that the rates being measured may not represent the long tails of the expected lognormal distribution, so the average is likely to be an underestimate of the long-term mean, particularly in the unmanaged areas.

Table 4.7 presents sediment loading from management-related headward incision over time. My understanding from the text was that 75% of this loading was assumed to have occurred from the initial logging up to the 1950s, and the remaining incision was to be distributed equally over time ("5% of the current total per decade", p. 4-25). However, in Table 4.7 the rates for Wildcat and related geologies vary from 13 to 74 yd^3/mi-yr, so I don't understand how these values were determined.

I also had trouble reproducing the calculations to yield the values listed in Table 4.8 for management-related streamside loading associated with stream bank erosion. The use of four significant figures is unwarranted given the accuracy of the underlying data and the assumptions used to calculate these values.

A major and inherent problem in the sediment source analysis is that the sediment sources vary tremendously over time due to the variations in rainfall, the changes in management practices, and also the fracturing of rock due to major earthquakes (Molnar et al., 2007). On p. 4-28 the PRD recognizes the potential effect of changing management practices on management-related shallow hillslope landslides by stating "the management-related shallow landslide analysis may not accurately reflect current management strategies". I appreciate this recognition and would strongly agree with this statement. But if this is the case, can the historic rates of shallow landslides—which generally occurred under a very different set of management practices—be used to justify the numeric targets and associated restrictions on timber harvest? Would not the same caveat apply to streamslide landslides?

Tables 4.9 shows a 100-fold variation in road-related landslides over time, and Table 4.10 shows a nearly 35-fold variation in management-related open-slope shallow landslides over the different time periods. These large variations beg for some more explanation in terms of how much of this variation is due to differences in storms, fracturing due to earthquakes, and changes in management practices. These different factors cannot be completely disentangled, but if there is already a "significant" reduction in sediment production due to changes in management, how much more can changes in management contribute to achieving water quality standards, and how much do we just have to wait for "natural" recovery to take place?

The PRD discusses the use of LiDAR data and two distributed physically-based models (SHALSTAB and a probabilistic model by Haneberg called PISA) to identify relative hazard in different geologic terranes, but the PRD it doesn't report any results or indicate if this approach can be beneficial. Certainly the use of a more physically-based model to identify high-risk areas would be highly beneficial, and would be a much more justifiable approach to limiting timber harvest in certain areas rather than applying a blanket restriction on timber harvest rates as currently proposed (Chapter 6).

In the section on management-related streamside landslides, large slides were >5 yd^3 and directly measured while small slides were <5 yd^3 and just counted. Table 4.12 indicates that the assumed volume for the small slides was 5 yd^3. Since this is the upper bound of the range of volumes for small slides, this would lead to an overestimate of the mean volume for the small streamside landslides. The midpoint volume should be used instead (i.e., if the minimum volume recorded was 1 yd^3, then the midpoint volume is 3 yd^3 and this should be used to estimate the total volume from small slides).

To estimate management-related streamside landslides the Regional Board combined the advanced second growth and recently harvested areas "because the spatial age distribution of riparian stands is unknown" (p. 4-37). The data in the PRD show that there is more than a four-fold difference in streamside landslide rates between advanced second growth (1126 yd^3/mi^2) and recently harvested areas (4926 yd^3/mi^2), and there also is a difference in the timing with 60% of the large slides in advanced second growth occurring in 1988-97 and only 20% in 1998-2003, while in recently harvested areas 38% of the large landslides occurred in 1998-2003. Combining these data will result in an overestimate of the loading from streamside landslides if more of the area is in advanced second growth, and underestimate streamside landslides if more of the area is in recently harvested areas. Given that streamside landslides are one of the largest sediment sources, more effort should be devoted to determining riparian age from aerial photos. Alternatively, a distribution of riparian ages could be assumed based on the timing of a given harvest relative to the the timing of the regulations limiting streamside harvest and requiring streamside buffers.

The process for estimating streamside landslide rates also was difficult to follow. I could not determine how the value of 988 yd^3/mi for managed streamside landslides in 1975-2003 was obtained, or how the streamside landslides in managed Class III streams were (or were not) accounted for. A clearer explanation is needed, and a more complete presentation of the underlying data would be helpful.

There are a number of points with respect to the management-related discharge sites that can be made that would affect the results.

1) For HRC lands the sediment from management-related discharge sites was equally distributed over the subsequent decades if no date was available. This may be a necessary assumption, but I would think that the discharge sites with no date would tend to be older, and distributing these equally would tend to disproportionally increase the more recent values.
2) The observation that there were NO additional sites from GDRC for 2007-2009 (p. 4-44) strongly indicates that improved management is having a big effect, and further suggests that equally allocating sites over time will disproportionately increase the recent values.
3) The PRD assumed that disturbance and sediment delivery in Lower Elk River and Upper South Fork was uniformly distributed over the 1950s to 2000s; while this may not have a big effect on the final values, this assumption should be clarified (which lands does it apply to?), justified, and quantified (how much effect does this have on the final values?).
4) Table 4.17 calculates the average discharge volumes from different studies as 36 yd^3, but this mean is heavily dependent on the value of 113 yd^3 for Bridge Creek. This is five times the mean of the values for the seven other studies. Hence there is a need to explain if the discharge volumes from Bridge Creek are "typical" or exceptional given that they presumably were associated with the railroad as opposed to roads and skid trails? Excluding the Bridge Creek data would reduce the mean value to 23 yd^3 or by 36%.
5) The per site value of 9.1 yd3 resulted in twice as much estimated sediment discharge than values calculated using the 1.1% fill volume (p. 4-50). The value of 9.1 yd^3 was used to ensure a margin of safety, but the biggest and worst sites were probably treated first, so it would more sense to use the percent of fill volume rather than the fixed volume of 9.1 yd^3.

Similarly, there are a number of points with respect to ground-based yarding sites that can be made that would affect the results.

1) Table 4.24 is not completely clear.
2) The calculated skid trail volumes assumed that the HRC CAO inventory missed 70% of the sites, and the assumed future sediment delivery was based on average of Elk Springs (76 yd^3) and Palco Freshwater Creek study (64 and 78 yd^3 for each of the two units). However, wouldn't the CAO inventory be more likely to identify the biggest sites, so any extrapolation using these values would presumably tend to overestimate the total volumes?
3) Sediment delivery from skid trails was assumed to occur at similar rates over time as all of the other management-related discharge sites. Hence there is relatively little improvement over time even though management practices have generally improved. Shouldn't there be some more recent data than 2001-2003?
4) The PRD draft assumed uniform future delivery over the next 50 years (p. 4-58); I would think that failure rates would tend to decrease over time as the sites most likely to fail will fail sooner. The reasonableness of this assumption should be clarified by tying it back to the underlying causal process(es).

The estimated in annual sediment loading from road surface erosion shows relatively little improvement over time (Table 4.18), yet there has been very extensive road stormproofing efforts. There also are no data after 2003, but given all of the recent efforts to reduce road surface erosion this table should be updated to better reflect current practices. It also should directly address likely road sediment **delivery** rates rather than just road sediment production.

Issues related to management-related Harvest Surface Erosion include:

1) Is burning happening on harvest units? (p. 4-64).
2) The sediment delivery rate for partial cut units is 63% of the value for clearcut units; this seems relatively high given the likely differences in percent ground cover and percent area disturbed.
3) The pattern of ECA values by subbasins in Figure 4.19 seems quite different than the pattern of sediment loading from harvest-related surface erosion in Figure 4.20. I would have expected more similarity, and it would be helpful to have some explanation of why these two figures are so different.
4) The list of management actions to control loading from harvest surface erosion should begin with the most important control, which is the amount of surface cover. If surface cover is at least 65%, surface erosion is generally very low. The next most important action is to minimize compaction. If these two actions are taken, there is little justification or need for the other actions. The suggested use of compost tea and mycelium (#7) seem particularly out of place and unrealistic.

Conceptually, should creep be included in bank erosion and streamside landslides, or should it be separate? This has important implications for the relative amount of background vs. management-related sediment sources.

Chapter 5. Sediment Loading Capacity and Load Allocations

Page 5-1 claims that the “load allocation also includes consideration of instream stored sediment from past land use activities because it has significantly altered the assimilative capacity”, but the TMDL does not fully quantify these stored sources, either legacy or current.

Page 5-2 claims that the amount of fine sediment stored in the channel would be rapidly reduced following large storm events back to levels favorable for spawning and rearing, but this is unlikely to be the case for all of the downstream reaches for several reasons. First, there is likely to some continuing delivery of stored sediment from further upstream. Second, some of the downstream areas are relatively low gradient, so it is not clear that these areas would have a channel bed that is anything other than sand, silt, or clay per my previous remarks.

On page 5-4 the PRD explicitly acknowledges that the “time lags between sediment input and discharge may be several years to decades or more”. I would agree with the subsequent statement on page 5-6 that activities can result in an immediate and measurable turbidity response, especially in fine-grained systems like the Upper Elk River. However, even the short-term response in turbidity and suspended sediment concentrations can be affected by sediment storage; most studies of short-term sediment responses show the phenomenon of sediment exhaustion, where the first high flow causes a very high response in turbidity and suspended sediment concentration, and each subsequent high flow from a rapid succession of storms (or daily snowmelt peaks) show a progressive decline in turbidity and suspended sediment concentrations as the most readily-available supply of fine sediment is depleted. . The rapid response in turbidity also does not mean that there is no storage of the sediment being supplied to the stream, and the amount of stored sediment can greatly affect the duration of a management effect and the time needed to recover from past actions.

I’m not completely clear on how the estimated storage relates to the proposed 97% reduction in management-related upslope sources. Page 5.10 notes that the instream deposits could be cleaned up in a 10-year period (5 years for planning and permitting, and 5 years for implementation; my calculations are for 5.6 years). I suspect that it might take longer to remove the stored sediment, depending on how much sediment is stored in the headwater channels, how much is in long-term vs. short-term storage, and what kind of flow events occur. My question is what happens to the load allocation once the stored sediment is removed; does this go up? Or if a longer time period is needed to remove the stored sediment, then this presumably would be a smaller mass (or volume per year); would this alter the load allocation? There effectively seems to be a trade-off between legacy effects (stored sediment and legacy hillslope sediment sources) and current load allocation. Who decides how long it should take for the legacy effects to largely disappear, and for the Upper Elk River watershed to recover? I also might expect that, once the sediment supply is reduced, that there would be a progressive decrease in the rate at which the stored sediment is exported from the stream network, and some of the stored sediment will become long-term storage.

Chapter 6. Numeric Targets

In my presentation I addressed the presumed hydrologic changes at the watershed scale (pages 6-2 to 6-3 in the PRD). With respect to soil piping, this is an interesting and important process, but I question the extent to which changes in piping can be rigorously linked to current management practices and the associated creation of in-channel sediment sources and downstream sediment delivery. With respect to the second change (sediment deposition in the low gradient reaches), this is clearly an important problem. I do, however, question the wording of the second sentence ("The hydrologic connection between the channel and its floodplain is out of balance, and the ecosystem is not functional to ensure expected flows, in combination with sediment loads, are able to be transported as they move downstream." The third hydrologic modification as written is questionable, as the forest harvest literature generally shows baseflows to **increase** following timber harvest. This also conflicts with some of the statements made in Appendix 6A.

On page 6-3 the PRD states that "The watershed hydrology objective would provide clarity to the connection between watershed hydrology, sediment loading, beneficial use protection, and prevention of nuisance." I don't see how the three watershed objectives will do this.

As I understand it, Appendix 6A provides the background to justify the watershed hydrology changes and objective. The problem is that the material in this Appendix is not up to date (e.g., the most recent citation on the hydrologic effects of forest management is Jones and Grant, 1996). This appendix also focuses on major modifications, such as urbanization, channelization, and surface water diversions, that are not relevant to the Upper Elk River. In general, the literature strongly shows that the effects of current forest management practices on watershed hydrology and beneficial uses is far smaller than the effects of urbanization, channelization, and other direct modifications. Hence most of the material in Appendix 6A should be deleted, and a more up-to-date and relevant discussion of the effects of forest management is needed to support the three changes listed on pages 6-2 to 6-3.

Table 6.1 sets out the numeric hillslope targets, and the following comments are directed towards several of these that I did not address in my earlier comments.

1) The first target is for zero headward incision and zero increase in existing drainage network. Ideally the channel network will be decreasing in length as some of the management-induced channel heads move downslope, but the one major caveat is that the locations of the channel heads may shift upstream in response to the exceptionally high-intensity rainfall events, regardless of management. The text suggests promotion of stabilizing vegetation in areas of subsurface flow paths, but it is not clear how the surface vegetation can control the subsurface flow paths. In my view, the most important factors for minimizing the formation of channel heads are to minimize compaction, as this will minimize the occurrence of infiltration-excess (Horton) overland flow, and to maintain at least 60-65% surface cover (litter, wood, live vegetation) to prevent rainsplash and soil sealing.
2) The second category is bank erosion and streamside landslides, and the first target is "decreasing trend in length of unstable channel". The problem with this target is that certain stream types inherently have unstable banks (e.g., pool-riffle channels), and it is

difficult to both measure channel instability, and to decide what is "natural" and what is "excessive" channel instability for each stream type and geology. The amount of unstable channels also will vary with the occurrence or absence of large storm events, as well as other factors such as the amount and movement and amount of large wood or debris dams.

3) For riparian areas, the PRD refers to the FEMAT report, which recommended two potential tree heights for Class I and II watercourses, and one tree height for Class III watercourses. For the Upper Elk River tree height is defined as 170-200 feet. The problem is that Figure 6.2 shows that virtually all of the riparian forest effects on streams occur within about 0.8 tree heights. The PRD argues for wider buffers to reduce blow-down and ensure appropriate age distribution in the riparian area, and that wider buffers also may be needed "to ensure sediment filtration within undisturbed land to preclude chemicals and nutrients from being introduced to the stream as a result of upslope management activities." The PRD also states "the maintenance of microclimate, especially under climate change, is crucial to ensure a sustaining environment." (p. 6-15 to 6-16). While I cannot speak to the argument regarding blowdown, the literature and Figure 6.2 show that nearly all of the sediment filtration will occur in less than 50 m, and larger buffers will have a negligible effect on sediment loading relative to the other sources addressed in the sediment source analysis. Chemical filtration is an issue primarily in agricultural areas where there is considerable application of fertilizers, pesticides, and herbicides. Water temperature is not an issue, and shade is maximized within one tree height (Figure 6.2). The microclimate issue also is not convincing given the relatively high levels of fog and precipitation compared to more interior locations where there is a sharper contrast between the riparian environment and the much drier hillslopes. If the PRD is going to advocate riparian buffers greater than one tree height, a more process-based and convincing argument must be made relative to the beneficial uses at risk.

4) The section on cumulative watershed effects summarizes the findings of the Independent Scientific Review Panel (ISRP), noting that "neither its analysis nor any other analysis could predict with certainty what combination of measures and logging rate restrictions would <u>ensure</u> the protection of water quality and recovery of impaired watersheds." (p. 6-16). Hence the ISRP recommended an adaptive management approach. Quoting from the PRD: "The best that could be done is to postulate a plan based on the best available information; test the plan using a combination of compliance, effectiveness, and trend monitoring; and revise the plan in a timely and appropriate manner based on the monitoring results." (p. 6-17). The current PRD puts very little emphasis on the adaptive management approach despite this recommendation of the ISRP. The adaptive management approach also would seem to be an allowable approach to implementing the TMDL (see p. 7-7).

The section on cumulative effects also states "the density of the road network and the intensity of use are closely related to harvest rate" (p. 6-17). I would agree that the intensity of use by heavy logging trucks is closely linked to harvest rate. However, harvest rate is probably having virtually no effect on the density of the road network in the Upper Elk River, as this is already fully developed. HRC and GDRC can provide specific data, but I would be very surprised if there is any new road construction other than to replace existing problematic roads. Hence it is not correct to state that road density is "closely linked" to harvest rate, and the rate of future harvest also is unlikely to have much effect on road density. In other areas where I've worked timber harvest is actually beneficial to reducing road sediment production and delivery, as the harvest is necessary to generate the funding that can then be used to relocate and fix roads. The lack of timber harvest on USFS lands means that similar funds are generally not available, so there is a huge problem of legacy roads that are adversely affecting water quality and only very limited funding to fix these roads.

Figure 6.5 (stairstep approach) puts forth very specific relationships between percent attainment of load allocations and annual timber harvest rate. The correlation between ECA and 10-15 year harvest rate in Klein et al. (2011) is a correlation, not necessarily a cause-and-effect. Over 10 years a 0.4% harvest rate is only 4% of a watershed as compared to 15% at the highest stairstep. The literature does not show that harvesting 4-15% of a watershed over a 10-year period, particularly in the absence of new road construction and given current practices, would result in any detectable difference in peak flows, erosion rates, or sediment yields, or that there would be a detectable difference between a 0.4%/yr and a 1.5%/yr harvest rate between these two harvest rates. Hence the presumed relationships shown in Figure 6.5 are not valid given the very low harvest rates on the y-axis.

Figure 6.5 is further flawed because harvest rate is to be calculated using proportion of canopy removed (p. 6-19). Per my earlier comments, this only deals with one of the several erosion processes that contribute sediment to the Elk River.

Chapter 7: Implementation Framework

On pages 7-6 to 7-7 the PRD notes that existing pollution control programs can be certified by the RWB as sufficient to implement the TMDL if it can find that: "1) The implementing program is consistent with the assumptions and requirements of the TMDL; 2) Sufficient mechanisms exist to provide reasonable assurances that the program will address the impairment in a reasonable period of time; and 3) Sufficient mechanisms exist to ensure that the program will be enforced, or ….further regulatory action would be unnecessary and redundant." There is no further discussion as to whether these three criteria are being met, or whether this approach would be suitable for the Upper Elk River TMDL. An explanation of the suitability or limitations of this approach is needed in order to more clearly justify the approach that will be taken to achieve the goals of the sediment TMDL for the Upper Elk River.

References

Molnar, P., R.S. Anderson, and S.P. Anderson, 2007. Tectonics, fracturing of rock, and erosion. Journal of Geophysical Research 112, F03014, doi: 10.1029/2005 JF000433, 12 pp.

Montgomery, D.A, and L.H. MacDonald, 2002. Diagnostic approach to stream channel assessment and monitoring. *Journal of the American Water Resources Association* 38(1):1-16.