Comments by Rowan Baker for M. Scurlock, Oregon Stream Protection Coalition
ODF Technical Report #21 (RipStream Large Woody Debris Assessment)
July 15, 2020

NOTE: Technical Report #21 by Adam Coble and Marganne Allen dated Jun 9, 2020 (43 pages) includes an analysis and discussion of pre- and post-harvest riparian stand conditions, understory vegetation, downed wood, and large wood in "small and medium Type F" fishbearing streams on non-federal lands in Western Oregon managed or covered by current Oregon Department of Forestry (ODF) riparian protection rules.

I. Responses to ODF Questions for Reviewers

· *Does the abstract adequately summarize the major findings of the report?*

There is no abstract. The executive summary and introduction do an adequate job of describing the main findings while admitting some (not all) of the study's limitations. The authors acknowledge (page 4, lines 150-155) that "the RipStream analysis is limited in addressing questions related to long-term processes such as large wood recruitment to streams and forest successional pathways. Also, disturbance processes such as landslides, debris torrents, or beaver dams are not included and are out of scope for this analysis." These are important caveats that strongly condition many of the study's primary findings, analyses and discussion points. More specific comments to follow will attempt to identify these and other major and minor problems with the study.

· *Does the introduction provide enough contextual information to understand the research questions and subsequent analysis*?

Context could be improved with respect to explaining the methods of site selection, decision to use a relatively small number of fully independent sampling sites, unknown number of landowners/operators and lack of specificity of Riparian DFCs. The rationale behind the initial study design was strongly influenced by RipStream temperature sampling as opposed to LWD metrics, and explaining why and how this came about would help the reader better understand other, unstated limitations of the data and analysis. One thing that would further understanding in the introduction would be a brief statement of what the next steps will be, based on this analysis. Will the study be repeated or expanded, to include more sites and landowners at intervals longer than 5-7 years post-harvest ? How can ODF determine broader scale or longer-term compliance or effectiveness toward meeting DFCs? How will these findings or any future findings influence decisions regarding sufficiency of the rules for improving LWD?

· *Are the methods thoroughly described to understand how the data were collected and analyzed?*

Yes and No. Site selection and assumed potential DFC achievement at the midpoint of the rotation are not accurately or completely described or interpreted. On the other hand, the authors show commendable diligence in, e.g., illustrating the layout of sites with upstream/downstream

"controls" (not true controls) and within-site transects and sub-plots (not true sample units) and the field methods employed for site surveys. The authors are careful to note problems with "psuedoreplication" or non-independent samples within site transects or plots, which is a valid concern that is not clearly explained away by the "mixed effects model" approach. More study sites and (arguably) fewer within-site plots would have benefited the data collection for the purposes of understanding both landowner behavior and the potential for LWD recruitment. True controls do not exist, as the authors themselves acknowledge.

· *Do the figures and tables add to the paper? Do they aid understanding or are they superfluous?*

Yes, in fact all are highly informative, but some are misinterpreted or de-emphasized. Figure 2, on page 6, shows that DFC, which should probably be defined somewhere with regard to LWD as the maximum achievable LWD recruitment potential under the rules, does not occur at mid-rotation but approaching and just prior to harvest. The report needs to better explain or incorporate these and other model results, as the field study component was too short. The authors could delete the statement or amend the interpretation that DFC or even "average mature conditions" will be achieved at mid-rotation. As the rules allow RMA basal area to be reduced at the start of each rotation, i.e., down to "standard targets," this will likely significantly (and repeatedly) reduce natural mortality of mature and semi-mature trees. This management approach may result in some level of mature forest conditions in RMAs, i.e. DFC, some of the time, but it is hard to imagine how it can result in recruitment of sufficiently large "key pieces" of LWD, or increasingly suitable amounts or accumulations of LWD, to streams.

Note: Some larger sized (but not truly large) LWD may recruit episodically or fleetingly every 50 years or so, pre-harvest, or perhaps post-harvest assuming larger trees comprising "standard target" basal area are allowed to die rather than be harvested in subsequent rotations (or where larger snags are left). And although landowners were observed in this study to leave some large trees in RMAs to meet standard targets, we do not know what average landowner behavior will be in 50-100 years. Repeated entry into RMAs over many successive rotations could liquidate these larger trees. "Gaming the DFC" in this manner by rotating the largest trees out to revert to standard basal area targets would clearly not increase key piece LWD in streams in 50, 100 or even 200 years because natural mortality and entrainment of these larger trees would be reduced or eliminated.

· *Are there certain aspects of the discussion that could be communicated better?*

The discussion section includes mention of a literature review to be conducted. Normally a literature review is completed before a study is designed or research initiated so that needless or repetitive studies and unproductive study design approaches may be avoided. The paper should include a literature survey that summarizes other LWD recruitment studies. Key questions that could be answered by a literature review include: Are there better answers to the questions posed, or even better questions answered by other studies? How do the results of this study compare to those results? This is all standard discussion section material that is missing here. The literature review should also include LWD recruitment model, for example those done in Washington State under a similar though not identical riparian rules and baseline conditions.

· *Are the conclusions consistent with the evidence and arguments presented and do they address the main question posed?*

It is not clear what the "main" question is. The set of three questions included in the report on page 4 (lines 144-147) are not all that helpful or very well-defined. Only the third question pertains to LWD. It states:

> "Are the riparian rules and strategies effective in maintaining large wood recruitment to streams, and downed wood in riparian areas?"

The question is essentially an effectiveness monitoring question that assumes reasonably faithful implementation of the rules and strategies. There is no way to answer this question using this study for this purpose, even if the assumption of faithful implementation is met, for four basic reasons: 1. No defined target. 2. it takes too long, 3. we don't know how or whether the sample frame of the study is representative, and 4. all relevant sources and mechanisms for LWD recruitment needed to "maintain" a desirable or even a reasonably achievable level are not included or considered by this study. Furthermore, it should be noted that the word "streams" in the question is general, and this study did not encompass all stream types but was limited to only two classes of streams. For that matter, it also does not include all riparian areas.

The study does not give us answers to some *more important* questions such as: what RMA delineations or site and watershed-wide riparian protection rules would result in a very high probability of significant instream increases in LWD (above current baselines, in all in ecologically significant size and length classes, over time)? Such a set of rules could include compliance monitoring, and if relying on basal area targets would probably impose a penalty for "gaming" the DFC in future by swapping out larger trees.

· *Do you feel that anything is missing from the report that should be included?*

See below under "Specific Comments" where I make a number of more specific recommendations. However, in sum, the recommendations point to a flawed design that can probably not be "fixed" via minor or major additions to the text of the report.

The above questions for reviewers would suggest that the assumptions, premises, framing and design of the study are acceptable, and any serious study limitations can be easily overlooked. The following comments focus on both the general and specific problems with the study design in terms of capturing actual or potential LWD recruitment to streams. Later sections provide even more specific (page and line) comments.

The study lacks true controls. There are no riparian stands on private lands that are in reasonably mature or recovered condition to deliver adequately sized LWD to streams. Moreover, what is needed are not small pieces of LWD that may increase temporarily from post-harvest slash or partial remains, but persistent, functional pieces and stable accumulations of LWD provided over time. As the paper states (page 3, lines 84– 87) all or most existing downed wood in the current baseline is small, and/or already too decayed, and most instream LWD is undersized:

> "The number of downed wood pieces and total downed wood volume inside and outside of the FPA RMAs was dominated by more-decayed logs (e.g., decay classes 3, 4, and 5). In streams, large wood was mostly comprised of small diameter (5-18 in), shorter (5-20 ft) pieces."

All pre- and post-harvest comparisons are between recently treated and past-treated areas, not treated/untreated. The process of selection of site locations and the number of individual (independent) landowners sampled is not described. If this information exists in the other reports on RipStream it should be included early on in the Methods section.

Sites in the study are the only potentially valid sampling units for the purposes of LWD analysis. Upstream and downstream "controls" for temperature analysis are not valid controls for LWD analysis. Not only are they not independent, they also are not inclusive of the full range of sources of LWD. Adjacent and even non-adjacent upslope areas including those outside the RMAs are potentially highly relevant given known landslide and debris flow mechanisms of LWD delivery.

The study design was apparently largely influenced by the thermal focus of RipStream, where the goal was to detect a relatively immediate temperature response to measurable streamside shade reduction allowed under the rules. The study design is not reliable or suitable for detecting long term changes in LWD.

Even if the selection criteria for the 18 sites were randomized and unbiased, there is still a question of whether landowner behavior at those sites is representative of more generalized landowner responses. This means that any observed or projected changes could be a result of site or landowner selection. One wonders if any of the sites sampled are on the same small or mid-sized fish streams or were harvested by the same landowners or timber operators. If so it should be stated early on in the Methods section.

The issue raised in the paper of "pseudoreplication" (e.g. page 10, lines 349-357) is a valid concern, especially for the within-site metrics but also could be a concern for the sites themselves. Two fixes for this problem would be to use a larger number of fully independent sites, if available, throughout Western Oregon, and to thoroughly describe how the sites were selected. From the results it appears that landowners that were a part of this study generally did follow the rules for distance and basal area retention: however, the reader wonders if willingness to follow the riparian harvest rules was part of the site or landowner selection criteria, and whether this level of willingness would be similar across Western Oregon. More sites and clearer description of selection criteria would help to answer this question.

The oversampling of within-site metrics (resulting in pseudoreplication) and relatively small number of independent sites hampers the interpretation of results and also limits understanding of overall landowner behavior. Thus, effectiveness monitoring and compliance monitoring applications of the study are limited. The study might be improved to meet the needs of compliance monitoring if there were a higher number of fully independent sites and if the report could give additional reasoning as to why the selected landowners are fully representative of all

private landowners. Effective compliance monitoring would randomly select landowners and sites and would try to represent the behavior of as many landowners as possible using as many different sites as possible.

The study has an all too limited timeframe for treatment/effects analysis. Even the past 26 years post rule development is too short, as acknowledged in the introduction, to determine longer term LWD responses.

The study fails to meet the basic tenets of well-designed effectiveness monitoring, largely because of inadequate or no controls, and the abbreviated study window relative to the long term and/or lagged recruitment of LWD in streams, not to mention the need for persistence of instream LWD over time.

An inadequate and incomplete picture of LWD contribution areas within watersheds is evident in the framing and basic premises of the study design. The design only looks at small and medium fish streams and the RMAs adjacent to those streams. A significant portion of instream LWD may originate in or along more numerous smaller type N streams which receive no protection, and from upslope areas (harvested areas outside of RMAs). A solution to this problem is to model LWD recruitment from all source areas over time. A whole watershed or landscape scale modeling effort is needed that accounts for all actual and potential sources of LWD. The modeling effort should use reasonable assumptions of tree mortality across multiple successive timber harvest rotations.

The implications of Figure 2 should be made more evident in the text of the report. The paper suggests that the DFC for riparian areas would occur sometime mid-rotation. Figure 2 actually appears to show that the maximum potential mature tree mortality that could deliver larger, functionally persistent or "key" LWD pieces to fish bearing streams from the adjacent RMA would be right before or near the end of the 50-yr. rotation.

II. Specific Comments Regarding the Desired Future Condition for Large Woody Debris

The overarching premise appears to be that following the riparian harvest restrictions on fish streams in the ODF rules will achieve "Desired Future Conditions" or DFC, over time. The report contains only general information to inform the reader as to what DFC actually is. DFC as defined should probably not be conflated with instream LWD or even potential instream LWD. This takes some unpacking.

For example, page 4 of the report (lines 113-1117) states:

> "FPA rule OAR 629-642-000(2) for the Desired Future Condition (DFC): (2) "The desired future condition for streamside areas along fish use streams is to grow and retain vegetation so that, over time, *average conditions across the landscape become similar to those of mature streamside stands.*" Conifer-dominated and mixed-conifer-hardwood stands appear to be at a good starting point for achieving mature conditions as described in the FPA. However, given the short time span of this study,

additional field work and analysis such as modeling stand growth and large wood recruitment will be appropriate for testing FPA assumptions related to long-term changes in riparian stand conditions and large wood recruitment" [italics added]

Comment: Neither "average conditions" or "landscape" is particularly well defined. These phrases cannot result in meaningful targets for effectiveness monitoring unless further specified. Agree with the statement that modeling will be needed. This study is not the full landscape. The landscape or whole watershed view of LWD recruitment has already been sacrificed.

Similarly, page 4, continued (lines 126-133):

> "The Forest Practices Act (FPA) water protection rules 1 on vegetation retention were designed to produce desired future conditions (DFC) for riparian stands along streams in Oregon. Crafted in 1994, the goal of DFC of riparian stands along fish use streams is to grow and retain vegetation so that, over time, a*verage conditions across the landscape become similar to mature streamside stands.* In the FPA, mature stands are characterized as often being dominated by conifer trees, 80-200 years of age that provide ample shade over the stream channel, an abundance of large wood in the channel, root masses along edge of channel, snags, and regular inputs of nutrients through litter fall. [italics added]

Comments: Are the average conditions to be maintained once achieved? And what does "average" mean in this context? Are there specified or measurable targets or ranges for LWD? The final report should state clearly that there are no defined targets for LWD, so should not imply any target or level of "achievement" is being evaluated unless one is proposed or developed. Agree that modeling should be done as stated in the report. I also looked at the separate monitoring questionnaire provided, which states:

> "The highest priority metrics include metrics related to stand growth, mortality, regeneration, and recruitment of large wood in streams. The goal of the modeling is to be able to characterize stand conditions 80-200 years under FPA rules, and how changes in stand conditions over time influence the contribution of large wood to streams."

Comment: Yes, please conduct modeling along these lines. Make sure to also evaluate and model potential upstream and upslope sources of LWD.

Page 5, Lines 150-155:

> "We recognize that the RipStream analysis is limited in addressing questions related to long-term processes such as large wood recruitment to streams and forest successional pathways. Also, disturbance processes such as landslides, debris torrents, or beaver dams are not included and are out of scope for this analysis. Wind throw that occurred during the study will be described as necessary to provide full transparency, as well as understanding potential sources of large wood recruitment to streams."

Comment: It is helpful of the authors to acknowledge the limitations of RipStream with regard to LWD. I note that this statement refers to other long-term processes that will likely be affected that are not the focus of this study. I would add stream channel integrity, hydrology, soil disturbance, sediment, nutrient regimes, and other processes that may be even more important and that are likely affected by or under the rules. The scope of the RipStream and hence this study is quite narrow. This is a serious limitation; perhaps the statement should be broadened or a footnote added to include these other processes.

Page 5, Lines 170-172:

> "An underlying assumption of these prescriptions is that managing riparian forests consistent with the prescriptive rules will result in the outcomes described above (e.g., shade and large wood)."

Comment: The outcomes mentioned are not specified in terms of large wood. The e.g. portion of this statement elides or conflates LWD recruitment with RMA basal area management. They are not the same thing.

Page 10, lines 349 – 358:

Comment: I agree with the concern and level of caution for pseudoreplication, but need to add more information on the "mixed effects model" and how it reduces or eliminates this concern. Modeling can inform the questions but it cannot completely eliminate problems with this study design.

Page 9, Lines 313-314:

> "Aquatic large wood measurements occurred at all sites pre-harvest, post-harvest year 1, and post-harvest year 3 within the control, treatment, and downstream reaches (described above)."

Comment: The three reaches (control, treatment, down) are of course not fully independent, as noted in general comments above. Because both upstream and downstream reaches are similarly degraded by past logging, the study design inherently shifts the baseline. There is no valid comparison with natural (i.e., potential) LWD recruitment in a formerly unlogged reach in a similar ecological setting; no untreated (historically not harvested) controls. Concern for pseudoreplication may apply also for any sites that might have been located close together on the same streams, and it is not clear from the outset that this was not the case.

Harvest Effects, Page 13, lines 426-442:

Comment: basically, this section shows that landowners followed the rules and harvested within RMAs to the maximum extent/amount allowed. Small fish stream RMAs especially showed significantly reduced basal area compared to "control" sites in all cases largely because of the

smaller size of buffers (site treatment medians consistently outside the "box" plot and non-overlapping boxes, in Figure 8B).

Comment: Small fish streams may be more numerous in length in most watersheds but not as numerous as type N (presumed non-fish) streams, which are not protected. This context is missing from the report. However, it is not insignificant that small fish streams took the bulk of the hit, so to speak, in terms of potential for long term LWD recruitment as well as other impacts.

Page 16, Lines 526-30:

> "Within the RMA, harvesting of conifer trees mostly occurred near the outer portion of the RMA (i.e., furthest away from the stream). This generally includes 50-70 feet away from stream along medium streams and 40-50 feet for small streams (Fig. 11a-d). Outside of the RMA, the large decrease in conifer basal area was associated with the adjacent clearcut."

Size of trees harvested; Page 14, Lines 467-471:

> "In comparing pre- and post1-harvest diameter distributions, harvesting appeared to target conifers in the smaller diameter classes along both medium and small streams (Figs. 9a-d). The mixed effects model results (Table 3) showed that the number of conifer trees (per 1000 ft of stream in the RMA) decreased for smaller trees (6-18 inch DBH class) along small and medium streams, but not for trees greater than 18 inches in DBH."

Comment: Clearly the smaller size classes (~6" to 18" DBH) are being selectively removed from the RMAs by the landowners (or operators). It appears that landowners are intending, or maintaining the option, to remove remaining conifers greater than 18" DBH, which would reduce future LWD recruitment in the larger more functional (i.e. stable, persistent) class. If that happens it would mean that DFC for RMAs is meaningless for instream habitat recovery, e.g. for fish.

Assumptions for Basal Area Targets (page 19, lines 559-565 (this is a key finding):

> "While these stands were at a desirable starting point (i.e., above the standard target), there is not sufficient information to identify whether the stands are on track to achieve desired future conditions. Additional analysis, such as modeling stand growth, would be required to project increases in stand basal area over time and to test the assumption regarding the change in basal area over time. The analysis up to this point does provide fundamental information about the landowner behavior with respect to harvesting in the RMA, which can be used to develop modeling and harvest scenarios."

Comment: Yes, with the strong caveat that the small size of the sample, and unknown and unclear selection criteria may limit interpretations of landowner behavior. Suggest re-visiting the interpretation, or testing the assumption, for example, that landowners will continue to favor

leaving larger Doug Fir in RMAs to meet residual (standard) basal area targets due either to current or future market conditions or lack of mills available to process larger logs. Models should incorporate a range of potential landowner behaviors allowed under the rules.

Large Wood in Stream Channels; Page 30-31, lines 837-856:

Comment: Shows that no real change in LWD in streams except smaller pieces (slash, portions of logs from upstream or potentially upslope). 5-10" LWD is not large enough to persist in channels, also "small debris jams" do not persist in channels. Any increase in LWD in streams was due to "other factors".

Page 31, Lines 842-844:

> "[suggests that] the increase in large wood was a result of other factors such as natural disturbance or upstream delivery of wood. We did not detect a change in large wood pieces over time in small streams in either control or treatment reach"

Comment: Hence no direct long-term benefit to streams from treatments. Support in text for this conclusion is at Lines 854-856:

> "From pre- to post1-harvest, there was a large increase in the frequency of small wood jams (Fig. 26). By post3-harvest, the distribution of wood jam size was similar to the pre-harvest distribution."

And in the Discussion; Page 34, Lines 960-969:

> "Harvesting tended to target smaller diameter conifer trees near the outer edge of the RMA along small and medium fish-bearing streams. There are a few possible explanations as to why harvesting targeted smaller diameter conifer trees. First, conifers such as Douglas-fir have a higher timber value than hardwoods, such as red alder. Second, smaller diameter conifers were more abundant than larger diameter conifers and likely had a greater probability of being harvested in certain situations where the clear cut extended into the RMA. Third, there are very few mills in western Oregon that can process larger diameter trees. Finally, the larger diameter trees, when left as part of the residual stand, can account for a greater portion of the total stand basal area as compared with smaller diameter trees. Consequently, this may provide some incentive for retaining large-diameter conifer trees."

Comment: The interpretation above could suggest another possibility which is that the landowners included in the study are not representative, or else are intending to harvest larger diameter conifers in the future, when market forces change or mill operations might re-tool. While larger trees currently can comprise the bulk of the basal area retention requirement, what happens later when it far exceeds that requirement? There is no rule disincentive for waiting until that occurs and then "gaming the DFC."

How will natural mortality of large trees occur under that scenario?

Average Mature Conditions; Page 35, Lines 982-985:

> "For stands that exceeded the standard target for conifer basal area (i.e., '6a' sites), trajectories of stand growth displayed a wide range in basal area increases over time. On average, these stands were above the standard target after harvest, placing them at a good starting point for maintaining average mature conditions."

Comment: This partially answers the question above. It's a starting point for maintaining but there is currently no requirement to maintain average mature conditions. Average mature conditions are the (vague and) only description of DFC related to LWD recruitment potential. It's happening in some sites now and possibly going to happen at some point in time in others, but there is no actual rule requirement to maintain that condition.

Page 37, Lines 1089-1090:

> "...the volume of downed wood decreased inside of the RMA along small streams"

Comment: A subset of down wood in sites adjacent to (or upslope of) small streams eventually ends up in them, even if it ends up smaller or partially decomposed. The smaller a stream generally speaking the smaller the functional piece of LWD.

Page 37, Lines 1103-1106:

> "The presence of large wood is particularly important for sustaining pools in streams with steeper gradients (which tends to be the case for small and medium streams), as compared with low- slope channels (Beechie and Sibley 1997). Maintaining a continuous supply of large wood pieces to streams from adjacent riparian forests is critical for protecting fish habitat in Oregon."

Comment: Steeper gradient streams are often associated with unstable slopes and can deliver significant amounts of LWD episodically e.g., during landslides or debris flows. This source of LWD can create productive long-term habitats that contribute to the DFC (at least the DFC for fish).

Page 37, Lines 1135-1147:

> "Overall, the increase in smaller diameter pieces as observed in this study is promising."

Comment: However, the report should critically examine this statement given that the increase was or will be temporary. So called "large" LWD pieces actually declined between year two and year three post-harvest (See Figure 24b, page 31) in small type F streams. Also, truly large LWD pieces do not develop given the time frame of the study. Both small and larger LWD contributions are ecologically very important. In medium Type F streams, there was a wider

range of sizes of LWD in controls than treatments, again particularly notable in years two and three post-harvest (Figure 24a, page 31). The results suggest a simplification of all the ecologically significant LWD components in both smaller and medium type F streams in treatments compared to controls. So not really all that promising. But again, one should be cautious in interpreting these trends, as meaningful as the study design is flawed with respect to time frame, lack of controls, baseline shift, and failure to ensure independence, not to mention inadequate sample size (18 sites, unknown criteria for selection) to have high statistical validity or certainty.

Page 38, lines 1136-37:

> **"However, long-term benefits of large wood in streams will likely be achieved through greater contributions by larger diameter, key pieces than what was observed in this study." [emphasis added]**

Comment: Yes. But as previously noted there is little likelihood this will happen under the current rules given that successive entry in multiple 50-year rotations will likely significantly reduce natural tree mortality both within and outside RMAs. LWD comes from both. RMAs do not include all sources.

Page 38, lines 1137-1147:

> "As mentioned above, these stands are relatively young stands (~40 yrs.) that established after clear cutting, so the contribution of larger diameter pieces will require time, well beyond the timespan of this study. Thus, modeling large wood recruitment is one approach to better understand the overall trajectories of managed riparian stands relative to unmanaged stands. Furthermore, a number of studies have evaluated the effects of harvesting on large wood recruitment (Hairstan-Strang and Adams 1988, Meleason et al. 2003, Czarnomski et al. 2008, Pollock and Beechie 2014, Benda et al. 2016, Burton et al. 2016), as well as stand age and temporal effects on large wood (Bilby and Ward 1991, Benda et al. 2002, Hassan et al. 2005), so a focused review of this type of literature will provide additional insight into the effectiveness of the FPA in achieving goals for large wood in streams."

Comment: Yes, first conduct a full literature review, including the above citations and any newer, studies. Then, assuming the really important questions are not already answered, design a new study, and conduct additional modeling. Modeling may be a better approach than more field studies given the long time lag and currently degraded (i.e. shifted) baseline of most streams in this particular landscape, particularly for LWD.

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