



Review Report for

Wood Equilibrium: A Conceptual Framework for Assessing the Wood Regime

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Round 1 Review

Comments by reviewer 1 (Anonymous Reviewer 2)

Completed: 2025-10-27 07:52 PM

Recommendation: Major Revisions

The manuscript titled “wood equilibrium: a conceptual framework for assessing the wood regime” is an interesting and useful contribution to understanding wood dynamics in rivers and floodplains. Although previous work has defined and assessed wood budgets, relating wood storage and storage capacity to the concept of equilibrium and trajectories of adjustment draws from fundamental concepts in fluvial geomorphology that have previously not adequately considered and integrated wood, even though wood is a strong and active agent of geomorphic processes in forested rivers. I have made general comments about the organization of the manuscript and a few larger questions below, followed by line-by-line comments. To better emphasize one of the unique contributions of this manuscript, I think it could be useful for the authors to highlight that the supply-limited and transport limited states in riverscapes is influenced by the physical complexity and characteristics of the storage zone of interest (here, the riverscape reach?), which is characterized/quantified in the manuscript by storage capacity. I suggest that the authors consider emphasizing this aspect of the wood equilibrium concept, since it highlights how geomorphic variability potentially modifies supplylimited and transport-limited states, and I think this may differ in emphasis somewhat from previous conceptualizations of transport and supply-limited rivers with respect to sediment. Greater emphasis on this aspect also highlights how the riverscape can adjust to different wood states through modification of storage capacity. I was also curious about the metric used for storage capacity and included in the line comments below a suggestion to more clearly justify this metric. I have additional comments and suggestions related to the organization of the manuscript that I think could improve it. For example, consider defining/explaining storage capacity should come earlier in the manuscript, prior to the explanation of the hypothesized adjustments and when first mentioned in the text. Could the authors provide examples of storage sites, and what increases or decreases storage capacity? I also made comments below about the hypotheses specific to the field examples—could these be generalized and moved into the introduction, so that they are clearly illustrations of the wood equilibrium concept? As a reader, I find that the hypotheses and revisions of hypotheses in the field methods and results/discussion are harder to follow, since they are not situated within the introduction. I suggest that the manuscript more clearly links the specific hypotheses tested at the field sites to the wood equilibrium concept described in the introduction. Finally, there are many details included about wood storage patterns and changes in wood storage at the field example sites. Many of these details are useful, but I suggest that the authors think carefully about what details are included and how the main conclusions from the field sites are summarized. This would help with emphasizing the main take-aways that illustrate how the wood equilibrium concept, when applied to these rivers, can help assess the wood regime. Line comments: Line 11: The authors could potentially consider using the term, retention capacity, to help the reader differentiate between storage and retention. Line 65: I would suggest expanding here on these concepts, at least somewhat, and particularly in relation to the ratio of sediment supply to transport capacity. I think that would provide further context for the wood equilibrium concept presented. Line 69: Recruitment and transport are supply, if the reference frame is what comes into a river reach? Is that correct? I suggest clarifying here. Line 72: could the authors provide example refs of supply-limited work and transport-limited work? Line 73: what do the authors mean by equilibrium-related terminology? Line 83: Can the authors define





what they mean by wood storage capacity, to increase clarity? Line 88: could the authors provide example timespans? Because the authors are relying on the equilibrium concept, I think it could be useful here or elsewhere to be more specific about timescale, and the timescale (or timescales) over which the wood equilibrium concept applies. Line 120: what would cause the lull in processes? And, could the authors indicate some time period here? Line 131: after reading through the different potential adjustments, I wondered whether the arrows within the figure should be bi-directional. For example, what if the river segment starts as low storage capacity and low wood storage, but then experiences significant wood addition (increased wood supply)? Or, are the arrows only for geomorphic adjustment, and not related to wood supply input? This would potentially be good to clarify. Or, would it make sense to add changes in wood supply to the figure in some way? Line 152: question here: are the authors stating that the channel and the floodplain are different wood process domains? I think it could be useful to include a broader conceptualization of process domains (e.g., mountain headwater streams versus lowlands, or something similar), in addition to describing different process domains in terms of channel versus floodplain.

Line 155: Similar to the timescale comment above, I think it would be helpful to be more specific when discussing timescale. Do the authors mean, the timescale over which the river segment is in wood equilibrium? If so, it would be good to be explicit about that. Line 157: instead of highly variable, what about “less frequent floods” as the contrast to frequent floods? Line 170: instead of “define” wood equilibrium, suggest stating, “can be used to assess wood equilibrium”, since the definition of wood equilibrium as provided above relates to a state of wood storage relative to retention capacity, not the metrics themselves. Line 207: Suggest removing the word “hypothesis” if this isn’t a main hypothesis of the paper/manuscript. If it is a main hypothesis of the manuscript, then I would suggest moving it to the introduction. Line 231: For this hypothesis, and the one above, are there ways to generalize these hypotheses and put them in the introduction, either before or after the explanation of the wood equilibrium concept? These hypotheses (or different formulations of them) could be used to illustrate different states (equilibrium and disequilibrium), and the analyses of whether they are in equilibrium results in explanations for why? Line 280: why was this straightforward? Line 297-300: Could the authors re-phrase this so that the assumption of similar height of wood jams was led by observing that from field data, not for the need to make that assumption? Line 301: in the field? Or on imagery? It would be helpful to be more clear about what was measured in the field versus remotely. Line 308: Could the authors provide an example of unmappable area? Area underneath a tree canopy? Line 334: Does the piton et al., 2024 reference use the same metric for wood storage capacity? If so, suggest stating that clearly, that the authors are using a previously used metric for wood storage capacity. Phrasing in the suggested way would provide stronger justification for using this metric. Line 357: it would be useful to clearly define “segment” versus “reach”. I may have missed it above, but is each study river considered a segment (tolt and Snoqualmie), and then the segments have reaches? In addition, this description of testing whether the segments are in wood equilibrium could be clearly stated in the introduction, to link the field cases with the wood equilibrium description.

Line 373: could the authors provide more support for why at least 10 is the chosen number?

Line 504: Meaning that the tolt in 1994 was in wood equilibrium? Because there was sufficient supply, which is indicated by the next sentence?





Line 612: in this instance, would that be a narrowing of the reach width, since the width is the metric for storage capacity?

Line 635: Because the assessment of the equilibrium condition is only of the reaches without local wood supply, how does this statement about local wood supplying influencing lack of adjustment fit within the assessment of equilibrium? I may be misunderstanding a piece of this discussion, so further clarification could help here.

Line 662-663: I can see that the wood equilibrium concept relates wood supply and wood storage, but as presented, it is assessed through relating wood storage to wood storage capacity and assessing whether that indicates a lack of supply. Could the authors rephrase here to make this clear, since as written it implies assessing wood supply?

Line 673: missing period here

Line 701: or, the inverse of transport capacity?





Comments by reviewer 2 (Tomáš Galia)

Completed: 2025-10-26 07:27 PM

Recommendation: Minor Revisions

Dear Authors, Dear Editor,

The manuscript presents an interesting and innovative concept of a wood equilibrium framework based on the field (or rather remote sensing) assessment of existing wood storage versus wood storage capacity. I highly appreciate this idea and the authors' effort to describe the wood regime of rivers through this conceptual approach, as well as their attempt to demonstrate its applicability using real-world case studies and measured data. I have no objections to the overall development of the concept, but I would like to offer several recommendations that could help clarify and strengthen the understanding of the presented framework.

(i) Applicability to different river types The proposed framework appears to work well for the study cases: relatively shallow, geomorphologically dynamic rivers with frequent wood recruitment and transport, where almost all wood can be detected from aerial imagery. However, I would appreciate it if the authors could discuss how this concept could be applied to different types of channels, such as small streams with limited transport capacity, or large, lowland rivers with restricted recruitment (e.g., due to human-altered floodplains) but high wood-transport potential. In such cases, how would the state of wood equilibrium be reflected? A short discussion on this issue would help readers understand the broader applicability of the framework.

(ii) Identification of segments with local wood supply The definition of segments with "active wood recruitment" (local wood supply) seems somewhat vague. Perhaps the authors plan to elaborate on this in a future paper, but I wonder how robustly these segments can be distinguished from others (as shown, for example, in Figure 5). During major flood events, wood recruitment through bank erosion may occur along almost continuous channel sections, except in strongly confined reaches. Could such events generate recruitment rates comparable to those currently categorized as "local wood supply" in the figures? A clearer explanation or justification for this classification would be helpful.

(iii) Wood storage capacity The concept of storage capacity should be described in a bit more detail. In my opinion, this capacity likely varies with flood magnitude, i.e. with the area subject to inundation. It could also be useful to discuss how roughness elements (e.g., vegetated islands vs. bare gravel bars) influence trapping efficiency, and whether this could be incorporated through a coefficient analogous to Manning's roughness coefficient. Such an approach could refine the estimation of "storage capacity" and enhance the framework's applicability.

(iv) Longitudinal profile orientation Is there a particular reason for showing the longitudinal profiles with "distance upstream" on the x-axis? It might be clearer to reverse the axis (showing "distance downstream"), which would better reflect the fluvial continuum and the potential wood transfer between consecutive segments.

(v) Clarification of measurement approach (l. 300–302) The text states that "every visible large wood was measured." Was this performed in the field or from aerial images? If the analysis focuses on areal polygons representing wood-covered surfaces, please clarify the specific purpose of these individual measurements and how they were used in the analysis. As mentioned above, I fully support the conceptual idea of wood equilibrium and its demonstration using direct data. My comments are intended solely to clarify the methodology and to encourage discussion about the broader applicability of the framework.

I hope you find my remarks helpful.





Best regards.





Recommendation by Associate Editor 2 (Anonymous)

Completed: 2025-11-14 03:22 AM
Recommendation: Moderate Revision

Dear authors and editor

I read with interest your manuscript entitled “Wood Equilibrium: A Conceptual Framework for Assessing the Wood Regime”, as well as the reviews from two anonymous reviewers. Based on my reading of your manuscript and the reviews, I recommend that the manuscript be accepted after a moderate revision.

Best wishes

Anonymous Associate Editor





Decision by Editor-in-Chief (Alice Lefebvre)

Completed: 2025-11-17

Recommendation: Moderate Revision

We would like you to undertake a **moderate revision** of your manuscript (I know the title of the email says major, but I cannot edit it). As this is a moderate revision decision, we will assess internally if the revised manuscript should be sent to one or both of the reviewers.





Authors' responses to review comments

2026-01-12

We appreciate the opportunity to revise this manuscript and the helpful comments from the reviewers. We have revised this manuscript according to these comments. Responses to these comments are shown below, associated with each comment, in *italics*. Please also see the tracked changes version of the revised manuscript.

Reviewer 1:

The manuscript titled “wood equilibrium: a conceptual framework for assessing the wood regime” is an interesting and useful contribution to understanding wood dynamics in rivers and floodplains. Although previous work has defined and assessed wood budgets, relating wood storage and storage capacity to the concept of equilibrium and trajectories of adjustment draws from fundamental concepts in fluvial geomorphology that have previously not adequately considered and integrated wood, even though wood is a strong and active agent of geomorphic processes in forested rivers. I have made general comments about the organization of the manuscript and a few larger questions below, followed by line-by-line comments.

Thank you for this thoughtful and constructive review. We have edited the manuscript in response to your comments.

To better emphasize one of the unique contributions of this manuscript, I think it could be useful for the authors to highlight that the supply-limited and transport limited states in riverscapes is influenced by the physical complexity and characteristics of the storage zone of interest (here, the riverscape reach?), which is characterized/quantified in the manuscript by storage capacity. I suggest that the authors consider emphasizing this aspect of the wood equilibrium concept, since it highlights how geomorphic variability potentially modifies supply-limited and transport-limited states, and I think this may differ in emphasis somewhat from previous conceptualizations of transport and supply-limited rivers with respect to sediment. Greater emphasis on this aspect also highlights how the riverscape can adjust to different wood states through modification of storage capacity.

Excellent point. We added a paragraph to section 3.2.4 discussing how the variability in retention capacity over time on the Tolt highlights this.

I was also curious about the metric used for storage capacity and included in the line comments below a suggestion to more clearly justify this metric.

Please see responses to line comments below.

I have additional comments and suggestions related to the organization of the manuscript that I think could improve it. For example, consider defining/explaining storage capacity should come earlier in the manuscript, prior to the explanation of the hypothesized adjustments and when first mentioned in the text.

We now define retention capacity at its first usage, and we elaborated on the description in section 2.4. We also refer to section 2.4 in the initial definition.

Could the authors provide examples of storage sites, and what increases or decreases storage capacity? *We expanded on section 2.4 to add more information on retention capacity, including emphasizing the landforms that act as storage sites and how the presence/absence of those influence retention capacity.*

I also made comments below about the hypotheses specific to the field examples—could these be generalized and moved into the introduction, so that they are clearly illustrations of the wood equilibrium concept? As a reader, I find that the hypotheses and revisions of hypotheses in the field methods and results/discussion are harder to follow, since they are not situated within the introduction. I





suggest that the manuscript more clearly links the specific hypotheses tested at the field sites to the wood equilibrium concept described in the introduction.

Please see response to that line comment (line 231). In short, we have generalized the hypotheses and moved them to the introduction.

Finally, there are many details included about wood storage patterns and changes in wood storage at the field example sites. Many of these details are useful, but I suggest that the authors think carefully about what details are included and how the main conclusions from the field sites are summarized. This would help with emphasizing the main take-aways that illustrate how the wood equilibrium concept, when applied to these rivers, can help assess the wood regime.

Good point – the results and discussion had gotten long and a bit wandering at times. We tightened them up, removing some extraneous material to narrow the focus on direct implications for the newly reframed hypotheses. We also revised the discussion sections to more quickly emphasize the main interpretations from the results.

Line comments:

Line 11: The authors could potentially consider using the term, retention capacity, to help the reader differentiate between storage and retention.

We love this suggested change in terminology – we use “retention” so much in speaking about rivers, and it is definitely a better term than storage when referencing the transient stops wood makes on its way downstream (or into the soil or atmosphere). We have changed terminology throughout to now refer to “wood retention capacity” instead of “wood storage capacity.” This also necessitated a more general reworking of our terminology throughout the manuscript away from storage and towards wood retention and wood load. We also added a note early in the introduction to explain to readers that “storage” is another term folks use for retention, just in case there is confusion.

Line 65: I would suggest expanding here on these concepts, at least somewhat, and particularly in relation to the ratio of sediment supply to transport capacity. I think that would provide further context for the wood equilibrium concept presented.

We expanded this paragraph to more explicitly connect the supply/transport capacity ratio to wood equilibrium.

Line 69: Recruitment and transport are supply, if the reference frame is what comes into a river reach? Is that correct? I suggest clarifying here.

We edited this to more explicitly define supply in an expanded parenthetical.

Line 72: could the authors provide example refs of supply-limited work and transport-limited work?

We added some example references on sediment supply- and transport-limitation with respect to sediment. With respect to wood, we provide references to papers that implicitly or explicitly use these equilibrium-related terms when describing the wood regime.

Line 73: what do the authors mean by equilibrium-related terminology?

We defined this in this sentence.

Line 83: Can the authors define what they mean by wood storage capacity, to increase clarity?





We changed this to retention capacity, as suggested above, and added a brief parenthetical definition here. We also added a lot more description in section 2.4 and referenced that section here, in case readers want to skip ahead.

Line 88: could the authors provide example timespans? Because the authors are relying on the equilibrium concept, I think it could be useful here or elsewhere to be more specific about timescale, and the timescale (or timescales) over which the wood equilibrium concept applies.

We explicitly discuss timescale in section 1.2. We felt it was appropriate to first discuss the general concept of wood equilibrium, then devote the entire next subsection to temporal and spatial scale.

Line 120: what would cause the lull in processes? And, could the authors indicate some time period here?

This could be due to a temporary lack of high flows, lack of channel migration due to a lack of sediment supply, or many other causes. However, we feel that adding detail on what might cause such a lull could risk distracting from the point we are trying to make here. We revised this sentence to focus more generally on wood recruitment processes. Going into more detail here on what might cause such a lull would risk distracting from the main point of this paragraph.

We added a relative time period over which a drop in supply could cause supply limitation.

Line 131: after reading through the different potential adjustments, I wondered whether the arrows within the figure should be bi-directional. For example, what if the river segment starts as low storage capacity and low wood storage, but then experiences significant wood addition (increased wood supply)? Or, are the arrows only for geomorphic adjustment, and not related to wood supply input? This would potentially be good to clarify. Or, would it make sense to add changes in wood supply to the figure in some way?

Yes, absolutely. Modes b and c are geomorphic changes (or, more broadly, changes in retention capacity, which could also be driven by, say, changes in vegetation), whereas modes d and a are changes in wood load (which are very likely due to changes in supply), although they can happen together. We added text to note this explicitly, changed the arrows to be bidirectional in Fig 1, and modified the caption of Fig 1.

Line 152: question here: are the authors stating that the channel and the floodplain are different wood process domains? I think it could be useful to include a broader conceptualization of process domains (e.g., mountain headwater streams versus lowlands, or something similar), in addition to describing different process domains in terms of channel versus floodplain.

Yes, process domains can vary both laterally and longitudinally. We clarified this in editing this section.

Line 155: Similar to the timescale comment above, I think it would be helpful to be more specific when discussing timescale. Do the authors mean, the timescale over which the river segment is in wood equilibrium? If so, it would be good to be explicit about that.

We rewrote this paragraph to be more explicit and clear about this topic.

Line 157: instead of highly variable, what about “less frequent floods” as the contrast to frequent floods? In rewriting this paragraph, we changed to focus more on flood frequency than variability, as suggested. We also focused more on the recency of large floods.





Line 170: instead of “define” wood equilibrium, suggest stating, “can be used to assess wood equilibrium”, since the definition of wood equilibrium as provided above relates to a state of wood storage relative to retention capacity, not the metrics themselves.

Good point – we edited as suggested.

Line 207: Suggest removing the word “hypothesis” if this isn’t a main hypothesis of the paper/manuscript. If it is a main hypothesis of the manuscript, then I would suggest moving it to the introduction.

We replaced “hypothesized” with “suspected” here as part of the larger reframing of our hypotheses in response to comments above.

Line 231: For this hypothesis, and the one above, are there ways to generalize these hypotheses and put them in the introduction, either before or after the explanation of the wood equilibrium concept? These hypotheses (or different formulations of them) could be used to illustrate different states (equilibrium and disequilibrium), and the analyses of whether they are in equilibrium results in explanations for why?

This is a great suggestion, and after implementing it, we realize this is a better framing for the paper. We have reframed sections 1 and 2 to introduce a new section 1.3 detailing objectives and hypotheses. Our hypotheses specific to each study segment are now generalized in section 1.3. We then explain how each study site corresponds to each hypothesis in 1.3.

Line 280: why was this straightforward?

In short, we were comparing wood load, which is easily measurable with many established methods, to wood retention capacity, which is a much more nebulous concept that is more difficult to design a good metric for. In revising this section, though, we deleted this sentence.

Line 297-300: Could the authors re-phrase this so that the assumption of similar height of wood jams was led by observing that from field data, not for the need to make that assumption?

Rephrased as suggested.

Line 301: in the field? Or on imagery? It would be helpful to be more clear about what was measured in the field versus remotely.

Clarified here.

Line 308: Could the authors provide an example of unmappable area? Area underneath a tree canopy?

Clarified here that this is solely due to obscuration by tree canopy.

Line 334: Does the piton et al., 2024 reference use the same metric for wood storage capacity? If so, suggest stating that clearly, that the authors are using a previously used metric for wood storage capacity. Phrasing in the suggested way would provide stronger justification for using this metric.

The cited reference does not refer to this measurement as storage capacity, so we can’t accurately state that they use it as a metric of storage capacity. In that study, the authors found that wood quantity correlated positively with active channel width. We rephrased this slightly.

Line 357: it would be useful to clearly define “segment” versus “reach”. I may have missed it above, but is each study river considered a segment (tolt and Snoqualmie), and then the segments have reaches?





We now define “segment” in the methods under section 2.6. We use segment generally to refer to multiple reaches, so a segment can be the entire studied length of each river or a subset of that. In addition, this description of testing whether the segments are in wood equilibrium could be clearly stated in the introduction, to link the field cases with the wood equilibrium description. Good point – we edited the last paragraph of section 1.1 to note this, using language similar to this section.

Line 373: could the authors provide more support for why at least 10 is the chosen number?
While we would like to provide a definite and well-justified lower bound, we cannot – it depends too much on variability in the correlation between wood retention capacity and wood load. We felt that using “on the order of” helps emphasize that even 10 isn’t a strict limit. To clarify that, we rephrased this to read “at least on the order of 10” instead of “on the order of at least 10”.

Line 504: Meaning that the tolt in 1994 was in wood equilibrium? Because there was sufficient supply, which is indicated by the next sentence?
Yes – this is discussed in section 3.2.4. We felt that it would be best to separate factual results, such as those presented here, from our interpretive judgement regarding equilibrium status, which we put in a separate discussion section (3.2.4).

Line 612: in this instance, would that be a narrowing of the reach width, since the width is the metric for storage capacity?
We assume this comment applies to line 621, as the referenced line only discusses wood supply, not storage capacity. Yes, this should manifest as a reduction in total width but note that we use width only as a proxy for the many factors that determine retention capacity. Clarified here.

Line 635: Because the assessment of the equilibrium condition is only of the reaches without local wood supply, how does this statement about local wood supplying influencing lack of adjustment fit within the assessment of equilibrium? I may be misunderstanding a piece of this discussion, so further clarification could help here.
This was a confusing use of “local wood supply”. We rephrased to clarify, but also substantially rewrote this portion in response to the comment above about refining the details of the wood equilibrium assessment findings to better tie back to the main messages of the paper. This statement regards the implications of the wood equilibrium assessment for avulsion processes and the large wood cycle on the Tolt, and not necessarily about the wood equilibrium state.

Line 662-663: I can see that the wood equilibrium concept relates wood supply and wood storage, but as presented, it is assessed through relating wood storage to wood storage capacity and assessing whether that indicates a lack of supply. Could the authors rephrase here to make this clear, since as written it implies assessing wood supply?
We rephrased this to clarify how equilibrium state can indicate the ratio of supply to storage, as opposed to that ratio being used to assess equilibrium.

Line 673: missing period here
Corrected.

Line 701: or, the inverse of transport capacity?





Yes – edited to note that.

Reviewer 2:

The manuscript presents an interesting and innovative concept of a wood equilibrium framework based on the field (or rather remote-sensing) assessment of existing wood storage versus wood storage capacity. I highly appreciate this idea and the authors' effort to describe the wood regime of rivers through this conceptual approach, as well as their attempt to demonstrate its applicability using real-world case studies and measured data.

I have no objections to the overall development of the concept, but I would like to offer several recommendations that could help clarify and strengthen the understanding of the presented framework.

Thank you for these helpful comments. We have revised the manuscript following these recommendations, as detailed below.

(i) Applicability to different river types

The proposed framework appears to work well for the study cases: relatively shallow, geomorphologically dynamic rivers with frequent wood recruitment and transport, where almost all wood can be detected from aerial imagery. However, I would appreciate if the authors could discuss how this concept could be applied to different types of channels, such as small streams with limited transport capacity, or large, lowland rivers with restricted recruitment (e.g., due to human-altered floodplains) but high wood-transport potential.

In such cases, how would the state of wood equilibrium be reflected? A short discussion on this issue would help readers understand the broader applicability of the framework.

We agree that this is an important point and do wish to emphasize that wood equilibrium could be a useful framework for any river that contains downed wood. We had previously described assessments in other systems at the end of section 3.3, but we now separate that material out in a new section (3.4) to call attention to it. We have also added additional information to that section. While we can't address all potential applications, we tried to speak generally about what methods would require modification to assess different wood process domains. We tried to stay away from detailed methods (e.g., recommending where to use remote sensing vs ground surveys, which will vary as technology progresses), but hopefully give sufficient conceptual information to allow readers to

(ii) Identification of segments with local wood supply

The definition of segments with "active wood recruitment" (local wood supply) seems somewhat vague. Perhaps the authors plan to elaborate on this in a future paper, but I wonder how robustly these segments can be distinguished from others (as shown, for example, in Figure 5). During major flood events, wood recruitment through bank erosion may occur along almost continuous channel sections, except in strongly confined reaches. Could such events generate recruitment rates comparable to those currently categorized as "local wood supply" in the figures? A clearer explanation or justification of this classification would be helpful.

We rewrote section 2.5, which explains the criteria for denoting a reach as having a local wood supply or not. Based on field experience with these rivers, we feel very confident in our delineation. The high frequency of high resolution aerial imagery gives us good confidence in detecting things like bank erosion into streamside forest or landsliding. We may miss very small sources of recruitment, such as individual tree fall that occurs during a flood, generates no evidence of bank erosion or noticeable change in canopy cover, and is transported downstream prior to the imagery we observed. However, these small sources of error are likely negligible. The more important deficit in our methodology is that here, we did not quantify the magnitude of recruitment, which we discuss in the methods and indeed see





as an improvement that must be addressed in future work. We now note that more explicitly in the conclusions.

(iii) Wood storage capacity

The concept of storage capacity should be described in a bit more detail. In my opinion, this capacity likely varies with flood magnitude, i.e. with the area subject to inundation. It could also be useful to discuss how roughness elements (e.g., vegetated islands vs. bare gravel bars) influence trapping efficiency, and whether this could be incorporated through a coefficient analogous to Manning's roughness coefficient. Such an approach could refine the estimation of "storage capacity" and enhance the framework's applicability.

We expanded on the storage capacity section to describe it in more detail in response to this comment and comments from reviewer 1. We don't currently see a clear way to translate retention capacity into a coefficient like a Manning's roughness coefficient, although that would be an exciting prospect to investigate further. This is partly due to the variety of processes that can regulate retention capacity and the potential for feedbacks between wood load and retention capacity. We note these in sections 2.4 and 4. We also expanded on the idea of multivariate measures of wood retention capacity in section 3.4. This is a good point about flood magnitude, and one that we struggled with in drafting the manuscript. For our study, we are comparing retention capacity (term has changed in this revision) in a solely relative capacity across a relatively short length of river in which flood magnitude for each time period studied varies minimally. So, for the relative evaluations we do for each set of analysis (Snoqualmie, 1994 Tolt, 2009 Tolt, and 2023 Tolt), we feel that it is ok to essentially ignore flood magnitude, as it should vary little across each study segment. However, this also means that we cannot make reliable absolute comparisons of the magnitude of retention capacity between our study segments (e.g., between 1994 and 2009 on the Tolt). We noted this issue at the end of section 2.4 but have now expanded on that note.

(iv) Longitudinal profile orientation

Is there a particular reason for showing the longitudinal profiles with "distance upstream" on the x-axis? It might be clearer to reverse the axis (showing "distance downstream"), which would better reflect the fluvial continuum and the potential wood transfer between consecutive segments.

We used distance upstream to ensure that the orientation of the longitudinal profile at least approximately aligns with the orientation of the site maps shown in Figure 2 (which is generally oriented north up, trying to follow typical map conventions). As noted, this does come at a slight cost of the longitudinal profile being less intuitive, but we feel that such a tradeoff is worthwhile to allow readers to go back and forth between the map (Figure 2) and the longitudinal profiles, if they desire to do so. However, to help clarify the directionality of the profiles, we added flow direction arrows to the longitudinal profiles.

(v) Clarification of measurement approach (l. 300–302)

The text states that "every visible large wood was measured." Was this performed in the field or from aerial images? If the analysis focuses on areal polygons representing wood-covered surfaces, please clarify the specific purpose of these individual measurements and how they were used in the analysis. *We clarified in this paragraph that it was done using aerial imagery. Section 2.6 elaborates on how these measurements were used in the analysis (i.e., testing for a monotonic, positive correlation between wood load and wood retention capacity). We have revised and clarified that section.*





As mentioned above, I fully support the conceptual idea of wood equilibrium and its demonstration using direct data. My comments are intended solely to clarify the methodology and to encourage discussion about the broader applicability of the framework.

I hope you find my remarks helpful.

Thank you – these remarks have indeed been helpful.





Round 2 Review

Comments by reviewer 1 (Anonymous Reviewer 3)

Completed: 2026-02-14 08:41 AM

Recommendation: Minor Revisions

The submitted manuscript is a contribution to the study of large wood dynamics of large rivers using state of the art geospatial technologies. The manuscript is timely, considering the accelerated impacts of global environmental change, fire, drought, and runoff on watershed health and the ecological functions that wood provide. The last revisions greatly enhanced the manuscript making it easy to follow. I have a few minor suggestions and a few questions, mostly to add to the background.

- I think there is a missed opportunity to highlight the benefits of large wood, including as a habitat forming feature for fish and other aquatic species. To add to the background, any background on who manages (e.g., state park) these sections of the study sites would be helpful, including a little more background on the disturbance history typically found in the bioclimatic region of study that may contribute to the fate of recruitment of wood in the systems of study.
- In the study site areas, what are the forest stand characteristics contributing to instream wood?
- In the abstract, it is hard to understand if the study sites are coastal rivers, large or small rivers, largely undisturbed/protected, etc.
- Lines 194-196: Reword or state in results or discussion.
- Line 317: What season or time of year did field observations take place? Did it matter if it was summer low flows or spring high flows?
- Figure 2: Light blue arrows are hard to see. I suggest to either enlarge, add outline, or change the color. Otherwise, great imagery in the figures.





Comments by reviewer 2 (Tomáš Galia)

Completed: 2026-01-20 07:00 PM

Recommendation: Accept

Dear Authors,

Thank you for submitting the revised version of your manuscript. The paper has improved compared to the initial submission, although I did not have any major concerns with the original version. I also appreciate your detailed and clear responses to my comments, which satisfactorily addressed all points raised.

I congratulate you on your work and wish you all the best.





Recommendation by Associate Editor 2 (Anonymous)

Completed: 2026-03-03 07:06 AM

Recommendation: Accept

Dear authors,

I read with interest your manuscript entitled “Wood Equilibrium: A Conceptual Framework for Assessing the Wood Regime”, as well as the reviews from two anonymous reviewers. Based on my reading of your manuscript and the reviews, I would like to recommend for the manuscript to be accepted.

Best wishes

Anonymous Associate Editor





Decision by Editor-in-Chief (Alice Lefebvre)

Completed: 2026-03-03

Recommendation: Minor Revision

I would like you to undertake a **minor revision** of your manuscript. I am aware that one reviewer and the anonymous associate editor already suggested accepting your paper, but one reviewer still had some minor comments which I would like you to address before accepting the paper. I'll assess those minor revisions myself to make the process faster.





Authors' responses to review comments

2026-03-10

We appreciate the opportunity to revise this manuscript and the helpful comments from the reviewer. We have revised this manuscript according to these comments. Responses to these comments are shown below, associated with each comment, in *italics*. Please also see the tracked changes version of the revised manuscript.

Reviewer 2:

The submitted manuscript is a contribution to the study of large wood dynamics of large rivers using state of the art geospatial technologies. The manuscript is timely, considering the accelerated impacts of global environmental change, fire, drought, and runoff on watershed health and the ecological functions that wood provide. The last revisions greatly enhanced the manuscript making it easy to follow. I have a few minor suggestions and a few questions, mostly to add to the background.

I think there is a missed opportunity to highlight the benefits of large wood, including as a habitat forming feature for fish and other aquatic species.

We edited the existing statement that highlights the function of large wood to emphasize its habitat formation functions.

To add to the background, any background on who manages (e.g., state park) these sections of the study sites would be helpful, including a little more background on the disturbance history typically found in the bioclimatic region of study that may contribute to the fate of recruitment of wood in the systems of study.

There is no single manager of either of the two study segments. They are located in areas with a diverse mix of land ownership (both private and public) and management. Decisions regarding managing both of these river segments are made at the landowner, municipal, county, state, and federal level (sometimes decisions must pass through approvals at all of those levels, but sometimes individual levels will make decisions), with input from various communities at all stages. Given its complexity, we do not feel that reviewing that complicated decision-making and management structure would be within the scope of the manuscript.

As for disturbance history, the primary disturbances with regards to wood are forest harvest and flooding. We detail forest harvest history in paragraph 1 of section 2.1. We detail recent flood history for the Snoqualmie and Tolt as they are relevant to the study in their respective sections of the results. We feel that these details would be missed if mentioned in the methods, whereas mentioning them in the results will help readers refer to them beside the information they are intended to contextualize.

Given the above, we have made no changes in response to this comment.

In the study site areas, what are the forest stand characteristics contributing to instream wood?





This was definitely an oversight on our part – thanks for catching this missing information. We have added a list of dominant species to paragraph 1 of section 2.1.

In the abstract, it is hard to understand if the study sites are coastal rivers, large or small rivers, largely undisturbed/protected, etc.

We added the clarifiers “large, lowland” to the description of the study rivers in the abstract to help readers better understand at a high level the types of rivers studied here.

Lines 194-196: Reword or state in results or discussion.

We reworded this statement for clarity.

Line 317: What season or time of year did field observations take place? Did it matter if it was summer low flows or spring high flows?

Field observations took place during all seasons, although we do not feel that is relevant information to mention, as we are not evaluating vegetation health or other factors that tend to vary with season. We did visit each site exclusively at low flows, however, and have clarified that here.

Figure 2: Light blue arrows are hard to see. I suggest to either enlarge, add outline, or change the color. Otherwise, great imagery in the figures.

Thanks – we added an outline to highlight the arrows.





Round 3 Review

Decision by Editor-in-Chief (Alice Lefebvre)

Completed: 2026-03-10

Recommendation: Accepted

Congratulations! I am pleased to inform you that your manuscript entitled "Wood Equilibrium: A Conceptual Framework for Assessing the Wood Regime" has been accepted for publication in Geomorphica.

