



Review Report for

Wood jam characteristics influence but do not fully explain wood jam morphologic functions

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Round 1 Review

Comments by reviewer 1 (Tomáš Galia)

Completed: 2024-06-26 02:14 PM

Recommendation: Minor Revisions

Dear Author and Editor,

The reviewed paper deals with the prediction of the geomorphic effects of wood jams, assessing several parameters of jams with respect to their geomorphic potential. This topic is interesting not only to scientists studying biogeomorphic interactions in rivers, but also to practitioners involved in designing river restoration projects. I appreciate the extensive dataset of wood jams (>700 observations, although some were repeated - but still independent), which provides a strong basis for assessing this problem. The paper is quite long but easy to follow: I appreciate the tables in the results section with clear explanations of the multiple regression outcomes. I have no objections to the methodology used or the interpretation of the results. The conclusion that the design and monitoring of river restorations should focus on a series of jams rather than the geomorphic impact of a single jam (i.e., it is difficult to predict the morphological impact of a single jam, and it is better to account for the variability of these impacts across the whole river reach) is an important message for management.

I have only two points that may be included or emphasized in the discussion.

(i) I fully agree with the statement that several factors, such as sediment supply and transport capacity, are important for the resulting morphological impact of a jam but are difficult to quantify (also considering variations in hydrology and the evolutionary trajectory of the river). In this regard, the author examined most of the jams in NW USA with a specific type of riparian tree as a source of large wood. Does the author have any idea if there would be differences in the "intensity" of geomorphic impact of jams (based on different hydrological regimes, tree recruitment, wood decay, and jam longevity) in other bioclimatic regions? There are some suggestions regarding various calibers of bed sediments (which I agree with), but other aspects could be briefly discussed to highlight the global potential of your obtained results (and perhaps express some knowledge gaps).

(ii) From the perspective of river restorations involving artificial wood supply, your reported statistics suggests that adding "complex" large wood with attached root wads or branches can enhance the geomorphic effect of a jam. Am I right? I think this issue could be emphasized in section 5.2.

Additionally, I have a minor issue: I am not sure about the size of the bed sediments, but I suppose this research was done in gravel-bed rivers. Please add some details somewhere in lines 104-110, because I think the morphological effect of wood jams can also be studied in meandering or anastomosing rivers with finer sediments (and this effect can be slightly different when compared to non-cohesive gravel particles).

Congratulations on this great work!





Comments by reviewer 2 (Julianne Scamardo)

Completed: 2024-07-01 04:11 PM

Recommendation: Major Revisions

For author and editor

The following is a review for the manuscript titled, 'Wood jam characteristics influence but do not fully explain wood jam morphologic functions'. Here, the author presents a dataset of wood jam characteristics and impacts (including the presence and magnitude of jam-induced features) for a wide range of field sites in the Rocky Mountains and Pacific Northwest. The manuscript is well-written and provides insightful results into the useful (or not so useful) features of wood jams as well as thoughtful and timely discussion on the scales at which we monitor and restore wood in rivers. However, parts of the manuscript could benefit from additional clarity, particularly given the intricacies of the dataset and discussion. Overall, I feel that the submitted work would be a good contribution to Geomorphica following major revisions. See below for more detailed comments.

General comments:

The paper presents an impressive dataset, particularly for a field-based study. As you mention throughout the methods, there are some intricacies to this dataset, including variations in what metrics were measured and where. Additionally, from lines 166-167, it seems that you might have measured all the WoodDAM metrics at some sites, but it seems that certain metrics (e.g., decay class) are not mentioned in the paper. To improve clarity, I would recommend adding a table with the metrics you measured, a brief description of those metrics, and a column with the sites where those measurements were made. Also, even if all the WoodDAM variables were measured at some sites, I encourage you to focus on only the variables used in your full models, and then to also describe which variables (if any) were not important in the selected models.

Overall, there are very few photos of the types of jams and jam-induced features you were measuring. An additional figure with visual examples of the jam-induced features (i.e., scour pools, forced bars, cut banks, and sediment wedges) could be helpful.

Finally, regarding the statistical models, you provide a good justification for why you present models using different sample sizes (i.e., you want to have the largest sample size possible for a given model). However, this becomes somewhat confusing when you present two different models of the same response variable and discuss them as one in the results. For example, you list jam porosity, blockage ratio, root wads, and substrate as important predictors for scour pool presence in Lines 293-294. However, root wads were also presumably included as a predictor in the full model for the smaller dataset but did not emerge as a significant predictor in the final model, which could leave one wondering about the true importance of root wads. This comes up again while predicting forced bar presence; alone, forced bars are only predicted by root wads, but when combined with sediment wedges, a larger set of predictors emerges. Additional reporting and discussion on the similarities and differences between models predicting the same outcome would be beneficial both in the results





and discussion sections. Additionally, plots (scatterplots or boxplots, depending on the predictor) showing these correlations while highlighting the different datasets could be informative.

Line-by-Line comments:

Lines 104- 107: What about channel planform? Could you give more information on whether these are all single threaded reaches or if some are multi-threaded. Based on the map (and the reach characteristics), it seems that the reaches may be spanning process domains as well, particularly in Washington. Consider more discussion on this.

Lines 142 – 153: These sentences are well-written are interesting, but this analysis feels like a tangent, and it involves results and discussion on metrics that haven't been introduced yet, so it could get confusing to readers who are not familiar with these measurements. I would recommend stating the justification for using the repeat measurements – i.e., their characteristics did not vary much through time, but their morphologic impact did – at the end of the last paragraph, and then moving the specifics of how they changed (or didn't change) over time to the results and/or discussion.

Lines 175-177: 'Left' and 'Right' side of the jam could be better defined, particularly for oblique jams where 'left' and 'right' are more nuanced. For channel-spanning jams or jams that extended beyond the channel, were measurements to the 'left' and 'right' of the jam made on the floodplain? Or were all measurements made within the channel? Perhaps a couple of photos with examples of measurement 'quadrants' could be added to the supplemental.

Lines 226 – 227: This is an interesting point that root wads may have such an important effect because they are attached to larger pieces, but it also seems that it's an interpretation of the results (albeit a side test!) that would fit better in the discussion. Although you didn't measure key pieces for the entire dataset, potentially this applies to the root wads that you measured in the rest of the dataset (i.e., those are also indicative of key pieces)?

Line 291: Given that obstruction index is referenced by number here, I think it would be beneficial to include a brief description of the index levels (1-3) from Scott et al. (2019) in the methods.

Sections 5.3 and 5.4. Predictors for the presence of forced bars appear to be discussed in both sections: first, alone in Section 5.3., and then as a component of sediment retention in Section 5.4. Could you clarify whether you are discussing the presence of the same bars in both sections, and if so, could you add an explanation for why you predict forced bar presence twice?

Lines 358 – 361: In Figure 2A, about a third of the points (jams) fall above the one-to-one line, which does not seem to support the claim that jam length often limits the maximum lateral morphologic impact length. In the next sentence, it appears that maximum length of impact does not correlate to jam length. If so, please clarify or add additional justification for this statement.

Line 380: Do you mean, "high [to] moderate accuracy.."?





Lines 423 – 425: What about stream power or slope as a control on sediment retention / transport capacity? Although confinement often correlates with stream gradient, there is variability within that relationship. Consider discussing stream power or gradient as a potential control on sediment retention and reach-averaged bed surface texture here.





Recommendation by Associate Editor 2 (Rocío Luz Fernández)

Completed: 2024-07-15 08:35 PM

Recommendation: Major Revisions

Dear author,

I have looked with interest at your manuscript entitled “Wood jam characteristics influence but do not fully explain wood jam morphologic functions”, added to the reviews we received from two anonymous reviewers. On this basis, I would like you to undertake a major revision of your manuscript focusing on clarity and completeness. Particularly, I agree with the reviewers' recommendations on the need for clarity in methods and statistical analysis, along with additional visual aids.

In addition, do you think that employing more advanced statistical techniques than the ones used here, or time series analysis (for the case that it exists data collected over time) could help in better capturing the complex relationships between wood jam characteristics and morphologic effects? This relates to your observations written within the discussion that wood jam characteristics, when taken in the context of channel characteristics, cannot predict wood jam morphologic effects. As this observation is contrary to your hypothesis (Abstract), please consider addressing this point more entirely.

Overall, the manuscript presents valuable research on the role of wood jams in shaping river morphology.

Best wishes,

Anonymous Associate Editor





Author's responses to review comments

2024-08-28

Response to Reviewers

I appreciate the opportunity to revise this manuscript and the helpful comments from both of the reviewers and the editor. I have revised this manuscript according to these comments and feel that it is considerably improved. My responses to these comments are shown below, associated with each comment, in *italics*. Please also see the tracked changes version of the revised manuscript.

Editor:

I have looked with interest at your manuscript entitled “Wood jam characteristics influence but do not fully explain wood jam morphologic functions”, added to the reviews we received from two anonymous reviewers. On this basis, I would like you to undertake a major revision of your manuscript focusing on clarity and completeness. Particularly, I agree with the reviewers' recommendations on the need for clarity in methods and statistical analysis, along with additional visual aids.

Thank you for facilitating this review and providing your comments. I have responded to the reviewer recommendations below and made changes to the manuscript accordingly, focusing on clarifying the statistics and adding visual aids.

In addition, do you think that employing more advanced statistical techniques than the ones used here, or time series analysis (for the case that it exists data collected over time) could help in better capturing the complex relationships between wood jam characteristics and morphologic effects? This relates to your observations written within the discussion that wood jam characteristics, when taken in the context of channel characteristics, cannot predict wood jam morphologic effects. As this observation is contrary to your hypothesis (Abstract), please consider addressing this point more entirely.

I explored the use of time series analysis for this paper but found that the degree of change through time was insufficient to support that type of analysis. In response to Reviewer 2's line comment on lines 142-153, I moved a section from the methods noting the infeasibility of analysis over time to a new section in the results, which details why I was unable to do the time series analysis I had originally set out to do (namely, my predictor variables did not sufficiently change through time across enough jams to allow a sufficient sample size for such an analysis). By placing this explanation in the results, instead of the methods, I hope it clarifies the lack of time series analysis and justifies the statistical analysis that currently exists in the paper.

Overall, the manuscript presents valuable research on the role of wood jams in shaping river morphology.





Reviewer 1:

the reviewed paper deals with the prediction of the geomorphic effects of wood jams, assessing several parameters of jams with respect to their geomorphic potential. This topic is interesting not only to scientists studying biogeomorphic interactions in rivers, but also to practitioners involved in designing river restoration projects. I appreciate the extensive dataset of wood jams (>700 observations, although some were repeated - but still independent), which provides a strong basis for assessing this problem. The paper is quite long but easy to follow: I appreciate the tables in the results section with clear explanations of the multiple regression outcomes. I have no objections to the methodology used or the interpretation of the results. The conclusion that the design and monitoring of river restorations should focus on a series of jams rather than the geomorphic impact of a single jam (i.e., it is difficult to predict the morphological impact of a single jam, and it is better to account for the variability of these impacts across the whole river reach) is an important message for management.

Thank you – I was especially nervous about the length, and I'm glad to hear it was still readable.

I have only two points that may be included or emphasized in the discussion.

(i) I fully agree with the statement that several factors, such as sediment supply and transport capacity, are important for the resulting morphological impact of a jam but are difficult to quantify (also considering variations in hydrology and the evolutionary trajectory of the river). In this regard, the author examined most of the jams in NW USA with a specific type of riparian tree as a source of large wood. Does the author have any idea if there would be differences in the "intensity" of geomorphic impact of jams (based on different hydrological regimes, tree recruitment, wood decay, and jam longevity) in other bioclimatic regions? There are some suggestions regarding various calibers of bed sediments (which I agree with), but other aspects could be briefly discussed to highlight the global potential of your obtained results (and perhaps express some knowledge gaps).

Good point – I think the main two factors that will vary across bioclimatic region and affect these results would be sediment size (namely, sand-bed rivers probably behave differently) and wood species. Otherwise, these sites cover a large range in hydrologic regimes, wood decay rates, and recruitment styles/rates). I added two sentences to the discussion to note this, one in section 6.1 paragraph 2 (noting the importance of tree species and noting the two main families of trees covered in this dataset), and the second in section 6.1 paragraph 4.

(ii) From the perspective of river restorations involving artificial wood supply, your reported statistics suggests that adding "complex" large wood with attached root wads or branches can enhance the geomorphic effect of a jam. Am I right? I think this issue could be emphasized in section 5.2.

Generally, yes. Because that interpretation (that rootwads and branching can lead to more likely and higher magnitude morphologic effects) comes from a synthesis of multiple results sections, I added emphasis, as suggested, to the discussion in paragraph 2 of section 6.2, which deals specifically with artificial wood placement.





Additionally, I have a minor issue: I am not sure about the size of the bed sediments, but I suppose this research was done in gravel-bed rivers. Please add some details somewhere in lines 104-110, because I think the morphological effect of wood jams can also be studied in meandering or anastomosing rivers with finer sediments (and this effect can be slightly different when compared to non-cohesive gravel particles).

Good point. Yes, these were largely gravel bed rivers. I added a clause to this paragraph to note this.

Congratulations on this great work!

Reviewer 2:

The following is a review for the manuscript titled, 'Wood jam characteristics influence but do not fully explain wood jam morphologic functions'. Here, the author presents a dataset of wood jam characteristics and impacts (including the presence and magnitude of jam-induced features) for a wide range of field sites in the Rocky Mountains and Pacific Northwest. The manuscript is well-written and provides insightful results into the useful (or not so useful) features of wood jams as well as thoughtful and timely discussion on the scales at which we monitor and restore wood in rivers. However, parts of the manuscript could benefit from additional clarity, particularly given the intricacies of the dataset and discussion. Overall, I feel that the submitted work would be a good contribution to Geomorphica following major revisions. See below for more detailed comments.

Thank you. I struggled to make this large dataset and analysis readable, so I especially appreciate the help with clarity here.

General comments:

The paper presents an impressive dataset, particularly for a field-based study. As you mention throughout the methods, there are some intricacies to this dataset, including variations in what metrics were measured and where. Additionally, from lines 166-167, it seems that you might have measured all the WooDDAM metrics at some sites, but it seems that certain metrics (e.g., decay class) are not mentioned in the paper. To improve clarity, I would recommend adding a table with the metrics you measured, a brief description of those metrics, and a column with the sites where those measurements were made. Also, even if all the WooDDAM variables were measured at some sites, I encourage you to focus on only the variables used in your full models, and then to also describe which variables (if any) were not important in the selected models.

Good point. Coming back to this, I took a solid half hour to figure out how I split my dataset and metrics for statistical analysis. That's a good indication that I needed to be clearer on that point! I first reorganized the methods section a bit to note up front that only some variables were measured for a subset ($n = 301$) of data on the Cedar River, which I now refer to, for clarity, as the "Cedar subset" of the dataset. I then added Table 1, which defines the wood jam characteristics variables (mostly from WooDDAM) measured in this study, as well as whether those characteristics were measured for the Cedar subset or not, and how that affects whether they were included in statistical evaluation. Then, for each model table in the results, I note under the sample size whether it includes or excludes the Cedar subset. All the intricacies to the dataset (the





problem with conducting a field study over so many years and trying to include all the data you possibly can in the analysis) still make for a pretty confusing situation, but hopefully this makes it a bit clearer.

Overall, there are very few photos of the types of jams and jam-induced features you were measuring. An additional figure with visual examples of the jam-induced features (i.e., scour pools, forced bars, cut banks, and sediment wedges) could be helpful.

I added Figure 3 to the methods, which shows examples of the four types of morphologic effects surveyed in this study.

Finally, regarding the statistical models, you provide a good justification for why you present models using different sample sizes (i.e., you want to have the largest sample size possible for a given model). However, this becomes somewhat confusing when you present two different models of the same response variable and discuss them as one in the results. For example, you list jam porosity, blockage ratio, root wads, and substrate as important predictors for scour pool presence in Lines 293-294. However, root wads were also presumably included as a predictor in the full model for the smaller dataset but did not emerge as a significant predictor in the final model, which could leave one wondering about the true importance of root wads. This comes up again while predicting forced bar presence; alone, forced bars are only predicted by root wads, but when combined with sediment wedges, a larger set of predictors emerges. Additional reporting and discussion on the similarities and differences between models predicting the same outcome would be beneficial both in the results and discussion sections. Additionally, plots (scatterplots or boxplots, depending on the predictor) showing these correlations while highlighting the different datasets could be informative.

I agree that this is confusing. To help clarify which data were included in each model, I added a note with the sample size in each model results table noting whether it includes or excludes the Cedar subset (see response to previous comment and revisions to methods related to this).

The discrepancy for scour pool presence is a good point. I failed to note that in the results. I added a sentence stating that this lack of rootwad emergence as a significant predictor in the smaller model may indicate that the signal of rootwad importance is more variable than other predictors (i.e., requires a larger sample size to detect due to a lower signal to noise ratio), or may just be really important on the Cedar subset. This also applies to porosity in the pool depth models (and I also noted that in the results paragraph for that response).

The discrepancy in variables for sediment retention vs bar presence is more difficult to explain, as the response variable is inherently different (one is just for whether the jam retained a bar, and one is for whether the jam retained either a bar or a sediment wedge). My guess is that the greater set of variables emerges for sediment retention due primarily to the response being more general for that model compared to the model of forced bar presence. Partially in response to the line comment in sections 5.3 and 5.4 below (now 5.4 and 5.5), I added a sentence in what is now section 5.5 explaining this and noting that this may be the cause of a greater number of significant predictor variables emerging in the sediment retention model.





As for the univariate plots, the only response variables for which plots could be made are scour pool depth and forced bar height. I added Figure 4 showing the univariate relationships between each predictor and scour pool depth and Figure 5 showing similar relationships for forced bar height.

Line-by-Line comments:

Lines 104- 107: What about channel planform? Could you give more information on whether these are all single threaded reaches or if some are multi-threaded. Based on the map (and the reach characteristics), it seems that the reaches may be spanning process domains as well, particularly in Washington. Consider more discussion on this.

Planform varied significantly. Each river itself was in a single process domain (i.e., sampled sites on a single river never crossed process domains, at least in my opinion). However, planform varied significantly. I added a sentence to the mentioned paragraph noting this.

Lines 142 – 153: These sentences are well-written are interesting, but this analysis feels like a tangent, and it involves results and discussion on metrics that haven't been introduced yet, so it could get confusing to readers who are not familiar with these measurements. I would recommend stating the justification for using the repeat measurements – i.e., their characteristics did not vary much through time, but their morphologic impact did – at the end of the last paragraph, and then moving the specifics of how they changed (or didn't change) over time to the results and/or discussion.

Thanks for weighing in on this. I was split when writing about whether to put this in methods or results, so I appreciate you settling that debate for me. I moved this whole paragraph to a new section at the beginning of the results, and added a small parenthetical to the paragraph above just to explain to readers why repeat measurements were done in the first place.

Lines 175-177: 'Left' and 'Right' side of the jam could be better defined, particularly for oblique jams where 'left' and 'right' are more nuanced. For channel-spanning jams or jams that extended beyond the channel, were measurements to the 'left' and 'right' of the jam made on the floodplain? Or were all measurements made within the channel? Perhaps a couple of photos with examples of measurement 'quadrants' could be added to the supplemental.

I added some details here as suggested. Morphologic effects on top of floodplains were not measured, as I was focused on in-channel morphologic effects. I added a drone photo showing three jams, their quadrants, and the morphologic effects in each quadrant to the methods to help clarify this. This figure shows both channel spanning and non-channel spanning jams, as well as an oblique jam.

Lines 226 – 227: This is an interesting point that root wads may have such an important effect because they are attached to larger pieces, but it also seems that it's an interpretation of the results (albeit a side test!) that would fit better in the discussion. Although you didn't measure key pieces for the entire dataset, potentially this applies to the root wads that you measured in the rest of the dataset (i.e., those are also indicative of key pieces)?

Good point – I moved this to the discussion.





Line 291: Given that obstruction index is referenced by number here, I think it would be beneficial to include a brief description of the index levels (1-3) from Scott et al. (2019) in the methods.

Please see response to comment about detailing the metrics measured here in more detail above. I added this description to table 1.

Sections 5.3 and 5.4. Predictors for the presence of forced bars appear to be discussed in both sections: first, alone in Section 5.3., and then as a component of sediment retention in Section 5.4. Could you clarify whether you are discussing the presence of the same bars in both sections, and if so, could you add an explanation for why you predict forced bar presence twice?

I added a sentence to section 5.4 (now 5.5) explaining this. Jams that forced only a sediment wedge, but not a bar, would have a positive response for this model, but not for the logistic model predicting forced bar presence, whereas jams that only forced a bar or forced a bar and a sediment wedge would have a positive response for both. Essentially, this model relaxes the criteria for a jam retaining sediment to include sediment wedges, and is thus a more general case of sediment retention than bar forcing, which is a very specific hydraulic and sediment deposition effect.

Lines 358 – 361: In Figure 2A, about a third of the points (jams) fall above the one-to-one line, which does not seem to support the claim that jam length often limits the maximum lateral morphologic impact length. In the next sentence, it appears that maximum length of impact does not correlate to jam length. If so, please clarify or add additional justification for this statement.

*I would actually argue that it does support that argument, but specifically, that it **often** does – 70% of observations, in this dataset. I edited this statement to be clearer about how often and to align better with the data presented.*

Line 380: Do you mean, “high [to] moderate accuracy..”?

Yes – thank you for catching this.

Lines 423 – 425: What about stream power or slope as a control on sediment retention / transport capacity? Although confinement often correlates with stream gradient, there is variability within that relationship. Consider discussing stream power or gradient as a potential control on sediment retention and reach-averaged bed surface texture here.

Excellent point – I edited this to note that slope, bankfull width, and confinement all did not correlate with sediment retention. I didn't measure drainage area (as a proxy for discharge to compute unit stream power), but I figure that if slope and width don't correlate, it's unlikely that stream power would.





Round 2 Review

Comments by reviewer 1 (Tomáš Galia)

Completed: 2024-09-06 02:46 PM

Recommendation: Accept

Dear Author,

In my opinion, you have incorporated all of my (rather minor) comments into the new version of the manuscript, which has improved the clarity of related statements. Therefore, I have no further recommendations from my side and I will be happy to see your manuscript in press.

Congratulations on your work!

Best regards!





Comments by reviewer 2 (Julianne Scamardo)

Completed: 2024-09-20 06:20 PM

Recommendation: Minor Revisions

I feel that the author of the manuscript – ‘Wood jam characteristics influence do not fully explain wood jam morphologic functions’ – has adequately addressed my previous comments on the paper. While reviewing the revised manuscript, I found a few places that I think could use additional clarity. However, following these minor revisions, I feel that the manuscript will be a good addition to the geomorphic literature on large wood. I look forward to seeing it published!

Line 29-30. The terms riverine landscape and riverscape might be considered jargon. Just as a note, ‘channel’ is typically not considered jargon, and you could use that throughout.

Line 140-142. Did you determine morphologic effects during field surveys or by using drone imagery (like shown in Figure 2)? I’m assuming – based on the fact that scour pool depth and bar height were measured – that this was done in the field but given that some measurements were made from imagery (like blockage ratio), it would be helpful to clarify.

Line 360-361. It’s unclear to me whether this sentence is saying that there are no significant correlations between lateral morphology effect length and jam characteristics excluding jam length or including jam length. If there is not a significant correlation between jam length and lateral morphologic effect length, consider removing the trend line in Figure 6A.

Section 5.6. Figure 5 is referenced throughout this section, but I think you are referring to Figure 6.

Figure 7. The blue outline and arrow are a little difficult to see against the predominantly blue-grey tones of the channel. Consider using a higher contrast color like orange or yellow to highlight the scour pool and flow direction.

Line 462. It looks like part of a sentence has been cut off or not fully deleted here.





Recommendation by Associate Editor 2 (Rocío Luz Fernández)

Completed: 2024-09-26 06:33 PM

Recommendation: Minor revisions

Dear Authors,

I read with interest your manuscript entitled "Wood jam characteristics influence but do not fully explain wood jam morphologic functions", along with the feedback from the anonymous reviewers. You have successfully incorporated most of the reviewers' suggestions, particularly improving clarity in sections such as the methodology and results. Overall, the manuscript is in strong shape, and I would like to recommend it be accepted after minor revisions.

Based on the final reviewer's comments, I suggest that if you wish to retain terms like "riverine landscape," it would be helpful to provide a brief definition when these terms first appear. This will help avoid confusion and improve accessibility for readers who may not be familiar with the specific terminology. Also, there still seems to be some ambiguity about whether the morphologic effects were determined through field surveys or drone imagery. Clarifying which measurements were obtained by which method (as was done for blockage ratio) would improve transparency. Lastly, I recommend a final read-through to ensure consistency in terminology, figure references, and methodological clarity. If possible, please enhance the visual contrast in Figure 7 (particularly for the arrows and outlines) to improve readability in print.

Best regards,

Anonymous Associate Editor





Author's responses to review comments

2024-10-06

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

Editor:

I read with interest your manuscript entitled "**Wood jam characteristics influence but do not fully explain wood jam morphologic functions**", along with the feedback from the anonymous reviewers. You have successfully incorporated most of the reviewers' suggestions, particularly improving clarity in sections such as the methodology and results. Overall, the manuscript is in strong shape, and I would like to recommend it **be accepted after minor revisions**.

Based on the final reviewer's comments, I suggest that if you wish to retain terms like "riverine landscape," it would be helpful to provide a brief definition when these terms first appear. This will help avoid confusion and improve accessibility for readers who may not be familiar with the specific terminology.

Please see my response to this comment from Reviewer 2 below.

Also, there still seems to be some ambiguity about whether the morphologic effects were determined through field surveys or drone imagery. Clarifying which measurements were obtained by which method (as was done for blockage ratio) would improve transparency.

All morphologic effects were measured in the field, never using any sort of remote sensing imagery. I clarified this in the two paragraphs detailing the morphologic effects measurements.

Lastly, I recommend a final read-through to ensure consistency in terminology, figure references, and methodological clarity. If possible, please enhance the visual contrast in Figure 7 (particularly for the arrows and outlines) to improve readability in print.

I have increased the line width, reduced line transparency, and increased line contrast with the background image in Figure 6 to improve visual contrast.

I have done a thorough final read-through and made corrections throughout the manuscript.

Reviewer 1:

In my opinion, you have incorporated all of my (rather minor) comments into the new version of the manuscript, which has improved the clarity of related statements. Therefore, I have no further recommendations from my side and I will be happy to see your manuscript in press.

Thank you.





Reviewer 2:

I feel that the author of the manuscript – ‘Wood jam characteristics influence do not fully explain wood jam morphologic functions’ – has adequately addressed my previous comments on the paper. While reviewing the revised manuscript, I found a few places that I think could use additional clarity. However, following these minor revisions, I feel that the manuscript will be a good addition to the geomorphic literature on large wood. I look forward to seeing it published!

Line 29-30. The terms riverine landscape and riverscape might be considered jargon. Just as a note, ‘channel’ is typically not considered jargon, and you could use that throughout.

I'm unsure how "riverine landscape" can be misconstrued, as I am using it in its normal definition and neither word is jargon in and of itself. "Riverine" simply means relating to, formed by, or resembling a river. Thus, "riverine landscape" must be the landscape relating to, formed by, or resembling a river. Riverscape is simply a shortening of riverine landscape. While "riverscape" might be new to some geomorphologists, I'd prefer to define riverscape as a "riverine landscape." As such, I did not replace or define instances of "riverine landscape" and used that phrase to define "riverscape."

Line 140-142. Did you determine morphologic effects during field surveys or by using drone imagery (like shown in Figure 2)? I'm assuming – based on the fact that scour pool depth and bar height were measured – that this was done in the field but given that some measurements were made from imagery (like blockage ratio), it would be helpful to clarify.

All morphologic effects were measured during field surveys, not using imagery. I've clarified this in the methods.

Line 360-361. It's unclear to me whether this sentence is saying that there are no significant correlations between lateral morphology effect length and jam characteristics *excluding* jam length or *including* jam length. If there is not a significant correlation between jam length and lateral morphologic effect length, consider removing the trend line in Figure 6A.

I think the line numbers this comment references should actually be 419 – 421. I clarified that the correlation is with jam length, and not other characteristics.

Section 5.6. Figure 5 is referenced throughout this section, but I think you are referring to Figure 6.

Corrected this.

Figure 7. The blue outline and arrow are a little difficult to see against the predominantly blue-grey tones of the channel. Consider using a higher contrast color like orange or yellow to highlight the scour pool and flow direction.

I enhanced the contrast of this figure.

Line 462. It looks like part of a sentence has been cut off or not fully deleted here.

I finished the partial sentence at the end of section 6.1.

