Reply to Anonymous Referee #1

The manuscript describes an evaluation of a stream restoration project by large wood introduction in three gravel bed streams in the US. A 2D hydrodynamic model is applied, which had been calibrated with field observations. The calibrated model is then applied to study the habitat suitability for a juvenile salmonid species at bankfull discharge. Large wood increases the size of suitable habitat in all three field sites.
I read this manuscript with a lot of interest. I think the subject is very relevant because large wood introduction is a cost-effective stream restoration method, with a lot of benefits for stream ecology. In general, the manuscript is well written and the figures are well prepared. The Introduction contains most relevant information, the methods are clearly described and the results are well presented, as well.
The main critic I have is that the authors only focus on a single discharge (i.e. bankfull) when presenting the results, while it might not be too difficult to extend the results with other relevant discharge classes as well. When I was reading the Introduction, I had the feeling the authors would go in that direction. On page 2 (lines 22-24) the authors argue that there is a lack of understanding of the effect of large wood on flow conditions under a range of discharges. So why are only results shown for bankfull discharge conditions and not for other conditions? As far as I understand it well, the model was calibrated for several discharge levels (Table 2). So the model calibration would not put limitations for model application at other than bankfull discharge conditions. Furthermore, in this age of abundant computational resources, I would never argue that additional model runs are not possible because of computational costs. Hence, I suggest to extend the results with other relevant discharge conditions to increase the implications of large wood introduction on habitat suitability for the Coho Salmon.

Reply: We appreciate this comment, which was also raised by the other reviewer. Given the Nays2D is unsteady we actually run the model for 35–45 hour long hydrographs that peaked around bankfull but included a wide range of flows in all sites. We made this clearer in the methods (P5, L17–20; P7, 28–30). Based on these simulations we now include a section in the results highlighting changes in simulated habitat availability before and after the addition of LW during the whole hydrograph duration (see section 3.3., Figure 7 and Table 3).

Overall, I think this manuscript has potential to be a valuable addition to the literature, but some works is still required to make it acceptable for publication. Below I have provided general and specific comments to the text.

Reply: We really appreciate your careful review.

General comments

The Introduction is mainly focused on the effect of large wood on streams in the Pacific Northwest (US). In Europe (and most likely also in other continents) wood is also used in stream restoration, which deserves some attention as well. I suggest to at least add some references to studies where wood is used, not only to improve the habitat conditions for fish, but also to improve conditions for macroinvertebrates.

Reply: We appreciate the suggestion. We added a paper about wood in European rivers (Kail, 2003) and two papers about the importance of wood for macroinvertebrates (Gerhard & Reich, 2000; Jahnig & Lorenz, 2008) (P1, L24; P1, L30; P2, L1).

Throughout the manuscript the authors use v and τ to refer to velocity and stress. Sometimes this results in sentences like "...depth-averaged flow v and shear τ ..." (Page 12, line 9), which may be difficult to read for readers without much knowledge in hydraulics. Therefore, I suggest to write "velocity" and "stress" in full where possible.

Reply: We agree. We eliminated most of the "v" and "t" to improve readability throughout the text.

Specific comments

• Page 2, line 34: From "Our objective...". I suggest to start a new paragraph here and first summarize in 1-2 sentences the main limitations of previous research, followed by the objective.

Reply: As suggested, we added a new paragraph clearly stating the limitation of previous efforts before stating our objective (P3, L 11–16).

• Page 3, line 12: It is more common to characterize annual precipitation sum in mm, than in cm.

Reply: Done (P3, L24).

• Page 4, lines 9-11: How was the discharge for the depth-discharge rating curves determined? Through measurements or modelling? Please clarify in the text.

Reply: We added information about how we developed the stage discharge relations: Discharge was measured using the velocity-area method (Dingman, 2002) using a Hack FH950 Portable Velocity meter and depth-discharge rating curves were developed based on 9-10 discharge measurements per site (P4, L13–14).

• Page 6, line 32: How were these flow velocity measurements performed? This is not mentioned in the text, please clarify.

Reply: We have clarified in the text that these 13–24 velocity measurements per site were taken across the stream (Figure 2, Table 2) for 2-3 flow levels (P7, L22).

• Page 8, lines 5-6: The authors mean that the velocity distribution was more homogeneous before LW introduction and more heterogeneous after LW introduction? Please clarify in the text.

Reply: We have added some text clarifying that the velocity distributions were more homogenous before the LW additions (P8, L26).

• Page 9, lines 4-6: I suggest to show the percentage increase or decrease, which is more consistent with the previous sentences.

Reply: The suggested changed was implemented (P9, L15–16).

• Page 10, lines 13-14: The authors refer to Fig. 6, but the spatial changes are shown in Fig. 5.

Reply: Yes, you are correct, thank you (P11, L8)

• Page 10, lines 14-17: These sentences are somewhat confusing. The authors are referring to a number of observations, but do you mean simulation results? Also, the results do depend on the chosen transport threshold, hence, the word "independent" should be "dependent", right? I also would not use the term "significant" in this context, since most readers associate it with statistical significance. In general, the authors are discussing the results here, maybe better to move this to the Discussion section.

Reply: We agree that these sentences do not belong in the results section. We decided to eliminate them as they do not add much to our findings.

• Page 11, lines 8-11: The fitted gamma parameter values are not shown. I suggest to add these values to each of the panels of Fig. 6.

Reply: We appreciated the suggestion. The values have been added to the figure.

• Page 12, line 7: Please add "in" between "increases" and "the heterogeneity".

Reply: Done (P14, 4).

• Page 14, lines 2-5: The authors refer here to "small reaches", do you mean "narrow"? Please clarify in the text.

Reply: We mean small not only in the sense of narrow but also smaller in terms of having less drainage area and thus less discharge. We have clarified this in the text (P16, L21 –22).

Figures and Tables

• Figure 1: I suggest to use some colors to indicate the wood and WSE rulers. Or maybe use a solid black line for the wood, instead of the pattern fill.

Reply: We believe you are referring to figure 2 here. We changed the color of the Wood pieces as suggested.

References mentioned in the reply.

Dingman, S. L. (2002). *Physical hydrology*. Upper Saddle River, N.J.: Prentice Hall.

Gerhard, M., & Reich, M. (2000). Restoration of streams with large wood: Effects of accumulated and built-in wood on channel morphology, habitat diversity and aquatic fauna. *International Review of Hydrobiology, 85*(1), 123-137. doi:10.1002/(sici)1522-2632(200003)85:1<123::aid-iroh123>3.3.co;2-k

Jahnig, S. C., & Lorenz, A. W. (2008). Substrate-specific macroinvertebrate diversity patterns following stream restoration. *Aquatic Sciences, 70*(3), 292-303. doi:10.1007/s00027-008-8042-0

Kail, J. (2003). Influence of large woody debris on the morphology of six central European streams. *Geomorphology, 51*(1), 207-223. doi:https://doi.org/10.1016/S0169-555X(02)00337-9