Review of "Open check dams and large wood: head losses and release conditions"

Summary:

This paper presents experimental investigation and a two-box empirical model describing the relative head loss due to wood jam formation at 4 types of check dams, compared to tests conducted with water only. Large wood (LW) pieces were fed to the check dam as the flow was progressively increased in several pseudosteady steps, creating a progressively larger jam that eventually failed. The water surface height upstream of the jam was measured 0.2 m upstream of the check dam upstream edge. As mentioned by other reviewers, the empirical coefficients related to LW show a high degree of variation, which the authors relate to variations in wood accumulation. The conditions leading to jam failure and the ratio of buoyancy to drag force are explored.

The experiments conducted measured a time-varying wood accumulation at four types of check dams involving several different wood mixtures. With multiple aspects in place, it is easy for the manuscript to become unwieldy, and I understand that a previous round of revision has improved the clarity of the manuscript. Additional clarity and organization in the manuscript and figures would help develop the authors' points and emphasize their findings to the reader.

Line-by-line comments:

Line 13: I agree with comments made by previous reviewers [Reviewer Comment 21]:

> *Reviewer Comment: 21. L82/84: The experiments were conducted without sediment. I*
>
> *recommend to either remove the sentences regarding sediment transport or add information on how to derive effect on sediment transport and elaborate more in detail how flow above the structure affects sediment transport*
>
> *Response: The following mention will be added in the revised paper after the mention of sediment. "(see Piton and Recking, 2016a, on this question)."*

Sediment was not investigated in this study, so it is rather confusing to see it listed in the abstract as what appears to be the first research question. Given that the study is focused on LW, please keep the focus in the abstract and elsewhere on the questions directly investigated.

Line 18-19: Was the depth of water above the dam spillway directly measured in this study? I understood from the Methods that the height of the water surface was measured 0.2m upstream of the dam by UDS (Line 155). Please clarify.

Lines 136-139: This becomes a bit difficult to follow, especially for a what is really a very minor point. The authors first state that their experiments are not scaled to any field site, but then provide a scaling to a field case study with a ratio of 1:34. They then go on to use a scale ratio of 1:33 when providing scaled measurements for the logs used (Lines 190-192), which are said to be comparable to another field study. Is this a typo or is there a reason for the slight change? Why does "scale ratio in the range 1:20-1:60 remain relevant?" Is this a physical scaling limitation (e.g, the hydrodynamics measured in the experiments are not applicable outside of this range), or do most river channels of interest fall within the range of 1:20-1:60? It is all right if several field studies were used to guide the choice of experimental parameters, but the description should be more straightforward.

Line 158: "the additional head loss related to LW accumulated further upstream of the sensors was not studied"—it would be useful to know for which experiments upstream accumulation of LW was observed. Would this have contributed to the variation observed in Section 4.2?

Lines 163-167: The equivalent return period of the scaled (1:34) peak discharge is not given directly but is said to be much greater than a 100 year return period flood. Is 8.5 L/s equivalent to a 1000 y flood, as is implied parenthetically (Line 167)? This section would be easier to interpret if the range of return periods tested was first stated directly, and then related to the range of event types.

Line 195: “somehow equivalent”—this phrase suggests that you do not understand why pine needles would be equivalent to twigs when scaled to real-world dimensions, I am sure this is not the case. Please delete “somehow” and if possible give the scaled dimensions of the needles, ex. ‘that are equivalent to twigs (d=, L=)’ to support this point, similar to that provided for the logs in Line 190.

Line 216: Given that additional wood was progressively introduced to each dam, was the amount of wood added with discharge kept constant between tests?

Figure 4: I do not find this figure helpful and feel that the text description (Lines 237-242) is sufficient.

Line 264: Do you mean Schalko et al. (2019a)?

Lines 267-268: “i.e, when the flow depth approached or exceeded the LW diameter.” Elsewhere (Abstract; Line 355) it is said that LW releases only occurred when flow depth exceeded 3-5 wood diameters. Please clarify.

Line 303, 314, 325,339: Please explain the quantitative criteria used to define the “satisfying” lower and upper bounds.

Figures 6-9: “each color shade corresponds to a different run”—were these runs effectively repeats of the same discharge and LW volume, as seems to be described in Lines 211-213? This could be mentioned again in the caption or the text to support your point that the large variation is due to variations in LW accumulation, as the conditions for each run are not immediately clear.

Figure 10: The difference between the brown and black marks is hard to make out. Please use colors with a higher contrast. Further, I understood from your previous description (Line 281) that all of the wood was eventually released; however, Figure 10 shows that this never occurs. Please clarify.

Line 315: typo (slightly)

Figure 11: The comparison of h^* to Π/F_D is interesting, demonstrating the higher backwater rise above the spillway observed for conditions likely to generate large interlocked jams. If possible I would find an inset photo more helpful rather than the plot with linear x-axis shown. I do not understand why the given trendline fit, marker transparency and size were linked to % of wood released. This confuses the meaning of the figure, especially since a similar trend exists for smaller release % (faint pink triangles in Quadrant II). I would find this figure easier to interpret if all symbols had an equal size and opacity.

Line 470-478: The effect of fine material in wood accumulations has been shown to be related to the projected area of the material (Follett et al. 2020), not necessarily its ability to percolate through the accumulation. The pine needles used in this study would have a very small projected area and therefore would have less effect on the observed head loss than the materials tested by Schalko et al. (2018).