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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Open check dams and large wood: head losses and release conditions" *by* Guillaume Piton et al.

Guillaume Piton et al.

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Reviewer Comment: 1. The comments of Reviewer #1 are written after "Reviewer Comment"

The responses by the authors follow in normal style.

Reviewer Comment: 2. General comments Reviewer Comment: 3. The authors present an interesting paper on the effect of large wood (LW) at various open check dams on hydraulic conditions. Based on an extensive data set, the authors describe resulting backwater rise due to LW blockage at check dams and analyze the process of LW overtopping the dam structure. From a flood hazard perspective, it is very important to determine when LW may pass the retention structure as this can increase flooding downstream. The authors introduce dimensionless parameters to 1) describe the physical process of LW overtopping and 2) inform engineers what relative overtopping flow depth results in LW overtopping. The paper fits very well to the scope of the Journal and provides new insights regarding the interaction between LW and hydraulic infrastructures.

The authors thank very much Reviewer #1 for his/her constructive comments and time spent in helping us to improve our work.

Reviewer Comment: 4. My general comments concern the description of the physical experiments, analysis of effect of LW characteristics, workflow to apply the "non-dimensional parameter describing the formation of a LW carpet", and the form (language) of the paper: The description of the experimental procedure should be improved. It is not clear to me how the authors added LW (L180 ff).

We will clarify how LW was added to the flume adding the sentences hereafter to the section describing the experimental protocol: "Logs were introduced manually at the upstream end of the flume, by groups of 5-15 logs, in an uncongested or semi-congested mode (sensu Ruiz-Villanueva et al. 2019). [. . .]. During each discharge step, we continuously checked that at least a couple of logs were recirculating and we introduced more when it was not the case."

We hope it is clearer.

Reviewer Comment: 5. A table of the test program should be added.

Good point. A table with all tests and data will be added to the supplemental data.

Reviewer Comment: 6. In addition, the authors refer to Piton et al. 2019b regarding the experiments. Please clarify the difference between the reference and this present study.

The report Piton et al. 2019b is the scientific report describing this experimental campaign. It was delivered to the French Ministry of Environement, that funded this study.

The report is written in French and has not been peer-reviewed. In essence, it is an extended pre-print of the present paper. Since several pictures and details of the experimental apparatus are provided in this report, we though fair and useful to mention its existence.

Reviewer Comment: 7. 2. The proposed computational steps to determine the effect of LW on stage-discharge relationship (beta1 and beta2) are easy to follow, but the resulting values exhibit large variations. The authors do propose that engineers calculate upper and lower boundaries, but recommendations on how to select a final value or how to proceed are missing.

This is a very good comment. A key lesson we learnt from this work is that LW jamming open check dam always trigger head losses, however this head losses varies in magnitude. Although previous work demonstrate that some parameters (e.g., presence of fine material) typically increase head losses, we observed wide variability in the beta coefficient values. We think that designers should acknowledge this variability and consider it.

In our opinion and experience, rather than trying to compute a mean value of the head losses, we recommend to use upper and lower bounds as "pessimistic" and "optimistic" scenarios. The challenge is to define which bound is pessimistic (or optimistic); actually, it is a matter of perspective. For instance, if one is intesrested in the design of the dam wings, the optimistic scenario is obviously the one with lower head losses and flow levels and the pessimistic scenario is the one with high head losses. On the contrary, when computing the sediment trapping capacity, the higher the head loss, the higher the deposition. Thus, the pessimistic scenario is the one with low water level and thus head losses. In essence, we recommend designers to consider two extremal scenarios rather than a mean behaviour, and to use each scenario whenever it is the conservative option as an assumption for further design steps.

We agree that this point was not described in the paper and we will add it in the discussion.

Reviewer Comment: 8. 3. The experiments were conducted for various LW dimensions. However, the effect of LW mixture or presence of organic fine material is not discussed. Due to the presence of organic fine material, the resulting backwater rise increases, as depicted in Figures 6-9. The paper would benefit from a short discussion on the effect of FM on backwater rise, as it also enables the comparison to previous studies with branches and leaves.

Indeed, presence of fine material was consistently demonstrated to be a key factor increasing head losses in previous studies. However, we did not observed consistently higher values of Beta coefficient in presence of pine needle. It was yet visible in Figure 12 but was not commented. We will discuss this point in the revised version of the paper.

Reviewer Comment: 9. 4. The authors introduce a dimensionless parameter describing when a LW carpet forms or when a more compact LW accumulation can be expected. I agree with the authors that the ratio of buoyancy to drag force has not been presented in that form yet. However, Schalko et al. (2019, Water Resources Research) state that “The initiation of a LW carpet formation corresponds to the state, where the buoyancy force is higher than the downward drag force.” The reference is included in this paper but the concept of the “characteristic LW volume generating the primary backwater rise prior to the formation of a LW carpet” is not discussed and no reference added when the ratio of the forces are presented. I recommend adding this reference, as it provides a great opportunity to compare the present analysis with other approaches.

Thank you for this suggestion. The many recent works by the ETH team clearly influenced us a lot. We fully agree that this concept of balance between drag force and buoyancy is both explicitly and implicitly described in the work by Schalko et al. and our contribution was merely to propose a dimensionless number to describe the concept.

We will revised the section to give proper credit where credit is due.

Reviewer Comment: 10. In addition, it should be added that the application of this concept (to identify how LW accumulates), required first to determine the resulting backwater rise and then insert this value to U in F_D; it would be interesting to discuss the limitations, as beta1 and beta2 exhibit large variations.

Good point. This is precisely why we address both the computation of head losses and the release conditions criteria (h* and Pi/F_D, both functions of h) in the same paper. The interconnection was stated in the discussion but we will try to make it clear earlier in the paper.

Reviewer Comment: 11. 5. The authors include a section regarding comparison to previous work with an interesting table. However, in the text the authors compare their results only to Schmocker and Hager. I recommend to either include more quantitative comparison or shorten the section.

Thank you for the suggestion. We will add comparison with other papers in the text and not limit them to the table.

Reviewer Comment: 12. 6. The paper is well-structured, and the majority of the figures are very informative. However, the paper is very difficult to read. I strongly recommend that the revised paper is proofread by a native speaker. Please also check consistency of terminology (see technical comments).

The revised paper version will be checked by a native speaker. Sorry for that.

Reviewer Comment: 13. Based on these general comments, I propose the paper needs major revision in content and form. I added more detailed comments below.

Thanks again for the very relevant comments and helpful suggestions.

Reviewer Comment: 14. Specific comments

Reviewer Comment: 15. Keywords

Reviewer Comment: 16. Recommendation: add driftwood (or replace woody debris using driftwood)

Ok, done.

Reviewer Comment: 17. Hyper-congested LW transport is defined as LW transport at the very front of a flood wave, where the amount of transported LW significantly exceeds the amount of water. As the type of transport is not discussed in this paper, I would recommend writing congested LW transport and also add this term in the text.

Well, hypercongested flow regime with "wetted front" are also described in Ruiz Villanueva et al. 2019 but we agree that the LW congestion regimes were not described in the paper and we will add them in the revised version.

Reviewer Comment: 18. Abstract Reviewer Comment: 19. The authors use the term "energy dissipation" in the abstract and also in the entire ms. I would recommend replacing this term with hydraulic losses, as energy dissipation in this context is very confusing.

Thank you for the suggestion. "Energy" will be replaced by "energy head" in the revised paper and "energy dissipation" by energy "energy head loss".

Reviewer Comment: 20. Introduction

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.

The following mention will be added in the revised paper after the mention of sediment. "(see Piton and Recking, 2016a, on this question)."

Reviewer Comment: 22. Computing open check dam discharge capacity

Reviewer Comment: 23. L95: The terminology of flow energy in m is not correct;

please use “energy head” (energy is confusing with [m] as units); in addition vertical height above datum is missing.

Agree, as mentioned before, we will correct this point. The level datum is located at the opening bottom, this will be mentioned in the revised version.

Reviewer Comment: 24. L98: The authors state a range of flow Froude number F between 0.01 and 0.3. F = 0.01 this is very small; is this a common value at check dams - in particular when the authors stated in L80 that the flow Froude number is expected to be larger at check-dams compared to reservoir dams. Please discuss.

Froude number with LW varies between 0.01 and 0.3. It was specifically low for the closed dam, which is quite similar to a dam reservoir structure. The bigger the opening, the higher the Froude number as shown in Figure 1.

FIGURE 1 HERE

Figure 1: Froude number with and without LW for each dam type

We know from other experiments with the same flume setting that the Froude number without structure would be close from 0.7 but the presence of open check dams tend to create a significant backwater and decrease of the Froude Number directly upstream.

In the revised version of the paper, the following mention will be added in the Material and methods section: “The mean value $\pm$ standard deviation of the Froude number was 0.04 $\pm$ 0.01, 0.06 $\pm$ 0.02, 0.1 $\pm$ 0.02 and 0.24 $\pm$ 0.08 for the closed, slit, slot and Sabo dam, respectively (see section 3.2 for dams’ features).”

Reviewer Comment: 25. Materials and Methods

Reviewer Comment: 26. Add more details on the experimental setup. Why did you choose the respective slope,

We will add the following sentence to the section: “This slope is relatively low but is commonly observed in bedload retention basin (Piton et al. 2015, p. 22). This slope is the order of magnitude of channel slopes in alluvial fan distal reaches, i.e., the slope used for the design of guiding channels that are increasingly used in open check dams (Schwindt et al., 2018, Piton et al., 2019c). In addition, since the open check dams triggered high backwater rise and subcritical flow regime (with and without wood), the bottom flume slope is of secondary importance: flow conditions are controlled by the open check dam."

Reviewer Comment: 27. what is the accuracy of the measurement devices?

This information will be added.

Reviewer Comment: 28. Regarding flow depth measurement: what if LW accumulated 20 cm upstream of the dam - how did you account for that?

Good remark. The paper focuses on the design of the barrier itself so we measured depth in its direct vicinity. However, we agree that if the floating carpet is huge, an additional head loss will occur further upstream and can be important to take into account for the design of side dykes for instance. This will be specified by adding the following sentences: "The water depth measured was thus representative of the flow conditions in the direct vicinity of the open check dam. The longitudinal additional head loss related to LW accumulating further upstream of the ultrasonic sensors was not studied, although it would be important to take it into account for the design of side embankments (see the approach proposed by Di Risio and Sammarco, 2019 on this point)."

Reviewer Comment: 29. Add here or in a subsequent section information regarding tested discharge, to what flood they correspond and why you tested those values.

Thanks for the suggestion. The following information will be added : "Water discharge was measured with an electronic flow meter (accuracy $\pm$0.01 l/s). It varied in the range 0-8.5 l/s, i.e., covering a wide range of discharge magnitude. This peak discharge of 8.5 l/s would then be equivalent to 54 m^3/s (using the scale ratio of 1:34), i.e., a discharge much higher than the Combe de Lancey 100 years return period peak discharge of 35 m^3/s. In essence, we intended to test not only project design events (sensu. Piton et al., 2019c), corresponding to 100-300 years return period events (5.5-7 l/s at model scale), but also safety check events (≈1000 years return period – 8.5 l/s at mode scale) to verify the structures' behaviour when experiencing events of higher magnitude."

Reviewer Comment: 30. L157: How did you choose the respective LW dimensions; please add quantitative information to the text instead of "twofold greater number of elements".

We will add this information in the revised paper : "LW logs are equivalent to logs with length of 1.6-6.6 m at scale ratio 1:33, i.e., not extremely long logs that are prone to be released over the dam. The distribution of sizes was arbitrarily decided based on field measurements obtain by the second author on his case study of Horiguchi et al. (2015).

Reviewer Comment: 31. L161: Regarding the fine material: how much organic fine material did you add, why did you choose pine needles, I assume this is very difficult to collect at the end; if you upscale pine needles using a scale factor of 30 it represents rather twigs.

Very relevant remark. We will add the following remark in the revised paper : "mixtures labelled "B" also included fine material, here fresh pine tree needles, that are somehow equivalent to twigs at real scale. The fine material mass was typically of 5-10% of the cumulated log mass. We did not include a model equivalent of leaves as Schalko et al. (2018, 2019a). Such material would have percolate through the LW jams and densify it, thus increasing in some extent the head losses (see discussion at section 5.1 on this topic)."

Although we agree that we were not on the conservative side on this topic, we believe that it has only a side effect since the relative head loss we measured are consistent with the results of Schalko et al. (2018, 2019a) who address this topic in much more detailed way. This will be discussed at section 5.1.

Reviewer Comment: 32. L167: In addition to the authors' experience, please include references to clogged LW volume at structures during previous floods or refer to previous flume experiments.

We will rephrase the sentence as follow : "Such amount of LW is typically found in open check dams after strong flood event (see e.g., data compiled by Piton, 2016, p. 66) and is sufficient to strongly disturb open check dam functioning (Shima et al., 2015, Tateishi et al., 2020)."

Reviewer Comment: 33. L189: See general comment regarding reference to Piton et al. 2019b

Suggestion taken into account as previsously noted.

Reviewer Comment: 34. Results:

Reviewer Comment: 35. L213ff: please also comment on the effect of flow condition on this process; please see description of LW accumulation process at racks by Schalko et al. 2019 WRR - it is very similar and worthwhile to compare

Thank you for the suggestion. The key difference with the work of Schalko et al. (2019) and several other work of ETHZ is that we used varying water discharge while most works published so far were focusing on jam formation under steady discharge. Anyway, we will add in the revised paper the following sentence: "More detailed description of the formation of LW jam can be found, e.g., in Schalko et al. (2009a) under constant water discharge."

Reviewer Comment: 36. L290: Regarding the surface waves: Why did you not add a floater or flow straightener to suppress surface waves - how can this test be included if the initial conditions cannot be compared to the other tests?

We had problems with the energy dissipation at the inlet when the pumps were working at full power in the initial set up. An adaption was made before launching the run with LW to better dissipate energy. In our opinion, the tests can be included in the dataset because they were performed mostly to check the validity of the equation for pure water conditions and the fit is very good for discharges lower than 4.5-5 l/s.

Reviewer Comment: 37. L292: How was this problem fixed for the measurements with LW?

The volume of the upstream tank where pumps discharged was increased to better dissipate the kinetic energy from the pipe.

Reviewer Comment: 38. L324: See general comment on Schalko et al. (2019, Water Resources Research) stating that “The initiation of a LW carpet formation corresponds to the state, where the buoyancy force is higher than the downward drag force.” Please add reference

Thanks for the suggestion. Indeed, the descriptions was yet present in Schalko et al. reference will be added here.

Reviewer Comment: 39. How did the authors account for the effect of organic fine material? Did you include the dimensions of the pine needles in an average “equivalent log diameter”?

Good point. No, the mean log diameter is determined only for coarse elements. We will add the following sentence: “Where, DLW,mean. is the mean log diameter of the LW mixture (m) determine only for LW elements (diameter > 0.1 m in the field, taken as 3 mm in our case assuming a scale ratio of 1:33).”

Reviewer Comment: 40. Figure 11: I agree that the data provide information that h* decreases with increase T/Fd ratio, but the variations are extremely high; please discuss.

We will discuss it but really, to our opinion, we should abandon the habit and hope that one can compute one single accurate value of water depth in an open check dam experiencing an extreme flood event. Working with range of uncertainties should become the standard way.

Here we will add the following sentence and add more element in the discussion: “Random variation in the log arrangement made the threshold h* value varying around the mean trend. Such stochasticity must be accepted as part of the process of LW jamming and behaviour.”

Reviewer Comment: 41. Discussion

Reviewer Comment: 42. See general comment regarding comparison with other studies

Agree, we will do it.

Reviewer Comment: 43. L375: Please clarify; Given the same approach flow depth, resulting backwater rise under supercritical conditions is higher because of the increased flow velocity and hence increased energy head.

Thanks for the suggestion. We will take it into account.

Reviewer Comment: 44. L377: What are “average LW volumes”, these classifications are based on previous flume experiments and do not correspond to measured LW volumes in the field. I advise to use specific volume numbers or base such categories on field observations.

Good suggestion. We will rather use the dimensionless volume of LW suggested by Schalko et al. (2019) to provide a quantitative and comparable assessment.

Reviewer Comment: 45. L379: If you use the term kinetic energy then please use "potential energy" and not height; but I would recommend to use terminology that reflects your equation. In addition, this is not only the case for supercritical flow, but also for subcritical flow. Also, in L98 you state that F varied between 0.01 and 0.3, which is subcritical. Please revise.

Correct. We will rephrase the sentence.

Reviewer Comment: 46. L391: The authors observed that the LW accumulation piled up? Would you not say that the initial logs block the open flow cross-section, and logs are pulled downward along the dam?

Right, downward but also upward when increasing the discharge. We really think that the jam formation is slightly different than when using steady discharge. This sentence seek to explain that if the opening are more jammed, then the crest will also be more jammed, so we do not mention the drowning component.

Reviewer Comment: 47. L415: Due to the characteristics of LW it should not be recommended to use 1D models when simulation the interaction between LW and infrastructures. Since the paper is very long, I would recommend deleting this section and add the application of the approach in the Conclusions section.

We do not really agree with Reviewer #1 on this point. Works by Gschnitzer et al. (2017, Geomorphology) describe fairly well that in narrow and regular torrent bed, the bulk effect of LW can be taken into account with simple methods. The simple equations of Schalko et al. (2019) are even simpler than 1D models. In addition, we think important to stress that our T/Fd number has a much broader potential than the sole question of open check dam and LW. The section being quite short (160 words) we would like to keep it.

Reviewer Comment: 48. L435: See general comment regarding uncertainty – to apply the ratio between buoyancy and drag force, the backwater rise or resulting flow velocity is required. This depends on beta1 and beta2, which exhibit large fluctuations. Please comment.

The section will be adapted according to our response to the general comment.

Reviewer Comment: 49. Conclusions

Reviewer Comment: 50. L458: The increase in flow depth includes a wide range - how should this then be considered by engineers?

See response to general comment. Working with a mean value of flow depth should be avoided in our opinion. It is more rigorous to acknowledge the random variability of the process and to bound the structure behaviour with scenarios.

Reviewer Comment: 51. Technical comments

Reviewer Comment: 52. Abstract

Reviewer Comment: 53. What is a piedmont river?

Wikipedia definition: “In physical geography, piedmont denotes a region of foothills of a mountain range. ”

Reviewer Comment: 54. Introduction

Reviewer Comment: 55. L30: “LW might actually play a significant role. . .”; please revise as several previous floods demonstrated the destructive power of LW accumulation at river infrastructures.

Done.

Reviewer Comment: 56. L35: Replace “disturbing” with affecting

Done

Reviewer Comment: 57. L55: Revise the two research questions, as they are very difficult to read in the present form. As described above, I advise that the authors use “hydraulic losses” instead of “energy dissipation”. In addition, I would recommend replacing “bridge jamming hazards” with a more generic term as “flood related and structural hazards”

Thanks for the suggestion.

Reviewer Comment: 58. L62: Recommend using “poles” or simply “racks” instead of piles as these terms were also used in the cited papers.

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Thanks for the suggestion.

Reviewer Comment: 59. Computing open check dam discharge capacity Reviewer Comment: 60. L96: Add flow depth to h and energy head to H

Ok, done.

Reviewer Comment: 61. L105: Add reference

Ok, done.

Reviewer Comment: 62. L107: Add h1 to Fig. 1

Well, we prefer not to add h1 because it varies depending on the dam type as explained in the paragraph just after Eq. (4).

Reviewer Comment: 63. L126: Revise sentence and refer to section instead of “see later”.

Ok, rephrase and enhanced.

Reviewer Comment: 64. Materials and Methods

Reviewer Comment: 65. L132: Either state one model scale factor or the range; in addition, please replace ”to the authors’ opinion” with a reference or remove it.

We first state that the range 1:20-1-60 is relevant to our opinion but use a 1:34 scale to provide field equivalent throughout the paper.

Reviewer Comment: 66. L144: than instead of that

Done, thanks.

Reviewer Comment: 67. L150: figure? Not clear

Rephrased.

Reviewer Comment: 68. L158: Check document regarding “error”

Done, thanks.

Reviewer Comment: 69. L161: The authors use the term “large wood” in the title and ms; I advise to only use this term and replace “debris” and “coarse debris”.

We mostly stick to the LW term however, woody debris is still widely used in the literature on hazard mitigation and is more concise than other formulation. We would like to keep the term.

Reviewer Comment: 70. L177: “to the flow” instead of “in the flow”

Done, thanks.

Reviewer Comment: 71. L177ff: Revise description on how the LW was added to the flow. “The LW jam could thus always grow up if flow conditions allowed it.” This is not clear.

We rephrased this passage and remove this particular sentence.

Reviewer Comment: 72. Figure 4: The scheme is very helpful; the data points are very informative, but to improve readability I recommend to only plot data of e.g., 2 LW mixtures and data without LW.

We added the whole mixture in the Figure only when all data are taken into account, i.e., when computing Beta max and min. It would be less clear if we plot only two mixtures and say that we compute the min and max only on these two mixture, wouldn’t it?

Reviewer Comment: 73. L196: Add “data” to point transparency

Done, thanks.

Reviewer Comment: 74. Results:

Reviewer Comment: 75. L200: Include section numbers or delete this summary

Done, thanks.

Reviewer Comment: 76. L203: what are “most runs”?

Well, phase 3 of overtopping was not observed on several run with the Sabo dam because our pumps were not powerful enough. Anyway, we remove ‘most’.

Reviewer Comment: 77. L204: “LW accumulation at check-dam” not against

Ok, done.

Reviewer Comment: 78. L205ff: Specify orientation and location of log (e.g.: in a horizontal position to the flow direction” or simply horizontal to the flow direction). In addition, revise: “They get stuck against and often parallel to the dam.”

Ok, done.

Reviewer Comment: 79. L210: Please specify “in the LW jamming”

Ok, done.

Reviewer Comment: 80. L219: Revise “overflowing on the spillway” and check used prepositions in entire ms

Ok, done.

Reviewer Comment: 81. L222: “few LW pieces finding a way over the spillway”, please revise, e.g. “few logs were transported over the spillway”

Ok, done.

Reviewer Comment: 82. L234: Delete “Nonetheless” or combine the subsections and make it clear to what “nonetheless” refers to.

Ok, done.

Reviewer Comment: 83. L239: If this was not tested or observed, please revise this sentence. e.g. it can be hypothesized and not "without any doubt".

Ok, done.

Reviewer Comment: 84. Figure 5: Please add flow direction arrows, and specify “most runs”

Ok, done.

Reviewer Comment: 85. Figure 6-9 and related text sections: See comment regarding “debris” and general comment regarding effect of LW dimensions on backwater rise.

See previous responses.

Reviewer Comment: 86. L270: delete “really”

Ok, done.

Reviewer Comment: 87. L276: close to each other not from

Ok, done.

Reviewer Comment: 88. L276: not clear what is meant by “current lines”

Sorry, wrong traduction, we meant streamlines. Now corrected.

Reviewer Comment: 89. L303: three instead of some

Modified.

Reviewer Comment: 90. Equation 5: please add definition of z2 again

Ok, done.

Reviewer Comment: 91. L312: maximum instead of max

Ok, done.

Reviewer Comment: 92. Figure 10: The different sizes of data points corresponding to release of LW are very helpful in Figure 11, but I would use same size for this Figure since the parameter corresponds to the x-axis.

Ok, done.

Reviewer Comment: 93. L322: Please revise, difficult to follow (LW submerged in number and tightly entangled?)

Revised.

Reviewer Comment: 94. L327: differentiate instead of “discriminate”

Ok, done.

Reviewer Comment: 95. Equation 7: I recommend using rho_LW instead of rho_s to avoid confusion with sediment density

Good idea, done.

Reviewer Comment: 96. L332: Recommend using V instead of u in Equation for consistency; based on the number of symbols a “Notation” section would be very helpful.

Ok, done.

Reviewer Comment: 97. L341: Delete “sucked” or replace

Ok, done.

Reviewer Comment: 98. L352: Close to the threshold

Ok, done.

Reviewer Comment: 99. Discussion

Reviewer Comment: 100. L363: I agree but it is somewhat strange to write this sentence in the section "comparison"; you may want to move it to “Conclusions”

Ok, done.

Reviewer Comment: 101. L365: represents instead of “encapsulates”

Ok, done.

Reviewer Comment: 102. L367 ff: exhibit instead of experience

Ok, done.

Reviewer Comment: 103. L374: approaching instead of incoming flow

Ok, done.

Reviewer Comment: 104. L383: dams

Ok, done.

Reviewer Comment: 105. 398: Revise “thus flow power to stuck LW against the dam”

Ok, done.

Reviewer Comment: 106. Table 2: What is meant by “marginal release”; definition of LW volume categories not clear; 540 dmˆ3 were added in Schalko et al. compared to 75 dmˆ3 in Schmocker and Hager

Quantitative values using Vs,rel as proposed by Schalko et al. will be use.

Reviewer Comment: 107. L403: Please revise, not clear.

The section was rewritten.

Reviewer Comment: 108. L430: Revise “fruit”

Ok, done.

Reviewer Comment: 109. L444: differentiate instead of discriminate

Ok, done.

Reviewer Comment: 110. Conclusions

Reviewer Comment: 111. L450: Please revise; what is “the other hand”; what are “transported element sizes” – logs?

Ok, revised.

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Reviewer Comment: 112. L451: affect instead of “trouble”

Ok, done.

Reviewer Comment: 113. L465: What is meant by “without calibration” – see general comment on this transition

We meant that it has not been necessary to calibrate a given empirical parameter to compute T/Fd and obtain a regime change at the threshold value: Q, h and rho_LW,D_LW are measured and C_D is taken from the literature and the change indeed appear at T/F_D =1. Anyway, it is a detail and we removed the words.

We would like to profoundly thank reviewer #1 for this thorough and very constructive review. We feel lucky to benefit from the feedback of such an expert and rigorous review on our paper.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-158, 2020.

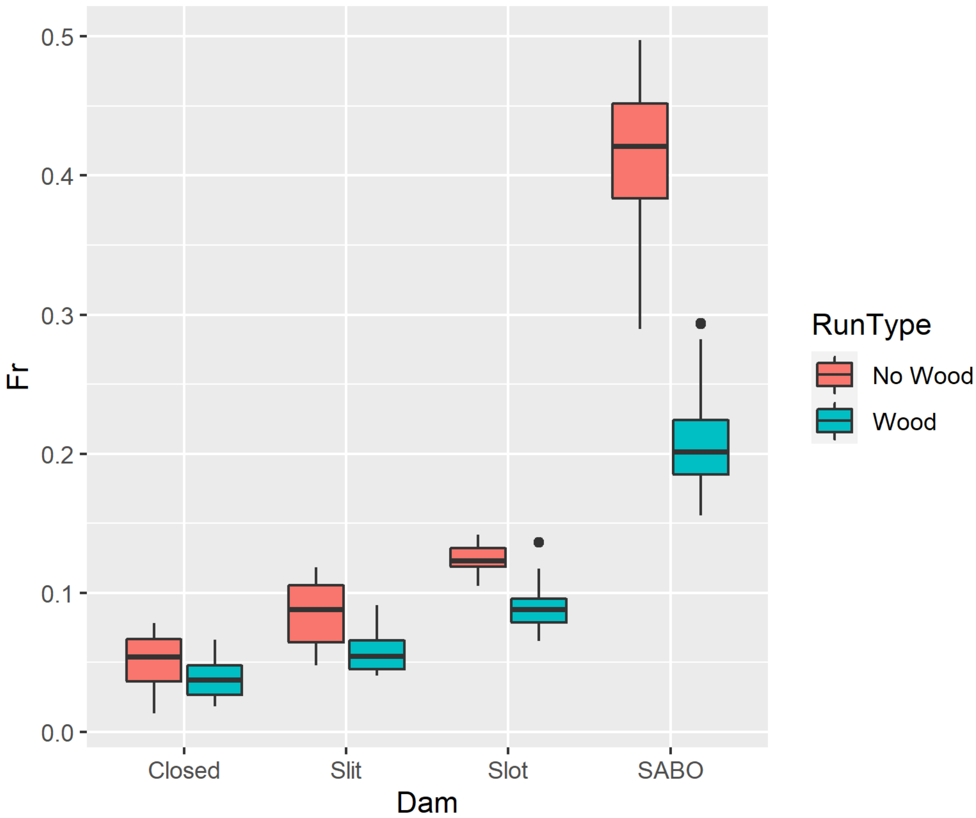

Fig. 1. Figure 1: Froude number with and without LW for each dam type