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Review and rebuttal of the paper

Analytical and Numerical Modelling of Debris Impact Events on Columns

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Editor handling the paper: Nils Goseberg

Response to reviewer comments for

Manuscript Number:

Analytical and Numerical Modelling of Debris Impact Events on Columns

by Patrick Joynt, Ioan Nistor, Dan Palermo, Jacob Stolle

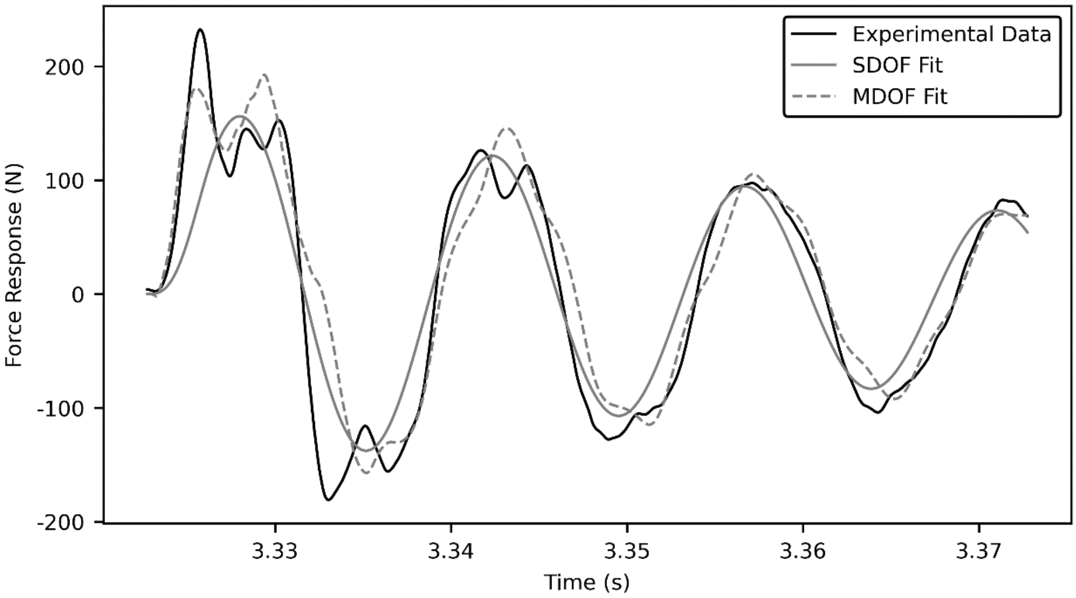
The authors wish to express their gratitude to all reviewers and for their detailed comments, constructive criticism, and for granting them the opportunity to improve the manuscript. All comments have been carefully addressed. In the following, the authors show each comment received in *italics*, followed by their response and action taken in a non-italicized font. All line numbers refer to those of the revised manuscript.

1 ADDRRESSING COMMENTS from REVIEWER A:

Reviewer A

Major revisions:

Comment 1: *Please add numbers at each line for the review.* **Actions taken:** Line numbers have been added for the review.
Comment 2: *Chapter 1 needs to be reinforced, and Chapter 3 is too short as a single chapter. The authors may consider re-organizing Chapters 1, 2, and 3.* **Action taken:** Combined Chapter 2 and 3.
Comment 3: *In Fig.3, Please explain that there are dual peaks. It is questionable why the authors picked this one-point data among 22. Is it a good case to show better results in MDOF? Is there any case that shows similar results between SDOF and MDOF instead?* **Action taken:** This specific event was picked as it displays the best fit to the measured data for both the SDOF and MDOF methods. The results between the SDOF and MDOF methods are similar in magnitude and phase, however, the SDOF method will never display any dual-peak behaviour. This is because that dual-peak behaviour is due to higher mode effects, which can only be included by modelling more than one degree of freedom. Below is another example of the fitting:



Comment 4: *The table. 1 compares the results of ASCE7-22, but there is less information on how to find kd and ks for ASCE7-22. Are ASCE7 data fitting results? ASCE7 suggests that the effective stiffness of the impacting debris or the lateral stiffness of the impacted structural element(s) deformed by the impact, whichever is less. No reason to compare both here.*

Action taken:
Ks and kd are outlined in Stolle et al. (2019), added an asterisk to Table 1 to clarify this. Both impacts for Ks and Kd were displayed to ensure the read is aware of the inherent assumptions being made when using either stiffness and how they influence the impact force.

Comment 5: *Fig. 5 needs to add experimental data for better comparisons. The performance of MDOF and SDOF varies depending on trials, so it is confusing how much MDOF is good or SDOF is bad. Also, it is questionable whether the results of fitted SDOF are the best-fitted results. The authors may clarify the method to fit all 22 trials for both SDOF and MDOF for Fig. 3 to 6.*

Action taken:
Added reference to Joynt et al. (2021) for the reader to investigate further how exactly the fitting was done. The experimental results did not measure i*mpact force histories* as presented in Fig. 5. The experimental results measured *force response histories*, the SDOF and MDOF methods used these *force response histories* to back-calculate the *impact force histories* by a fitting method. The fitting method is different for both the SDOF and MDOF methods as these are inherently different methods for discretizing the structure and come with their own assumptions. The MDOF method includes mass, damping, and stiffness matrices that define these quantities for each DOF whereas the SDOF method assumes a constant value as opposed to a matrix. The fitting presented Fig. 5 are the best-fitted results for the SDOF method and MDOF method irrespective of each other.

Comment 6: *Page 9, in the second paragraph. The authors mentioned that " It is apparent from the plot that regardless of the stiffness value chosen, the maximum force response using the methods*

outlined in ASCE 7-22 resulted in an overestimation of the maximum measured force response." It is still not clear how two stiffness are decided in this study. Also, the impact loading from ASCE7 assumes the exact collision to the structure, which causes the maximum impact loading. I think the 22 trials by Stolle (2019 et al.) were not perfect colliding conditions. For design purposes, overestimation is obvious, but this sentence may mislead readers. **Action taken:** Ks and kd are outlined in Stolle et al. (2019), added an asterisk to Table 1 to clarify this. Clarified the sentence to say *"…, the maximum force response using the methods outlined in ASCE 7-22 resulted in an upper-bound estimation of the …"*
Comment 7: *Chapter 5.1 introduces the numerical model (ALE method) for this study with very brief descriptions. Can the authors provide more details about complicated processes in hydrodynamics and debris transport? How to control debris entrainment and dragging? What are the governing equations and any assumptions applied in the ALE method to apply in the current study?* **Action taken:** Added reference to Joynt et al. (2021) where more details of the ALE method can be found about how the model treats Fluid-Structure Interaction, the governing equations, and assumptions in the ALE method.
Comment 8: *Fig. 7 provides the sketch of the Dam-break experiment while the experimental data were already utilized and explained earlier without a detailed sketch in Chapter 4. If this figure were placed earlier, it would be helpful to explain the data set (22 trials) utilized in the analytic approach in Chapter 4.* **Action taken:** Added a "Fig. 3" from Stolle et al. 2019 that shows the sketch of the wave flume at the University of Ottawa
Comment 9: *Fig. 9 shows the sensitivities of mesh on surface elevation and velocities of leading edge flow including lots of uncertainties in experimental data (e.g., WG4). It is not clear whether experimental data were performed with debris or not. In addition, please give any notes on why there are lots of uncertainties in measured data at WG4. Similarly, Fig. 12 shows the case with multi-debris, and the results at WG4 are different from Fig. 9. Please clarify the experimental conditions between Fig. 9 and 12.* **Action taken:** Mesh sensitivity is only completed for single-debris impacts, the measured data seen in Figures 10 and 12 are the same. The modelled data between Fig 10 and Fig 12 are different as these are different simulations. The variability in the measured data can be due to many factors that would influence any experimental dataset of this nature such as, slight variations of the impoundment depth, slight variations in the release time of the gate, variability in the transport of debris that could influence the measurements right at the structure. Stolle et al. (2019) display that there isn't much variation in the water surface elevation when a mean and standard deviation are calculated. I am directly presenting the water surface elevation timeseries without calculating the mean or standard deviation between the runs.

Comment 10: *Fig. 11 and 13 display distinct deviations between the experiment and numerical model results at quasi-steady state(after 3.7 sec) conditions. Can authors provide the any possible reasons for those deviations?* **Action taken:** As described in the paper the bore front in the model is traveling faster than the experimental data, this results in larger hydrodynamic forces. The reasoning for the bore traveling faster is detailed in Joynt et al. (2021) but essentially a bed friction value of 0.0293 was applied in the model based on results from Stolle et al. (2019) but this model parameter could have been calibrated better to match the experimental data.
Comment 11: *Fig. 14 shows both experimental (Stolle et al., 2020b) and numerical model results, but there was no information about the data from (Stolle et al., 2020b). I think this is not mentioned. Are they the same data used in Stolle et al., 2019?* **Action taken:** Yes, they are from the same experimental program. Added text to outline that this experimental data is from Stolle et al. (2018) and revised the reference in the Figure.
Comment 12: *Fig. 14, I think the experimental data show much narrower spreading angles rather than two empirical model results (Naito and Nistor et al.). The numerical model results are supposed to follow the results of experimental data rather than two empirical model results. Therefore, I think the authors need to mention why numerical results show large deviations from the experimental data. It could be from the different assumptions or configurations utilized in numerical models. This figure clearly shows the limits of the current numerical model studies.* **Action taken:** Note that the experimental data is only for single debris transport, which when comparing to the model shows that the model is within the bounds of the experimental data. Text as been added to clarify this in the manuscript.
Comment 13: *Fig 15, I think the comparisons of the impact forces may be meaningless without improved modeling on hydrodynamics without debris and debris transport (spreading).* **Action taken:** See comment 10 and comment 12.
Comment 14: *The conclusion part needs to be improved.* *a) Consider adding some discussions after or before the conclusions. For example, the 1st conclusion is not like a conclusion. The 3rd and 4th could be merged as one, and the 4th conclusion may not be appropriate to the conclusion (it is like a discussion). The 2nd and 5th conclusions are somewhat similar.* *b) Some conclusions are not well supported in the study. Moreover, it is questionable how to quantify the simulation results that fell within the bounds of the experimental data as concluded here. The reviewer understands the difficulties of modeling flow and debris transport. The modeling of debris is very sensitive to the flow and difficult to model all 3-D motion. Also, there is not enough experimental data as benchmark tests. I think the limits of the current approaches are provided in discussions.*

c) The last conclusion may not be sufficiently supported in this study. It is uncertain whether the numerical model truly simulates and predicts debris impact loadings. **Action taken:** I have revised the conclusion section considerable and added some clarifies on the limitations of the modelling effort.

Major revisions:

Comment 1: *Please reconsider appropriate "Keywords". Some keywords (e.g., Coastal engineering) seem too broad.* **Action taken:** Revised key words to the following: Debris-impact, dam break wave, impact force estimation, extreme events, structural response, numerical modelling
Comment 2: *Page 3, 1st "Observing post-tsunami.." Please finish the sentence.* **Action taken:** Fixed error.
Comment 3: *In Fig. 3, Legend, ASCE7-16? Or ASCE7-22?* **Action taken:** Updated legend
Comment 4: *6, What is \xi stands for?* **Action taken:** Added text, it stands for the damping coefficient
Comment 5: *In Chapter 4.2.1, in the third paragraph. Who are the authors here? Is it Stolle et al. (2019)? Please rephrase this paragraph.* **Action taken:** Added Stolle et al. (2019) instead of "The authors"
Comment 6: *7. 'u' uses as an x-direction velocity scalar in 4.2.3 (eq. 9), but it was shown as a displacement vector in Eq. 7. It will cause confusion for readers.* **Action taken:** Changed 'u' in Eq. 9 to 'x'
Comment 7: *10 is missing.* **Action taken:** Fixed

Comment 8: *Some critical typo relates to the missed figure. Please carefully review and correct them.* **Action taken:** Resolved
Comment 9: *The redundant expression "The conclusion can be made"* **Action taken:** Revised all sentences that included "The conclusion can be made" to not include these words.

2 ADDRRESSING COMMENTS from REVIEWER B:

Reviewer B

The present review refers to the manuscript "Analytical and Numerical Modelling of Debris Impact Events on Columns" submitted to the Journal of Coastal and Hydraulic Structures. The manuscripts presents a study on the impact of (single) debris on vertical columns, showing the effectiveness of using MDOF approach over SDOF. In addition, the study also provided some numerical simulations that were shown to be successful in reproducing the process.

The topics discussed in the manuscript are relevant to the readership of JCHS and the results presented will support the community in understanding the interaction between debris and structures. For these reasons I am positive about the manuscript submitted, but I have a few comments that could help the reader improving the quality of the manuscript.

Major revisions:

Comment 1: *Page 2, 2nd line after Figure 1 – Authors only mention concrete debris and ships, but the description of debris type could be more extensive, and more realistic of tsunami events.* **Action taken:** Added the following after that sentence: "However, it is important to note that debris sources can encompass anything that is being transported by the tsunami."
Comment 2: *Page 3, 3rd line – Authors mention that this type of loading has not received much attention. Personally, I think that this is misleading because there have been quite a lot of studies looking at this topic. However, the complexity of the processes involved (and the assumptions previously made) require new approaches and more detailed studies.* **Action taken:** Revised to: **"**This type of loading has seen more attention in recent history, however, the complexity of the processes involved require new approaches and more detailed studies."
Comment 3: *Page 3, 3rd line of chapter 2 – Although debris damming is not the objective of this manuscript, IMO it should be mentioned in the list of possible scenarios for multi-debris impacts.*

Action taken:
Revised first sentence to the following:
“Debris impact loads can be generated by debris damming, single debris impacts, and multi-debris impacts.”

Comment 4: *Chapter 3 – I think this chapter is too short to be self-standing and I would recommend the authors to include it within the previous one.*

Action taken:
Combined with previous one.

Comment 5: *Section 4.1, 3rd line – authors should say “up to 0.7m”, since in the next parts they use a depth of 0.4m.*

Action taken:
Revised to “up to 0.7m”

Comment 6: *Page 5, 3rd line of 2nd paragraph – “did not present any obvious impact event”. The authors mean peak impact force?*

Action taken:
Revised to:
“… did not present any obvious impact event (i.e. no peaks in the signal).”

Comment 7: *1st line after Eq.[7] – Why 160 elastic beam? This is not clear to me*

Action taken:
Added reference to Joynt et al. (2021) where the reader can find details on the mesh and time step sensitivity analysis. 160 elements was needed such that shear force in the model was converged to a constant value.

Comment 8: *Section 4.3, 2nd paragraph, 3rd line – The authors find an estimation of 8.7% for MDOF. Would be interesting to see the same comparison for SDOF.*

Action taken:
In the paragraph above it is mentioned that the SDOF method underestimated the peak by 40%.

Comment 9: *Figure 3b – Would be nice to read a bit more on why there is such a difference between the 2 approaches.*

Action taken:
Details on the two approaches can be found in Joynt et al. (2021). The two approaches vary in how they discretize the system. The SDOF method assumes the entire structure can be represented by one single mass. The MDOF method discretizes the structure into 160 nodes that each have a mass, stiffness, and damping. This is the reason the MDOF can capture the dual-peak behaviour of the system, it can capture more modes of vibration in the system that a SDOF method cannot capture.

Comment 10: *Table 1 – Not sure what the authors mean with "one experimental data point"* **Action taken:** Clarified title to be: "Table 1: Impact force history properties associated with Figure 4(b)."
Comment 11: *Figure 4 – Lines are too similar. Difficult to see which one is MDOF.* **Action taken:** Increased line width to make figure easier to read.
Comment 12: *Page 7, 4th line after Figure 4 – In this case SDOF overestimated, while in Figure 3a it underestimated. Can the authors explain why this is the case?* **Action taken:** In Figure 4a the SDOF results are from fitting to the force response history using a SDOF method to solve the system. In Figure 5, the impact force histories presented in Figure 4b and Table 1 are applied to the MDOF system to see how a more complex system responds to these different impact force histories. The objective of Figure 5 is to show the consequences of assuming one method of estimating impact force histories over another.
Comment 13: *Figure 6 – Data for MDOF should be more visible.* **Action taken:** Made the scatter larger.
Comment 14: *Page 9, first line – Authors mention ratios of 0.9. For design purposes, this could be of concern, since it would underpredict the impact force. Do the authors have any recommendation on how to rightfully implement the MDOF for the estimation of the loads?* **Action taken:** In this case the goal is to get a ratio as close to 1.0, as that suggests we've captured the maximum measured force response exactly. For design purposes the design engineer should include safety factors by following the ASD or LRFD process.
Comment 15: *Figure 9 – I have some concerns on the initial water levels. It is well known that dam-break waves on wet bed behave very differently compared to those on dry bed. A clarification on this matter is needed. Also, could this be a reason for divergence between numerical results and the experiments?* **Action taken:** The initial water levels are influence by the swing-gate structure at the University of Ottawa Flume which Stolle et al. (2018b) suggest are the cause for the profile at WG1. Added some text around line ~324 to outline that these are for wet-bed simulations.

Comment 16: *Page 10, 3rd paragraph – Can the authors explain how they have considered the effect of roughness in the numerical simulations? Could that be an additional reason why there are differences between the results?*

Action taken:
Added some text around line ~340 to describe roughness. A constant factor of 0.0293 was applied to the bed flume, which is based on the mean frictional factor determined in Stolle et al. (2019). This is a reason why the bore front is traveling faster in the numerical model when compared to the measured data and can explain the some of the difference in the model. Essentially this could have been a calibration parameter to better match the measured data.

3 ADDRRESSING COMMENTS from REVIEWER C:

Reviewer C

This is a good quality manuscript, which is generally very well written. The descriptions are succinct; in fact, it would be easier to follow some of the methods if there were some additional narrative, but for the interested reader there is clear signposting to the underpinning reference material. The only major comment, more of a reflection, is related to the arrangement of material. The numerical model could be presented much earlier on, alongside the analytical and experimental methods. It might then be possible to make more comparisons of results (not that this is lacking!). However, it might be the case that this has already been considered and ruled out, so it's only a suggestion. Also, by the time that I was reading through the simulation results I had forgotten that the experiments used only single item debris; this might need to be reiterated when comparisons are made?

Major revisions:

Comment 1: *Also, by the time that I was reading through the simulation results I had forgotten that the experiments used only single item debris; this might need to be reiterated when comparisons are made?*

Action taken:
Added a reminder at around line 401.