Author's response to anonymous referee #2

Suggestion for revision:

Summary:

The Authors improved many aspects in the manuscript and the modelling of the slide phase includes some advancements with the limitations (e.g. the shape factor as a fitting parameter) clearly communicated. However, the modelling of the landslide-tsunamis is a step backward (shallow-water equations (SWEs), excluding frequency dispersion) in my view, reflecting the state of the art 10-15 years ago. The Authors do not fully acknowledge these limitations in the article, despite the concerns of both Reviewers. Yes, there is a new statement on L368 about these limitations, but these limitations also need to be made clear in other key sections such as the Introduction, Conclusions, during the discussions of Figs. 4 and 9 (where the limitations of the applied model are obvious), etc. Further, there are still some linguistic issues, typos and smaller inconsistencies. Finally, as already highlighted in the 1st review, I do not think that some key conclusions are supported by the findings in the article. More details are given below.

- Authors: the limitations have been developed in the respective sections

Specific comments:

L39: Good that the RANS equations are now introduced as well as an alternative to the model applied by the Authors, but the full term needs to be introduced, not only the abbreviation. More background into other options (DNS, LES) would also be desirable.

- Authors: the full terms have been introduced and the other suggested options have been included (L40-41).

L46/L185: It is not clear what the Authors mean by "free-surface nature". Do not all mentioned models have a free surface? Or do they mean the way the free surface is treated (e.g. Volume of Fluids)?

- Authors: The terms have been changed (L51, 54, 64, 199)

L315 and Fig. 9: "…the numerical simulation reproduces the wave observed in the physical experiment very well in terms of amplitude and timing at each probe." The "very well" is subjective and needs to be backed up with a more objective criterion (goodness of fit parameter). It is fine if the Authors do not want to use the nRMSE, but they need to use an alternative way to scientifically quantify the goodness of fit (e.g. a % deviations between the amplitudes), as already pointed out by both Reviewers in the 1st review round in other contexts.

- Authors: % of deviation have been added (L339-349)

Fig. 9 (first panel): The numerical wave appears to be cut? This makes it impossible to appreciate the quality of the agreement.

- Authors: The top of the wave have been added (Fig. 9)

L359: This new paragraph is helpful showing that a simulation is performed efficiently. However, I believe this section is at the incorrect place in the manuscript (it should be part of the Methodology). Further, have convergence tests been performed?

- Authors: The paragraph has been moved (Sect. 3.2). No convergence tests have been performed.

L396: "…overall, the model effectively handles the complex phenomenon occurring during the interaction between the landslide and the water." I do not fully agree with this conclusion, given that the model cannot model impact craters and wave breaking, as stated in the manuscript on L365: "This lack also implies that the model cannot consider the impact crater as long as the steepness of the water surface is not steeper than sub-vertical". I do not think many scientists and engineers would find the shallow-water equations (SWEs) a wise choice to model subaerial landslide-tsunami generation.

- Authors: Context have been added (L423-424) and the sentence is more nuanced (L425).

L402 and Fig. 10: "…by the fact that the results of our model also fit well with those experiments." I understand that this mainly based on the comparison in Fig. 10. Yes, the data scatter between the +/-30% for P < 9 (investigated in the original study). However, it is obvious that the data trend systematic deviates from the prediction, for both P < 9 and more significantly over the full presented range of P. It is further obvious that the trend of the data conducted for water depths of up to 0.17 m is very different for the experiments conducted at water depths of 0.10, 0.08 and 0.05 m. There are clear recommendations in the technical literature that laboratory experiments should not be performed certain Weber and Reynolds number limitations (roughly corresponding to water depths smaller than 0.20 m) to avoid significant scale effects. There appears currently to be no attempt in the manuscript to explain this systematic deviation in Fig. 10 with scale effects, or another potential explanation offered by the Authors.

- Authors: The results have received more details (L383-386) and the discussion has been extended (L406-409; 431).

L403: "Finally, our model is validated by a benchmark test performed herein, as this approach is very simple to implement and is very efficient in terms of computational resources. Therefore, we consider our model as a tool of choice for the assessment of landslide-generated tsunami hazards." I agree that the model is very efficient, but I do not feel that "Therefore" is justified as the efficiency of a tool is not the only criterion for hazard assessment (it also needs to represent the underlying physics to an appropriate level, as appreciated by the Authors in the article). I do not think it would be a wise choice to select a SWEs model for landslide-tsunami hazard assessment due to points L396 and L402 above and the fact that the model is not able to consider frequency dispersion (partially responsible for the dangerous underestimation in Fig. 9 in some of the panels). I do not think the answer from the Authors in the response "The comments of both reviewers show that they understand the statement of the balance "correctness-efficiency" and that we had to make a choice of the level of approximation. We chose the non-linear shallow water equations (without multiple add-ons), hence, the inherent approximations and incomplete physics (such as frequency dispersion). We won't discuss in the paper the well-known limitations of this approach." is satisfactory. There are efficient, more appropriate and widely applied alternatives to the SWEs (non-hydrostatic non-linear SWEs, RANS equations, coupled approaches, GPU acceleration, etc.) to model landslide-tsunamis available. The Reviewers review the article not for themselves, but on behave of the readership of the Journal. My feeling as a Reviewer is that 95% of the readership of the Journal are not fully aware of the "correctness-efficiency" aspects and the fundamental limitations of the SWEs to model subaerial landslide-tsunami generation and propagation and will be misled by such statements if the limitations are not better communicated in the article.

- Authors: We agree with the reviewer statement and we have developed a more detailed explanation and provided a better context (L45-48; 57-59; 255-257; 349-351; 431; 433-435)

Suggested grammatical corrections and minor points:

- Authors: All the suggested corrections have been performed

Abstract: There are still formatting aspects which should be improved, e.g. the abstract should be presented as 1 paragraph (not several small ones) and the text/figures need to be arranged such that there are no large free spaces on the pages (see e.g. at the bottom of page 10 or 13).

L57: Please add the missing free space in "whichis".

L64: Please drop one of the "that".

L84: Please revise the unclear expression "static critical state friction".

L90: Please replace "paper" (rather informal) with "article".

L96: I do not think "eq. 1" is the common writing style in this journal, normally it would be "(1)". Please check and apply it correctly for all equations.

L100: Again, it is common practice to write parameters in italic in research articles. This is still a general issue in the article, there are many parameters with an inconsistent writing style (L100, L106, L147, L218, caption Fig. 2, L252, L254, L255, L256, L257, L279, captions of Figs. 5, 7, 8, 9 and 10 and many times in the Notation).

L121 (Eq. (8)): On the other hand, numbers, "sin", "cos" and "tan", Fr, P, Re and brackets should not be written in italic (Eqs. (8), (9), (10), (11) and (12), L147, L153, L155, Eqs. (20), (21) and (22), Table 1, L248, L329, L331, L335, L336, L338, L341, Eq. (37), L345, Eq. (39), L350, L355, L356, L400 and in the Notations).

L181: Please write "…when applied in our, never fit the experimental data."

Legend of Fig. 2: Please improve the presentation and remove the typo in "Differnce"

Figs. 4, 6, 7 and 8: Should it read "z" and "x" rather than "Z" and "X" on the axes? Further, some of the numbers on the axes are too small.

L212-L218 and legend of Fig. 2: The writing style of the shape factor is inconsistent (SF, FS, S_F (F as subscript)).

L231: Please write "…in two-dimensions."

L235: Please write "…the momentum transfer is… the wave propagates…"

L257 and L258: A full stop after "al" is required.

L266: The meaning of the word "discrimination" in this context is unclear.

L272: Please write "…identify the best…"

L284: Please write "…is the finally chosen rheological model."

L339: The parameters have just been introduced, so just write "The relationships between P and Fr and S…"

L370: Please write "…lack of modelling frequency dispersion…".

Notation: P is used for two different parameters. Also S is used twice, this should not be the case.

References: Please remove inconsistencies such as inconsistent use of upper and lower case letters in the article titles, abbreviation and no abbreviation of Journal titles, typos (e.g. L437 "genrated"), etc. This was already pointed out in the 1st review round.