

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1
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Comment on nhess-2021-292

Martin Mergili (Referee)

Referee comment on "Physically based modeling of co-seismic landslide, debris flow, and flood cascade" by Bastian van den Bout et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-292-RC1>, 2022

The authors present an application of an extended version of the OpenLISEM model for the integrated simulation of a landslide – debris flow – flood cascade related to the 2008 Wenchuan Earthquake, Sichuan, China. The process chain considered includes co-seismic landsliding (slope stability & runout) and rainfall-triggered post-seismic erosion of the landslide deposit, leading to a debris flow the deposit of which blocked the Min river, resulting in flooding of the Yingxiu town. The simulations are built on a diverse and detailed data acquisition campaign, needed to feed the data-hungry OpenLISEM model.

The work is highly relevant both from a scientific and from a practical point of view, and is certainly within the scope of the NHSS journal. Increasing our capacity of simulating complex landslide cascades is fundamental for better scenario-based predictions which can be used to inform risk management. This this work can be considered a major contribution in this direction, even though the uncertainties in the input data still represent an important limitation (which is addressed in the paper). Further, the manuscript is well written and illustrated, adequately describing the background, study area, data and methods, and results of the study. Challenges and limitations are adequately discussed.

As a consequence, I would certainly like to see this work published. Before recommending publication, I suggest some minor-moderate revisions. I have not identified any fundamental issues, but there are some rather minor suggestions from my side, including a number of language issues particularly towards the end of the manuscript.

Here are my detailed comments:

Eq. 21 and 22: $S_{x,f}$ and $S_{y,f}$ would be correct, I think.

236: S_f is the momentum source term for fluids, not for solids. Please thoroughly check all equations and variable descriptions for correctness.

Figure 4: It might be nice to add a photo showing the situation. I am sure the authors have plenty of field photos available – if not, I can offer the following:

<https://www.mergili.at/worldimages/picture.php?/7252/category/60>

410: “pedotransfer functions” (not “pseudo transfer functions”), I think

440ff: Are the root systems deeper than the typical landslide depth, so that it is appropriate to consider root cohesion for slope stability?

455: r.slope.stability includes seismic forcing (Newmark and pseudostatic), but these functions were added very recently and have not yet been thoroughly evaluated.

Fig. 9: Nice figure, but maybe better put (D) as (A), this would seem more logical to me.

Table 6: Very promising results. However, one has to keep in mind that flow modelling is always easier than slope stability modelling, as the areas where the flow concentrates are prescribed by the topography.

561f: Please explain in some more detail the correspondence of modelled and observed timing (the text is not fully clear to me).

Fig. 12: Clearly indicate also in the legend what is observation and what is simulation.

589: “effects” instead of “effecta”

598: “as a result”

599f: “Despite its importance ...”

604: entrainment constant or entrainment coefficient?

605ff: Note the new entrainment model of Pudasaini and Krautblatter (2021). [Pudasaini, S. P., Krautblatter, M. (2021). The mechanics of landslide mobility with erosion. *Nature communications*, 12(1), 1-15.]

5.2: This is a very important step. But shouldn't it rather be described in the results instead of discussions?

Fig. 14: Nice figure, but (i) legends should show classes instead of values, and (ii) the colouring is not ideal, as the medium probabilities are much paler than the low probabilities, which gives a strange visual impression.

626: "show illustrate": remove one of the two words, and: "Figure 14", not "Figure 17".

634: This statement is true in principle, but it has to be considered that the Huascarán events considered by Mergili et al. (2018b) were of a completely different type (extraordinarily rapid and energy-rich, with air-lifting of material, etc.), and there was the specific situation of the ridge that was overtopped, completely changing the impact and inducing threshold behaviour. I suggest to briefly mention that this comparison has to be interpreted with care.

644: Check language.

658: Mergili et al. 2018 a or b?

662f: In principle, this type of hazard chain is not so rare, but its magnitude and specific characteristics are.

680: "first sequence"

683: "on its banks"

688: "still under development" – is this wording appropriate when citing a 2011 paper?

693: "elevation models"

713: "Authors C. Tang and C. Tang": there is only one author named C. Tang provided in the list of authors.

723: "A dataset"