Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-224-AC2, 2018

Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "The Lituya Bay landslide-generated mega-tsunami. Numerical simulation and sensitivity analysis" *by* José Manuel González-Vida et al.

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Received and published: 8 December 2018

First of all, we would like to thank the very positive review from Referee #1 (we include a pdf version of the text at Supplement: nhess-2018-224-supplement.pdf) The edits described here have been highlighted in the revised manuscript

Regarding his/her comments:

1) Concerning the question about the initial slide velocity, we tried to numerically reproduce the Fritz et al's laboratory experiment as we tried to point out at several places along the paper. We have included a sentence at the end of section 2 in order to make

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this more explicit, in particular, we wrote: “Based on the experimental work of Fritz et al, in the present numerical study we will follow the same approach: an initial slide speed (analogous to the impulse of the pneumatic landslide generator in the lab experiment) will be imposed in order to get the 110 m/s slide impact velocity that Fritz et al measure in their experiment. The same way the laboratory experiment did reproduce the observed run-up, is the way the numerical experiment has been initialized.” Therefore, the references justifying this approach are the experimental works of Fritz et al.

2) References for the 3-parameter models. We think we do not understand precisely referee’s comment. In the model used in the present work 5 parameters are required: 1) r, ratio of densities; 2) mf, friction coefficient used in the friction between the water and the slide; 3) n1, Manning coefficient for water/bottom friction parameterization; 4) n2, Manning coefficient for slide/bottom friction parameterization; and 5) alpha, the Coulomb friction (static) angle. We have chosen for the sensitivity study 3 of these parameters (r, mf, and alpha). Therefore, it should be more precise to refer as a “5 parameter model” for which we have retained the two Manning coefficients as constant values and the other 3 parameters have been varied for the sensitivity analysis. In any case, we have included several references to models using the same kind of friction parameterizations as the ones used here, as we think this is the point that the reviewer wanted to highlight.

References included:

- For the parameterization of the term Sc (water/granular slide interface) we used a particular case of Pitman and Le (2005) or Pelanti et al (2008) parameterization. We ha added these two references.

- Dyakonova and Khoperskov (2018) for the Manning parameterization and Arcement and Schneider (1989) and Phillips and Tadayon (2006) for Manning coefficient values.

- Savage and Hutter (1989) and Gray et al (1999) for Coulomb law

3) Friction of water. All possible frictions are modelled: (a) interface water-slide (Sc) with the mf coefficient, (b) water-bottom (Sa); and (c) slide-bottom (Sb). These three are the dynamical friction terms. For the friction with the bottom, (b) and (c), a Manning law is used, and as the reviewer points out, we did not give the explicit values set for the two Manning coefficients (now included). Besides, there is a static Coulomb friction term for the granular slide.

We have provided the value for n1=0.02. It is a quite standard value used for this coefficient for water and sea bottom, usually varying between 0.015 and 0.03 in shallow water models. For river and channels tables can be found as in:

https://www.engineeringtoolbox.com/mannings-roughness-d_799.html

https://www.engineersedge.com/fluid_flow/manning-constant.htm

We have added the reference Arcement and Schneider (1989)and Phillips and Tadayon (2006) to justify the use of that value.

The value for n2 was set to 0.05, as correspond to a larger friction between the slide and the non-erodible bottom.

We have added the sentence: “The other two parameters required in model parameterizations, the Manning coefficients n_1 and n_2, were set constant with standard values of $n_1=0.02$ and $n_2=0.05$ \citep{Arcement_Schneider_1989, Phillips_Tadayon_2006}.” in Section 8.

4) Sensitivity tests. This was the most difficult item to give a suitable answer to the reviewer’s comments, but I think the result deserved the effort. The reviewer is absolutely right when he/she points out the interest of a sensibility analysis, this provide the paper an added value and it is interesting per se. The reason why we did not perform and we did not include in the paper was that some of the criteria used

- were not given by a number (C3) or

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- were difficult to measure and mostly to do so automatically (C4).

To overcome this problem and fulfil reviewer's request we have considered 4 regions (A, B, C, and D) and we have measured the maximum runup in these 4 regions. Doing so, criteria 1 and 2 are considered by runups in regions A and B respectively; in some sense criteria 3 is quantify by computing the runup in Cenotaph Island (region C), and criteria 4 is not considered and substituted by the maximum runup in a fourth region, D, closer to the exit of the Bay. Then we perform numerical experiments for a reduced number of the parameters considered for this study, the "macroscopic" set of parameters, composed by, for r= 0.3, 0.4, 0.5 and 0.6; for ïĄą = 8o, 10o, 12o, 14o, 16o; and for mf = 0.001, 0.005, 0.01, 0.025, 0.05, 0.075, 0.1. Summing up a total of 180 numerical experiments. For all these experiments we have measured the maximum run-up in the coastal strip in each of the 4 regions considered and used these data to generate 3x4 graphs that we have gathered in three figures. I must acknowledge reviewer's comment as I think it has improved the quality of our work. We have included the figures and the text required to explain all this.

5) Justification for using NLSW models. Part of the introduction is devoted to this aim, and confinement is one of the arguments, we dedicate some paragraphs to address this particular issue, and we also mention NTHMP agreement on the fact that NLSW models remain a suitable tool in enclosed basins as fjords. But another important argument used is that no previous more comprehensive model has been able to perform a numerical simulation of the complete 3D Lituya Bay problem and we do prove that a NLSW model in what respect inundation area and run-up does a very good job. 6) The introduction has been slightly reduced in 12-14 lines.

All minor corrections have been included in the new version of the manuscript.

Finally, we would like to acknowledge this very positive and constructive review.

Jorge Macías on behalf of all coauthors

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-224/nhess-2018-224-AC2-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-224, 2018.