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Discussions

Interactive comment on "Multilayer-HySEA model validation for landslide generated tsunamis. Part II Granular slides" *by* Jorge Macías et al.

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N.B.: A compiled pdf version of the response is attached to check the references not appearing in the automatic compilation of the system.

Anonymous Referee #2
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In this manuscript, the authors validate their new wave-slide model Multilayer-HySEA against to three granular landslide benchmarks from US National Tsunami Hazard and Mitigation Program (NTHMP). Their work focuses on presenting the deformable Savage-Hutter type landslide module additional to the wave model framework. The subject of the manuscript is important to both research of wave modeling and landslide tsunami hazard. The model has certain innovation by allowing the slide model and the overlying wave model to be run in the same Cartesian coordinate system. Their simulation results show that the model can capture landslide-generated waves well with a deformable granular slide and the simulations are quite efficient with the GPU-parallel computing technique. However, I find the paper to be somewhat lacking substance on its own. Some questions are necessary to be answered in the manuscript before it is published:

1. Are the governing equations of deformable landslide derived in local coordinate or Cartesian coordinate? In most of landslide cases, steep slopes are involved in the bathymetry. If the landslide governing equations are derived in Cartesian coordinate, the assumption in regular shallow-water-type equations that the pressure gradient in vertical momentum equation is balanced by gravity force is not valid anymore. It needs non-hydrostatic pressure to represent the vertical acceleration of the landslide. If the landslide governing equations are derived in local coordinate and then transformed into Cartesian coordinate, the problem will not be there. The vertical acceleration perpendicular to the local bed is negligible.

   The governing equations of the landslide motion are derived in Cartesian coordinates. In some cases where steep slopes are involved, landslide models based on local coordinates allow representing the slide motion better. However, when general topographies are considered and not only simple geometries, landslide models based on local coordinates also introduce some difficulties on the final numerical model and on its implementation. Besides, the computational component is important.

   In this work, we focus on the hydrodynamic part, and that is one of the reasons for choosing a simple landslide model based on Cartesian coordinates. Of course, *the strategies presented here can also be adapted for more sophisticated landslide models.*

For example, in () a non-hydrostatic model for the hydrodynamic part that is similar to the one presented here for the case of a single layer was introduced. In the aforementioned paper, the authors study the influence of coupling the hydrodynamic model with a granular model that is derived in both coordinates references: Cartesian and local coordinates. The front positions calculated with the Cartesian model progress faster and, after some time, they are slightly ahead compared with the local coordinate model solution (see, for instance, Figure 4 in ()). This is due to the fact that the Cartesian model uses the horizontal velocity instead of the velocity tangent to the topography. In any case, the differences between the two models are not very noticeable.

A granular slide model based on local coordinates might gives better results. However, when introducing a non-hydrostatic pressure, the model is closer to a 3D solver. In such a case, the influence on the reference coordinate system barely exists. That is the reason why in (), both non-hydrostatic models based on different coordinate systems show similar results.

In any case, although on the present work we focus on the hydrodynamic part, we can appreciate on the benchmark tests that the numerical results show a very good agreement with the lab experiments, despite the simple landslide model chosen here.

At the end of section 2, a paragraph has been added including reviewer's comment and our response to it.

2. What is the physical connection of the parameters in the landslide model to the material properties? How sensible is the change of these parameters to the landslide motion and induced tsunamis? In all three benchmarks, the internal friction angles and basal friction angles were. In the present model, another set of characteristic angles were used. The author should provide the relation between the angles used in the simulations and the real material properties.

   The parameters involved at each simulation are:

   $$g,\ r,\ n_a,\ n_m,\ d_s,\ \delta_i,\ \beta, \text{ and } \gamma.$$

   The parameters $g,\ r,\ n_m,$ and d_s are related to physical settings given at each experiment. β and γ are empirical parameters that were chosen as in the seminal paper ().

   The friction angle $\delta_1,\ \delta_2$ are characteristic angles of the material, and δ_3 is related to the behavior of the slide motion when starting from the rest. Thus, the values of the angles depend strongly on the granular material. The three angle values were adjusted within a range of feasible values according to the references ((), (), and ()):

   $$\delta_1 \in [1^\circ, 22^\circ], \quad \delta_2 \in [11^\circ, 34^\circ], \quad \delta_3 \in [3^\circ, 23^\circ].$$

   In the present paper we have employed the values

   $$\delta_1 = 6^\circ, \quad \delta_2 \in [17^\circ, 30^\circ], \quad \delta_3 = 12^\circ,$$

   for the three benchmark problems which is consistent with the references. As noted in (), in general for real problems involving complex rheologies, smaller values of these parameters δ_i should be employed.

   With regard to the sensitivity of the model to parameter variation, an appropriate sensitivity analysis can be performed, as it is done in (). However, the aim of the present work was to prove if the non-hydrostatic model couple with the granular model was able to accurately reproduce the three benchmarks considered. A set *of tools can be developed to study the sensitivity to varying input parameters by adapting the ideas in (), but this is not the purpose of this paper.*

The parameter denoting the buoyancy effect is just a tuning parameter if the author cannot link it to the status of real landslide.

We agree. For field case problems, $r = 0.5$ *is usually taken, and then the parameter is tuned based on available field data. In general, the complexity of the rheology introduces a difficulty that is always present on the modelling as well as on the tuning of the parameters. Moreover, the more sophisticated is the model (considering, for example, the rheology of the material), the more input data -that may be unknown- will be required.*

What is the model user to set up this parameter If the user does not know any field or experimental observations ahead of time?

Although the final user has to adjust some model input parameters, within a range of acceptable value (such as the aforementioned angle frictions), we remark the simplicity of the final numerical model proposed, and thus, on the efficient of its GPU implementation. This allows performing uncertainty quantification (see ()) on a few parameters, and investigating the sensitivity to them varying on small ranges (as in ()).

When field or experimental observations are available, a different approach is proposed in () where an automatic data assimilation strategy for a similar landslide non-hydrostatic model is proposed. That strategy can be also adapted for the present model.

Finally, we would like to emphasize on the simplicity of the granular model, the extremely overall efficient implementation, and the good results obtained for representing both water and granular motions. The very good computational performance achieved by the proposed numerical model allows the possibility of considering, for future works, data assimilation, uncertainty quantification or performing sensitivity analysis on the parameters.

At the beginning of section 5, just before presenting benchmark results, a paragraph has been added, containing the answer to the second main point raised by the reviewer and describe here in our answer.

Minor suggestions:

1. I am not sure if the authors use of the term "two-phase model" is correct or not. Most of the time only the model solving both the fluid phase and solid phase of the landslide is called "two-phase model".

   In this paper we have two phases: water and granular material. Therefore, we consider two phases in the sense that two different fluids are modeled. However, in fact there is no mixing between them.

   *We have found in the bibliography this terminology used exactly in the same way. For example, just to mention one: in two initial sentences of the Introduction of Drew (1983) “Mathematical model of two-phase flow" in Ann. Rev. Fluid Mech.(*), you can read "Dispersed two-phase flows occur in many natural and technological situations. For example, dust in air and **sediment in water**..."*.

   (*) `https://www.annualreviews.org/doi/abs/10.1146/annurev.fl.15.0`

   *After many readings of reviewer’s comment, and not really understanding his/her point, and after thinking about it, we think that the reviewer is meaning that "**Most of the time** only the model solving **OR** the fluid phase **OR** the solid phase of the landslide is called "two-phase model". (with BOTH in the sentence we do not really understand the question). We have looked for all the appearances of the term "two-phase model" in the paper and it appears four times. Two of them (at* *the beginning of section 3 and in the conclusions) where, to our understanding, they perfectly fit the meaning of a two-phase model. The other two times, is true that the term is referring only to one component of the coupled system. In these two appearances, the term two-phase (in lines L195 and L219 of our current version) has been removed.*

2. In Line 54 the authors mention particular difficulty of the BP6 simulations. If they do not say somewhere what the difficulty is, they can simply delete the words in the brackets.

   Done. We have delete the words in the brackets, as suggested.

3. Both BP5 and BP6 provide the slide shape at different time in the experiments. I would recommend the authors to compare the slide shape of simulation results to the observations in Figure 4 and Figure 8.

   In () some snapshots of the landslide evolution are shown at different time-steps that can be compared with Figure 4 corresponding to benchmark 4. It can be seen that the location of the landslide front is well-captured, but there is some mismatch of the landslide shape at the front, mainly due to the simplicity of the landslide model considered here. In particular, we consider that the density remains constant in the landslide layer during the simulation, this is not true due to the water entrainment into the slide material.

   A similar comparison can be made between Figure 8 (BP5) and some snapshots of the landslide evolution that can be found in () for the benchmark problem 5.

   As far as we know, no data is publicly available on the evolution of the slides, except for the aforementioned snapshots that can be found in (;) or in the website (). Using these figures would require asking for permission to use them. Instead, we have included some comments about this in the final version of the paper for the interested reader that could check the cited references.

4. The symbol r is used twice.

   It is true, but extremely difficult to avoid such coincidence as both symbols are really standard notations. One denotes the slide liquefaction and the other the wave gauge locations. We have changed this second r to R for Radius (hopefully not appearing anywhere else in the present work).

5. Some typos: 2020a in line 17; duplicate sentences from line 276 to 280.

   Both typos have been corrected.

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