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Interactive comment on "An efficient two-layer landslide-tsunami numerical model: effects of momentum transfer validated with physical experiments of waves generated by granular landslides" *by* Martin Franz et al.

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Specific comments:

We agree with the statement that it would be appropriate to discuss the used computer resources, the computation time of typical run, the cell size etc. These aspects will be presented in the section 3.

L19(L22): We will add "partially submerged".

L38 & L41: As answered to anonymous referee #1, a more detailed discussion about models based on RANS equations (Reynolds averaged Navier-Stokes equations) will be provided. The enhancement of this discussion will also clarify what we call “full-3D” or “less classical hybrid approach (which will be rephrased)”. A more appropriate term will be used. Nevertheless, while this paragraph will be more detailed to better capture the context, we won’t present in detail the pros and cons of each approach.

L43: Indeed, predictive model can be of different nature (as described in the paragraph above). This sentence will be rewritten more clearly.

L 50: We will add a sentence that explain the approaches using drag equations. The “drag-like equation” term was used because there are different approach and simplification used in the literature that make not all of them fall under the definition.

L59: We will rename our approach as “rigid (discrete) collision” as the term “perfectly elastic” is misleading. Moreover, as only the velocity is exchanged (no change in mass and no deformation), in a very simple way, the approach remains very simple and is, in fact, correct in a physical point of view. The paragraph will be rewritten.

L86: The non-linear shallow water equations is in our point of view appropriate as it works well for tsunami propagation (as mentioned by Referee #2) and landslide propagation (as demonstrated in the manuscript and in literature, e. g. Hungr and Evans, 1996). The interaction between these two layers is the core of the paper and we think it performed well, as demonstrated through the effect of the momentum transfer on the landslide deposit and the generated waves.

L120: It is true that cannot be named “relative density” in this context. We will rename it with a new term such as “buoyant density”. The concept of relative density appears when it is divided by s.

L163: We will provide a more detailed explanation in the final paper. This is justified because using this approach, there is no special need to introduce new equations for wet-dry transition.

L184: We will rename our approach as “rigid (discrete) collision” as the term “perfectly elastic” is misleading. Moreover, as only the velocity is exchanged (no change in mass and no deformation), in a very simple way, the approach remains very simple and is, in fact, correct in a physical point of view. Eq. (31): In fact, the maximum slide thickness is not a manual input. During the simulation, the landslide maximum thickness is tracked (and registered) in the numerical equivalent location of the Cam1 (Figure 1). In the case of a real scenario, the same principle will apply. The operator only need to point out where this value is numerically measured.

L226: It is sure that this paragraph is not strategically placed in the manuscript. We will either integrate it in the discussion or remove it.

L250: In this paper, we present the comparison between our model and a specific physical experiment. It shows that when we implement exactly the measured rheological parameters in our model (for granular flow), the behaviour of the landslide is correct. Moreover, speaking about the dry case, our model was by no mean modified to fit the physical experiment. The isotropic Coulomb rheology was used and the friction angles were implemented, that’s it. Thus, we think that it is a robust approach to validate a numerical model. While we won’t discuss the scale effects on the physical experiment conducted in Miller et al. (2017) here, the fact that they used slide masses greater than 500 kg (while Kesseler et al. (2020) investigated side masses between 1 and 110 kg), place this particular study on the “positive” side. Back analysis of real cases are also a way to validate numerical models, but “playing” with the input parameters and tuning the code was not the topic of this paper. We have already used our model in real case study, either prospective or in back analysis, and showed good capacities. Unfortunately, at this time, the code was not in its actual state (e. g. the momentum transfer was not implemented). (e. g. Franz, M., Rudaz, B., Jaboyedoff, M. & Podladchikov, Y. (2016). Fast assessment of landslide-generated tsunami and associated risks by coupling SLBL with shallow water model. Proceeding of the GEOVancouver

2016 conference; Franz, M., Jaboyedoff, J., Podladchikov, Y. & Locat, J. (2015). Testing a landslide-generated tsunami model. The case of the Nicolet Landslide (Québec, Canada). Proceeding of the GEOQuébec 2015 conference). The use of the present state model in real case scenarios will be presented in a future paper to address this crucial point, on which we agree.

L258: The Voellmy rheology is widely used to simulate snow and rock avalanches. The parameters used in prospective study are often based on regional back analysis cases (e. g. Hungr, O. & Evans, S.G. (1996). Rock avalanche runout prediction using dynamic model. Landslides.).

L271: a) We won't use the "same order of magnitude" expression. b) Yes, the mass is conserved in the numerical model. The difference in fig. 5 are due to an incomplete graph (stops at 3.2 s) and the fact that thickness of the landslide in the physical experiment is expended (bouncing beads). This is not reproduced numerically.

L323: We think that the scale effect is not dominant in this case. This is highlighted by the fact that fairly good correspondence between the laboratory and the numerical simulation are present over and under the 0.2 m threshold Referee #2 mentions.

Fig. 9: The suggestion to use the nRMSE to judge the agreement between laboratory and numerical results is welcome. We will apply that. Fig. 9: We don't think that the lack of frequency dispersion is a complete weakness of the approach. Nevertheless, we will add a detailed discussion on the topic.

L 343: The link between the wave trains and the frequency dispersion will be discussed.

Figure 11: The data of the numerical simulations are indeed a little lower (smaller Am) than the lab data and it would have been beneficial for hazard assessment to be in the opposite case. Nevertheless, they stay within the +/-30% deviation from the Eq. (39) which is considered as a good fit (Heller et al., 2010).

L381: The dashed lines are strictly the Eq. (39) +/- 30%. It is obvious, for instance, if we check the plain line at Am = 3. The upper dashed line is at Am = 4 (3+(3*0.3)) and the lower dashed line is at Am = 2 (3-(3*0.3). Therefore, as the data are within the dashed lines (except for 0.05m) they are within +/- 30%. The maximum relative wave amplitude Am is a function of the impulse product parameter P. We also should keep in mind that the figure 11 uses the same axis lengths as in Miller et al. (2017). The figure presented in Heller et Hager (2010) has the P axis that stops at 9, which means that the relationship from Eq. (39) is not valid after this point. Nevertheless, the numerical data for a water depth of 0.05 m is not so distant from the laboratory data.

L384: While it is true that the model cannot handle breaking, it is not true that it cannot handle the impact crater, as long as the steepness of the water surface is not steeper than “sub-vertical”. We will mention this aspect in the final paper.

L388-293: The argumentation will be corrected, in parallel with the correction regarding Fig. 9.

L403: We will begin the chapter with a paragraph concerning the motivation and the methods of the study.

L410: We will clarify and update the Conclusion. This will be link with all the enhancement throughout the manuscript.

L416: This will be corrected.

L 417: As discussed for the L381, if we look at the data for a P smaller than 9, the fit is good. Moreover, looking at the figure depicting Am and P relationship in Heller and Hager (2010), we see that the data are contained between the +/- 30 % lines. It is the case of the data from our numerical model. Except for h=0.05m, which is anyway for a P greater than 9. Suggested grammatical corrections and minor points:

L220/1: We will rephrase. Indeed, Am is not equal to 0 in Miller et al. (2017). Here we plot the deviation from Am in Miller et al. (2017). We mean: “The value used in Eq. (33) (red line) is not the best fitting curve” in the panel a). But they are the best compromise for a) AND b).

All the other suggested corrections and improvements will be done.

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