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Interactive comment on "Role of friction terms in two-dimensional modelling of dense snow avalanches" *by* Marcos Sanz-Ramos et al.

Anonymous Referee #2

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This paper aims to assess the role of friction parameters, notably cohesion, in snow avalanche dynamics simulations. Besides an analysis of the respective contributions of the different friction terms, numerical results are compared to physical data for three test cases spanning different scales, from lab experiments to a large-scale chute and to a real avalanche case. I would certainly agree with the authors that systematic studies to better constraint the use of avalanche models are strongly needed, in particular for hazard assessment applications. The models currently used in the community need stronger validations and benchmarking (see e.g., Issler et al., J. Glaciol., 2018), and the presented study offers interesting insights along this line.

Unfortunately, the paper does not do full justice to these valuable objectives, and thorough revisions would be needed to meet the required standards of scientific publica-

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tions. Needed improvements concern several axes: (1) Better description of the conditions and parameters used in the simulations. Currently, one would certainly not be able to reproduce the obtained results with the information provided. (2) More in-depth physical discussion of the results, notably in regards to the relations between friction parameters and snow quality (wetness in particular). This issue, which is practically not covered in the paper, would probably constitute the most important takeaway of the paper from a snow science perspective. Without such discussion, the presented results remain essentially formal, and drawing general conclusions applicable beyond the selected test cases appears difficult. (3) Clarification of numerous unclear sentences and statements throughout the manuscript. (4) Improvement of several figures and captions.

I provide below a detailed list of main and technical comments, intended to help the authors in this revision task. Among these, comments 15 to 22 concerning the physical discussion of the results, are probably the most important.

Main comments

1/ Introduction. The literature review on numerical models for simulating avalanche propagation needs to be completed. Models based on Voellmy-Salm friction law or variants have been developed by numerous groups, e.g. among others (far from exhaustive list): Naaim, M., Durand, Y., Eckert, N., & Chambon, G. (2013). Dense avalanche friction coefficients: Influence of physical properties of snow. J. Glaciol., 59(216), 771-782. Naaim , M., Naaim-Bouvet, F., Faug, T., Bouchet, A. (2004). Dense snow avalanche modeling: flow, erosion, deposition and obstacle effects. Cold Reg. Sci. Technology, 39(2–3), 193-204. Sampl, P., Granig, M. (2009). Avalanche Simulation with SAMOS-AT. International Snow Science Workshop, Davos 2009, Proceedings. Pudasaini, S. P., and M. Krautblatter (2014). A two-phase mechanical model for rock-ice avalanches, J. Geophys. Res. Earth Surf., 119, 2272–229.

2/ Eq. (3). Strictly speaking, entrainment and deposition during the flow influence not only the mass conservation equation, but also the momentum balance: see e.g. Naaim et al, Surv. Geophys., 2003.

3/ The cohesion model used in the paper (Eq. (6)) is pretty complex, and was obtained from fitting a limited number of data (Bartelt et al., J. Glaciol., 2015). Did the authors also consider simpler models, such as a constant cohesion (which would also be consistent with the data)? The current model produces an abrupt drop in the cohesion contribution to shear stress for low depth values (as seen in Fig. 4). Does this abrupt drop play an effective role in the simulation results? A dedicated sensitivity analysis of this issue would certainly be useful.

4/ The whole section 2.2 on numerical schemes, including Fig. 5, is pretty difficult to follow. I would suggest either providing more details and explanations in order to have a really self-contained presentation of these issues (maybe in a dedicated appendix), or either removing this section altogether and referring the readers to previous publications in which they can find the relevant information.

5/ P.6, l.159. The wet-dry limit is mentioned here for the first time, without being properly defined before. Since this numerical parameter appears to play an important role, as later discussed in section 4.1, it would need to be introduced earlier in the paper. The criteria used to select the value of this dry-wet limit in the different application cases should be explained.

6/ Sections 2.3.1 and 2.3.2. The initial conditions used in the simulations of case 1 (Hutter experiments) would need to be described more precisely (initial geometry of the granular mass). Similarly, the way the authors deal with the lateral walls of the channel (boundary conditions), for case 1 as well as for case 2, should be explained. From Figure 7, it seems that no lateral variations are observed in the simulation results (quasi-1D flow). Is this true? What is the added-value of using a 2D model in this case?

7/ Sections 2.3.2 and 2.3.3. The main characteristics of snow used in the experiments of case 2 should be recalled. In particular, the liquid water content is an important information for discussing the cohesion values later employed in the numerical simulations (see also comment 22). Same remark for case 3: can the authors provide information regarding the quality of snow involved in the simulated avalanche?

8/ P.9, l.210-211. It is doubtful that slush flows would be characterized by large values of the friction coefficient mu. In fact, the rheology slush flows is frequently assumed to obey viscoplastic models, i.e. without a friction contribution (e.g., Jaedicke et al., CRST, 2008). Hence, mentioning friction stresses up to 11,000 Pa for slush flows appears irrelevant.

9/ Section 3.1: Besides discussing the individual contributions of friction, "turbulence" and cohesion to the stress, it would be instructive to cross-compare these different contributions between one another. Figures showing which contribution dominates the overall behavior depending on flow height and velocity, typically, would certainly be interesting.

10/ Section 3.2: How exactly are the rear and front positions of the avalanches extracted from the simulations? Are the definitions used for these positions comparable with those employed in the study of Bartelt et al. (J. Glaciol., 1999) used as a reference?

11/P.14, l.296-300. The criteria to select the different simulations "that better approximate the observed results" should be clearly explained. Is the matching based on runout, flow height, flow velocity? In particular, one can expect the correlations found between the different friction parameters (Eqs (7) and (8)) to strongly depend on the number and choice of these criteria. What is then the robustness of these correlations? Don't they simply reflect an insufficient number of matching criteria?

12/Section 3.3. Still on the correlations between friction parameters: if the authors can demonstrate some general relevance to these correlations, the ranges of validity of relations (7) and (8) would need to be clearly mentioned. I do not understand what is meant by a "good adjustment even for values that were out of the already reported

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range" (l. 303-304). Wouldn't it be possible to use similar functional forms (either linear or logarithmic) for adjusting the results of the two experiments? If not, are there any differences between the two experiments, in terms of physical characteristics, snow type, etc., that could explain these different results?

13/ Section 3.4. Please explain how the three scenarios analyzed in detail were selected.

14/ Section 3.4. The discussion of Figures 9 and 10 is not really clear. Are these two figures obtained with different models? Or just with different parameters? The authors also mention the "use of summer topography" as a possible explanation for the differences observed between the two figures. However, the actual topography used in the modeling is never indicated. And why using a different topography in the two cases? Finally, for the sake of comparison, it would be interesting to show velocity results also for the cases represented in Figure 9.

15/ Section 4.1. Besides the continuum assumption, one of the main assumption involved in 2D-SWE-based models is the shallow-flow assumption. The relevance of, and limitations implied by, this assumption would also need to be discussed in view of the different test cases considered in the paper.

16/ Section 4.2. Considering values of K_p different from unity allows one to consider anisotropic normal stresses in the material. The vertical stress does however remain "hydrostatic", ie linear with depth. I suggest modifying the title and discussions of this section accordingly.

17/ Figure 12. What is the friction law considered for water in this example? And what is the interest of only considering turbulent friction for the "snow" flows in this part? Since the comparison with water seems to add nothing to the discussion, I would actually suggest only showing results obtained for snow, with typical values of mu and xi and different values of K_p.

18/ Section 4.2. While the discussion concerning the capability of the model to represent block-like motion with low values of K_p is certainly interesting, the physical significance of such low K_p values would also need to be discussed in view of, e.g., classical active / passive theory in soils.

19/ P.22, l.435-437. The fact that Iber reproduces measured velocities better than Bartelt et al.'s model, is really not obvious in Figure 13. To me, both models actually appear to show considerable discrepancies with the measurements.

20/ P.22, l.438-439. The fitting performed on the volume of the avalanche should be clarified. If the flow volume considered in the two models is different, direct comparisons between the obtained results appear to loose much of their meaning.

21/ Section 4.3. The whole discussion about the possible relation between xi and Manning coefficient / roughness does not appear very relevant for avalanche applications, especially since a large part of the terrain roughness can be expected to be smoothed out in winter. The proposed analogy appears to be of little practical use, unless the authors can provide clear indications about the scale of the roughness to be considered.

22/ Section 4.3. Only a brief physical discussion of cohesion values is provided in this section, while this issue actually appears to me as the most interesting for avalanche applications. It is generally considered that dry snow can be represented as cohesionless, and that cohesion becomes important only for wet snow (e.g., Bartelt et al., J. Glaciol., 2015). However, in their simulations, the authors apparently applied cohesion values irrespective of snow quality. If the considered test cases only involve dry snow, one could question the relevance of including cohesion in the model. Can the authors provide arguments as to why cohesion would be needed also for dry snow? I strongly urge the authors to try and examine the role played by cohesion as a function of snow quality, and to add test cases involving wet snow if none is currently present.

Technical issues

- P.2, l. 42, “However, the effects of the friction model on the individual terms of the equations. . .” Unclear statement. Please consider rephrasing.

- P.3, l.70. Sentence is ambiguous, since dU/dt is also an inertia term.

- Different notations and decompositions are used throughout the paper for basal friction: tau_d, tau_t, tau_mc, etc. in eq. (2); S’_rh, S”_rh in eq. (4), tau_mu, tau_xi later on. This unnecessarily complicates the reading. Please homogenize these notations.

- P.3, l.83-84, and later. In fluid mechanics, pressure is generally defined an an isotropic component of the stresses. Hence, one should rather speak of non-isotropic normal stresses when K_p is different from unity.

- P.3, l.79-81. Related to the previous comment, the sentence starting by “Thus, if for water flow . . .”, is not very clear.

- P.7, l.183. What is meant by “(stable condition)”?

- P.7, l.188. It would be useful to also indicate the total volume of the simulated avalanche.

- P.12, l.260. Typo: xi instead of mu.

- Figures 5 and 6. It would be clearer to use similar symbology in both figures, i.e. avoiding representing simulation results with discrete points in one figure and continuous curves in the other.

- Figure 5. The caption mentions different combinations of mu and xi, while only the value of xi is varied in the displayed results.

- Figure 6. The fact that very similar results are obtained with significantly different combinations of mu and xi appears surprising, and would certainly deserve to be commented in the text.

- P.12, l.268. “Bartelt et al., 1999, used a 2D model in the vertical.” This formulation is not very clear, as both Bartelt et al.'s and the present study use a depth-integrated model. The model of Bartelt et al. could be described as 1D (or 1.5D), whereas the present model is 2D (or 2.5D).

- P.12, l.270. Among the differences with the model used by Bartelt et al. (1999), one should also mention the use of anisotropic normal stresses with active/passive coefficients. In contrast, and although this is not clearly indicated in the paper, the authors only considered isotropic normal stresses for this application case. Can this difference explain the different behaviors observed in the results?

- Figure 7. Figures 7b and 7c are not very clear. A horizontal scale should be indicated. What do the different black lines represent?

- Figure 7. The exact definition of the "inertial forces" represented on Figure 7c should be given.

- P.15, l.308. What is meant by "a uniform estimation of the parameters throughout the model"?

- P.15, l.323-333. The fact that three scenarios are described in more detail should be explained prior to this paragraph. Otherwise, the transition with what precedes is hard to follow.

- P.17, l.342. Sentence starting with "Figure 10a shows the slope vectors" is unclear.

- Figure 12. To what do the different curves correspond? Different times? This should be explained.

- Figure 13 and related text. The values of mu and xi used in Figs. 13a should be indicated. Same for the value of mu in Figs. 13b and 13c. Also, the value of K_p indicated at the top of the right column appears to disagree with the caption and the text.

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