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Natural Hazards and Earth System Sciences
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Discussions

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

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Author's response to the referee #2 comments:

GENERALITIES:

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

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Discussion paper

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 publications. 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.

ANSWER:

The authors want to thank Referee #2 the time and dedication for reviewing the manuscript, besides sharing our vision regarding the necessity to carry out this kind of studies to improve the hazard assessment due to snow avalanches. The reviewer mentions four axes in which several improvements are required according to his expertise and opinion. The four axes involve mainly work of better description, more in depth discussion, clarification of sentences and figures and captions improvements. The authors are sure that these requirements can be fulfilled in the final version of the manuscript, and expect that the answers to the specific main comments and technical issues that follow will help to illustrate that.

MAIN COMMENT 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 SAMOSAT. 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.

ANSWER: We want to thank the bibliography provided. We will thoroughly review the advances these documents represent and refer to them in the introduction, as well as follow them for other relevant literature, enlarging the introduction if necessary. A first view of the provided references shows that they are certainly of interest. In Naaim et al. (2004) a three parameter friction model is proposed, but in Naaim et al. (2013) the same authors perform a calibration using the Voellmy model (referring to it as “the standard two-parameter model”) for an avalanche path in Chamonix valley. A detailed discussion of the Coulomb-Voellmy model can also be found in Pudasaini and Krautblatter (2014). Here, the authors remark that Coulomb-Voellmy type models are based on effectively single-phase dry granular flows, and as their purpose is to develop a two-phase model for rock-ice avalanches, they end using a model mainly based on friction angles. A difference approach is used by SAMOS Sampl and Granig (2009), which uses the SPH approach for the dense flow layer, with a bottom friction made of different terms which includes a yield stress, a Coulomb friction term affected by a fluidification factor, and a turbulent term. The 2D SPH approach assimilates each particle to a cylinder of height the avalanche depth, in comparison with the classical SPH approach where the particles are spheres (López et al., 2010; López Gómez et al., 2015) as in DAN3D (Schraml et al., 2015).

MAIN COMMENT 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

ANSWER:

We totally agree. Certainly, the entrainment of new snow, with zero momentum, slows down the flux of the bulk. Anyway, in the document the entrainment is only mentioned in the introduction to present the context of the paper. About deposition, as the model is depth averaged, it takes place when in on element the total snow column has stopped, and as the governing equations are mass and momentum conservation, this happens when momentum becomes null. We have already started working in including snow entrainment in our model in the context of a Master Thesis (Castelló, 2020).

MAIN COMMENT 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.

ANSWER:

Certainly the cohesion model proposed by Bartelt et al. (2015) is quite complex, but we decided to include it in the model in order to assess its performance and its role in the frictions terms. We did not considered another cohesion model, as suggested by the Referee #2 (constant cohesion), but it is a quite interesting suggestion for future works in order to develop "simple" 2D-SWE models, in agreement with Salm (2004) and as we mentioned in the Referee's #1 comments (https://doi.org/10.5194/nhess-2019-423-AC1).

The abrupt drop of this model is more abrupt for low values of the depth, but also for low values of cohesion (C). For values of C lower than 250 Pa, the cohesion contribution in the friction terms is only linearly dependent on the Coulomb friction coefficient (mu), so a model based on a linear approximation as a function of mu, could be also a good approximation. As we observed, the model proposed by Bartelt et al. (2015) plays an important role in the snow avalanche tail definition as it affects the stopping moment of the avalanche when its depth decreases. However, the general contribution of this friction model is quite limited in comparison with the other two terms (Coulomb friction and turbulent friction) due to the range of application of the parameters involved as we indicated in the last paragraph of section 4.3 of the manuscript.

MAIN COMMENT 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.

ANSWER:

Following the suggestion of the Referee #2, and also to satisfy the interest of the Referee #1, we will include a full description of the numerical scheme as a supplementary material. Additionally, we will re-write this section, making it lighter, focusing on a conceptual description of the numerical scheme, but relegating the detailed mathematic expressions to the supplementary.

MAIN COMMENT 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.

ANSWER:

We will modify the manuscript accordingly to include an extended definition of the wet-dry limit described in the Referee's #2 general comment 2. If when doing that the document exceeds the recommended length, we will include that as supplementary material. The criteria to select the wet-dry limit is mainly user-knowledge based, but it should consider the particle size that defines the flow. For this reason, we used a wet-dry limit equal to 0.003 m in the case study 1 (section 2.3.1), which is equal to the material size, and 0.01 m for the other cases because it is good enough for representing the extension and propagation of the flow. In any case, the model is able to simulate the flow propagation without numerical instabilities even using very low wet-dry limits (see for example Cea Gómez et al. 2020; Cea et al. 2007; Cea and Bladé 2015; Sanz-Ramos et al. 2018). The effect of the wet-dry limit for snow avalanches, is more on the results of the avalanche tail extension and thickness than on its propagation velocity, maximum depths and thus avalanche hazard. It is well known that for hydrological studies (with water) the effect of the wet-dry limit is more on the drying process than on the flood propagation (Bates and De Roo, 2000; Cea et al., 2007; Cea and Bladé, 2015; Grimaldi et al., 2018), which is consistent with the results for avalanches.

MAIN COMMENT 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?

ANSWER:

For the experiment #117 (case 1) of Hutter et al. (1995), a constant elevation of snow was imposed on the release area warranting the same volume of the released material in both cases, physical and numerical experiment. We will modify the final version of the manuscript to clarify this aspect.

Regarding the wall conditions, in both cases we used “no-friction” conditions because the wall has a limited effect in comparison with the bottom roughness effect. However, Iber can compute the wall-boundary effects (see for example Bladé et al. 2014a; b; Cea and Bladé 2015).

Due to that, we decided to simplify the geometrical discretization of the Cases 1 and 2 in a 1D-mesh because the flow behavior is mainly 1D. In these cases, the lateral spread of the avalanches is negligible, so no important differences in the time-position relation of the avalanche are expected. For this reason we use a 1D mesh discretization, in which Iber solves the 2D-SWE but the Y component is zero.

MAIN COMMENT 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?

ANSWER:

The aim of these tests is to calibrate the numerical model testing several combination of the friction terms in order to reproduce the study case, also assessing the friction terms and its effects in the flow propagation.

On one hand, for Case 2 (section 3.2.2), Dent and Lang (1980) only indicated that the snow “[. . .] had been sifted through a 6 mm wire mesh [. . .]”. No additional information about the snow characteristics has been found for Case 2. On the other hand, the previous snow profiles and the post-avalanche study of the Case of Pal suggest that the snow was quite dry (no tests post-avalanche were performed). This avalanche occurred with relatively low temperatures (around -11oC) and a “quite cold” snow. The cohesion can be considered to be medium because the limit of the deposition area was clearly defined and some snow-aggregates were observed during the characterization of the avalanche (see attached Figure 1). The avalanche may be classified as a dense and dry with cohesion. However, the sudden change of the slope due to the road probably changed the avalanche properties from a dense to powder. The following video, which shows a controlled avalanche at the Coll de la Bonaigua in 2010 (Catalan Pyrenees range, northeast Spain), aims to illustrate this effect: https://www.youtube.com/watch?v=5yO-PSTKxCY. For this reason, as indicated, we tested several combinations of the friction parameters. As Referee #2 indicates, knowing the properties of the snow (liquid content, among others) can help to compare the results of the numerical simulations. For this reason, we will provide the characterization of the avalanche (Coll de Pal) as Supplementary material.

The authors want to notify a misprint when referencing the Case study 2. The correct reference of this experiment is Dent, J. D., and Lang, T. E. (1980). “Modeling of Snow flow.” Journal of Glaciology, 26(94), 131–140. We will correct in the final version of the manuscript.

MAIN COMMENT 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

ANSWER:

Certainly, slush snow-avalanches should be treated with a different rheological model, such as a viscoplastic-based one. This fact is denoted in the results of the experiments of Platzer et al. (2007) when dealing with slush snow, in which the shear stresses are lower in comparison with wet or dry snow. We will re-write this paragraph accordingly as follows:

"[. . .] For high density values this limit can increase to 11,000 Pa. However, when dealing with high density snow, e.g. for slush snow that can reach a density of up to 750 kg/m3, the shear stress contribution is reduced (Jaedicke et al., 2008; Platzer et al., 2007a) because the fluid behavior can be similar to that of a viscoplastic fluid. [. . .]"

MAIN COMMENT 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.

ANSWER:

To carry out this cross-comparison and to represent this with a single graph involving the two or the three friction terms would be an excellent result, but it is a challenge, and even more if the figurer's objective is to clarify concepts. In terms of shear stress, on one hand, the Coulomb contribution and the cohesion contribution depend only on the flow depth and, on the other hand, the turbulent contribution depend only of the flow velocity. As we can observe in Figure 3, Coulomb and turbulent contribution are of the same order of magnitude for depths and velocities ranges from 0 to 2.5 m and from 0 to 40 m/s, respectively. Additionally, depth and velocity are correlated: high velocities imply low depths, and vice versa, for the same flow intensity. Indeed, for "fast" avalanches the turbulent contribution dominates versus the others, while "slow" avalanches are the two other, Coulomb and cohesion, which dominates the shear stress contribution. Some authors (see Bartelt et al. 2015) suggest that mu and xi may vary within the volume of the avalanche and over time as well, increasing the complexity of analyzing these friction models. Considering that, we analyzed the different factors individually in order to simplify the analysis, and we think that the provided figures (Figure 3 and Figure 4) indicate, in an understandable way, the contribution of each parameter to the avalanche behavior, which is the main aim of the document.

MAIN COMMENT 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?

ANSWER:

In this section we numerically reproduced and compared the experiment described by Bartelt et al. (1999) in which, among other things, the time-position of the front and rear part of the avalanche are assessed. This experiment is based on the laboratory experiments of Hutter et al. (1995) in which, using high-speed photography, the evolution of the avalanche longitudinal profile was defined allowing "[. . .] to determine the position of the avalanche as a function of time [. . .]". In the numerical experiments we extract the position of the front and the rear part of the avalanche at the same time steps by observing the first and last element with a non zero snow depth.

MAIN COMMENT 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?

ANSWER:

We carried out these simulations aiming to achieve good results in terms of the position of the leading-edge of the avalanche, mainly in the final position of the avalanche front at the deceleration area but also during the deceleration process (see Figure 7). Considering that, several combinations of the three friction parameters allowed to obtain a good fit between experimental and simulated results. The correlations (Eqs. (7) and (8)) extracted from the numerical results are an example of how using different parameters we can obtain similar results. This fact highlights the need to calibrate the numerical model, as well as the wide range of values of these parameters, besides combination of them, that allow to obtain very similar results. If field data had provided the values, or narrower ranges, for some parameters, the combinations to fit the final avalanche position would be less, but this case is a clear example of a practical case in which a calibration has to be carried out with the available parameters.

MAIN COMMENT 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 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?

ANSWER:

These correlations are extracted from two particular experiments, thus Eqs. (7) and (8) are valid for reproducing them in the range shown in the Figure 8. When we indicate "[. . .] the already reported range [. . .]" we refer to the range of values described at the beginning of the Section 3.3, which are: mu (0.1–0.3), xi (5,500–10,000 m/s2) and C (490–1,060 Pa). We carried out some other numerical experiments using these correlations, but for values out of the range previously indicated. The results shown also a good adjustment but with a lower R2. For that reason, at the end of this section we clarify that the application range valid for these experiments is "[. . .] limited to mu < 0.7 for Eq. (7) and for mu = xi = 0 for Eq. (8) [. . .]". In any case, we will re-structure this section in the final manuscript to clarify how the equations were obtained and form which data.

Regarding the functional forms, we did not find any particular reason for experiment 1 and experiment 2 to be adjusted by a different functional form. No differences between the experiments are reported in terms of snow properties. The main difference was the terminal velocity achieved at the beginning of the deceleration area (12 m/s for Exp. and 18 m/s for Exp. 3). This fact probably modified the properties of the snow and, consequently, the relation between the parameters.

MAIN COMMENT 13:

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

ANSWER:

Once the 27 scenarios had been analyzed, we checked the fitting of observed data with the simulated in terms of run-out distance and amount of snow cumulated on the road. We finally selected 4 of 27 simulated scenarios (A1B1C2, A1B3C3, A2B2C1, A2B3C2) because these 4 had a run-out distance around 400 m and a snow depth on the road of around 2.4 m. The authors want to notify a misprint in the run-out distance, the correct one is 400 m (not 500 m such as indicated in the manuscript). We will modify the final version of the manuscript to correct this misprint and to clarify the selection of the analyzed scenarios.

MAIN COMMENT 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.

ANSWER:

The numerical model was the same for all scenarios (topography, initial conditions, etc.), we only modified the friction parameters. Figure 9 shows the final position of the avalanche for the 4 scenarios selected, which are discussed in the paragraph that starts in L323. Figure 10 shows the results of another parameter combination that follows the recommendations of Bartelt et al. (2017), besides the main directions of the topography (Figure 10a). For that, we use free topographical data available from the Catalan administration (Institut Cartogràfic i Geològic de Catalunya). The case study of Pal was not a “full-depth avalanche”, so we think that it is important to highlight that the topography commonly provided, also used in this case, represents the “summer” topography, i.e. the elevation data without snow pack. In some cases, this fact could condition the flow propagation because the topography can be smoother or sharper depending on the previous meteorological and snowy conditions.

We will re-structure this section to clarify the referee’s comments, also including the topographical source and the representation of the maximum velocity for the 4 analyzed scenarios (Figure 9).

MAIN COMMENT 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.

ANSWER:

When referring to the two-dimensional Saint Venant Equations as the “Shallow Water Equations”, the word “shallow “ refers to the fact that the flow is indeed 2D. This, when developing the SWE equations from the 3D RANS equations, is considered when performing the depth averaging of variables. Thus the implications are that vertical components of the velocity are lost, and that a uniform velocity in the depth, in the longitudinal and transversal direction, are assumed. This is an intrinsic hypothesis of the equations that we used, and many other authors have been using, and certainly it is very interesting to analyze in which cases an avalanche flow can be reasonably simulated with this kind of equations. For example, powder avalanches that have an aerosol behavior cannot. Very shallow dense snow avalanches in most cases can (and that is why we refer to dense snow avalanches in the document). Anyway, the aim of the article is not to determine these ranges of validity, which should be part of the first chapters of avalanche dynamic modelling manuals, but to contribute to understand the role of the different terms involved in the simulations once the option to use SWE based models has been taken, whatever the reason is (perhaps as simple as the access to the tool).

MAIN COMMENT 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.

ANSWER:

Linear pressure distribution with the depth, and hydrostatic pressure distribution are not synonyms. The general equation of hydrostatic pressure states that the gradient of the pressure (P) is proportional to the acting forces per unit of mass b(vector), being the proportionality constant the density of the fluid rho, that is rho X b(vector) = gradient(P). Thus, a value of Kp different than 1 means a non-hydrostatic pressure distribution, although it is linear. On the other hand, it is possible for the pressure distribution to be non-linear even in hydrostatic conditions (for compressible fluids). That is why we use the expressions "non-hydrostatic" and "anisotropic" along the document.

MAIN COMMENT 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.

ANSWER:

For the first example used in Section 4.2, we used a Manning-type friction law for water flow with Manning value equivalent to the turbulent friction coefficient obtained from Eq. (9). This part of the manuscript wants to highlight how using a 2D-SWE based model, even when modifying the pressure terms with a Kp factor, the flow has a water-like behavior. This only modification slightly improves the numerical representation at the first steps of the slab avalanches, but not the subsequent motion. We consider that this example contributes in highlighting the effect of the different terms of the equations on the avalanche movement, which is the main aim of the document.

MAIN COMMENT 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.

ANSWER:

The authors want to thank the referee suggestion. We agree with this comment and the necessity of comparisons with other theories. The physical significance of this low Kp can be inferred from the discussion in the document: at the first times steps, because of internal cohesion, snow moves as a rigid block. In a totally rigid block, as for example a solid, pressure is not transmitted through the mass, and thus Kp is zero. As snow is not totally rigid, Kp can be low but not zero, and indeed Kp should depend on the internal characteristics of the snow body (cohesion, layers structure, humidity, etc.). A comment in this line on this significance of the low values of Kp will be included in the final document. As the reviewer says, there is extensive literature on active/passive theory for soils.

MAIN COMMENT 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.

ANSWER:

Certainly, neither of the two models could fully represent the behavior of snow in the channel experiment presented by Bartelt et al. (2015). However, the numerical results presented by Bartelt show that the bulk arrives 0.5 s faster at the measured point, while in Iber this gap is reduced to 0.2 s and 0 s for Kp = 1. For Kp = 0.1 respectively (xi = 2000 m/s2). Additionally, the simulated avalanche by Bartelt did not follow a "block-like" behavior because the flow depth and velocity decreases smoothly. In this way, the results presented herein show a more "block-like" behaves, especially when using low values of Kp, because the majority of the snow pass through the measuring point before the 2.5 s. In any case, with this case study the authors wanted to show the benefits of using the Kp factor, and as we suggest at the end of the Section 4.2, more accurate observations and research are needed.

MAIN COMMENT 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.

ANSWER:

The initial volume is not stated in Bartelt et al. (2015), only “starting volumes” lower than 25 m3 are indicated in this reference. The original experimental campaigns were carried out by Platzer et al. (2007a, 2007b), who indicates that the initial volume was approximately 13 m3 for dry snow (Exp. 9 of Bartelt, used herein as an example for assessing different Kp values). So, we use this volume as initial condition. However, as we can observe in Figure 13a, the results of the shear stress were underestimated for both Kp evaluated, but specially for Kp = 1. For this reason, in order to achieve similar results in terms of shear stress we suggest that a greater initial volume can be required. We will modify the manuscript in order to clarify this aspect.

MAIN COMMENT 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.

ANSWER:

The authors think that the analogy presented could be interesting from the point of view of calibration of the numerical model, in particular for full-depth dense-snow avalanches and for rare, large avalanches that penetrate into the forest. This correlation (Eq (9)) could help in the selection of the value of the turbulent coefficient (mu) as a first approximation, or especially when using spatially distributed mu values. The selection could be even automatized in a similar way to what is done with the Manning coefficient in hydraulics, associating it to the land uses.

MAIN COMMENT 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.

ANSWER:

In the experiment presented herein, corresponding to Exp. 9 in Bartelt et al. (2015), a cohesion value of 396 Pa was used for all the simulations, the ones performed by the authors but also in the simulations presented by Bartelt. We did not discuss the necessity to consider or not the cohesion in this experiment, we just only applied it in order to compare the numerical results. In this section the analysis is focused in the relevance of Kp factor, not in the effect of the cohesion. However, the authors will consider the suggestion of the referee for future works.

TECHNICAL ISSUE 1:

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

ANSWER:

This sentence will be re-written as follows: “However, the effects of the friction model on the equations are commonly ignored.”

TECHNICAL ISSUE 2:

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

ANSWER:

The description of the governing equation will be re-written as indicated in the general comment #5 of the Referee #1 (see https://doi.org/10.5194/nhess-2019-423-AC1).

TECHNICAL ISSUE 3:

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.

ANSWER:

The authors want to remark that assuming the hypotheses of shear stress grouping (L74), the shear stress can be considered by different components. For example, the Voellmy friction model involves the turbulent term (τ_t (xi)) and the Mohr-Coulomb term (τ_t (mu)).

TECHNICAL ISSUE 4:

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

ANSWER:

The manuscript will be corrected accordingly.

TECHNICAL ISSUE 5:

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

ANSWER:

There is a misprint in L78. This sentence will be re-written as follows: “Thus, for water flow the shear terms due to friction are expressed by means of the friction slope [. . .].”

TECHNICAL ISSUE 6:

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

ANSWER:

We refer to “stable conditions” for those scenarios where the snow pack does not generate an avalanche because the terrain slope is gentle (< 28o) or the terrain is not able to keep enough snow pack (> 45 o).

TECHNICAL ISSUE 7:

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

ANSWER:

The total volume of the snow was not directly measured, but it is was estimated in approximately 2431 m3. The manuscript will be corrected accordingly.

TECHNICAL ISSUE 8:

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

ANSWER:

The manuscript will be corrected accordingly.

TECHNICAL ISSUE 9:

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.

ANSWER:

These figures will be modified accordingly.

TECHNICAL ISSUE 10:

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

ANSWER:

The caption will be re-written as follows: Figure 5. Comparison between the measured positions (r: rear; f: front) by Hutter et al. (1995) and the computed results using mu = 0.49 different values of xi for Exp. 117. The lines represent the results of the computed simulation by Bartelt et al. (1999).

TECHNICAL ISSUE 11:

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.

ANSWER:

These results reinforce the hypothesis suggested by the authors, in which it is possible to achieve similar results in avalanche modelling with very different combinations of the parameters involved. We will add the following sentences: "[. . .] can be observed (Figure 6). This figure only represents the results of the combination best fit to the experiments, achieving similar results with different parameters combination. After t =

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0.3 s, the velocity increases, resulting in a larger rear and front positions and further expansion of the avalanche. Small differences can be identified on the simulated rear part of the avalanche, whereas the runout of the front part decreased when mu and xi increased. [. . .]".

TECHNICAL ISSUE 12:

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).

ANSWER:

With this expression we colloquially use, we meant a "2D depth-averaged model", in which the depth averaging is performed "in the vertical" in comparison with 2D models that are averaged "in the horizontal" or in the flow width, as for example CE-QUAL 2D used for reservoirs. In the final manuscript we will use the standard, and appropriate, expression "1D depth-averaged model".

TECHNICAL ISSUE 13:

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?

ANSWER:

The differences could also be due to the consideration of the active/passive coefficient that affects the pressure terms in the model proposed by Bartelt et al. (1999). In this case study we aimed to achieve good results comparing with the experimental data, without considering any additional hypothesis such as anisotropic pressure.

TECHNICAL ISSUE 14:

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

ANSWER:

The Figure 7b and 7c represent a plain view of the numerical model. The black lines represent a distance gap of 0.1 m. The figures will be modified accordingly.

TECHNICAL ISSUE 15:

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

ANSWER:

The results of inertia are derived directly from Eq. (3).

TECHNICAL ISSUE 16:

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

ANSWER:

We use “uniform” to indicate that all the parameters are were considered uniform spatially and temporally.

TECHNICAL ISSUE 17:

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.

ANSWER:

The manuscript will be modified accordingly to properly introduce the scenarios that are analyzed in detail.

TECHNICAL ISSUE 18:

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

ANSWER:

This sentence will be re-written as follows: “Figure 10a shows the slope main directions of the terrain (vectors), which are in concordance with the RIT051 area (light blue polygon)”.

TECHNICAL ISSUE 19:

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

ANSWER:

The different curves represented in Figure 12 correspond to the evolution of the free surface (12a) and the inertia (12b) of the described experiment (L403). The experiment duration is 2 s and the results are plotted each 0.5 s. The water fluid is represented by blue lines and the snow-like fluid with kp = 1 and kp = 0.5 is represented by green dashed lines and brown dotted lines, respectively. The text will be modify as follows:

[. . .] Figure 12a shows the free surface evolution during the 2 first seconds. For snow flow, only turbulent friction (xi = 1,600 m s-2), equivalent to the Manning coefficient for water flow, was implemented, with two different Kp values (1 and 0.5). [. . .]

And the caption as follows:

[. . .] Effect of the Kp factor on the flow behavior of water (blue lines) and snow with Kp = 1 (green dashed lines) and Kp = 0.5 (brown dotted lines). Evolution of the free surface (a) and the inertia (b) during the first 2 s, with intervals of 0.5 s in a dummy case study that represents a dam break. [. . .]

TECHNICAL ISSUE 20:

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.

ANSWER:

The manuscript will be modified as follows: "[. . .] total shear stress (red line) simulated with Iber using the parameters suggested by Bartelt et al. (2015) (mu = 0.55 and xi = 2,000 ms-2). The Coulomb (mu) and turbulent (xi) contributions are also represented (dashed lines). Different tests were also performed considering different xi values of 250, 500, and 1,000 ms-2. [. . .]". Additionally, the misprint in the Kp for the figures 13b, 13d and 13e will be corrected accordingly.

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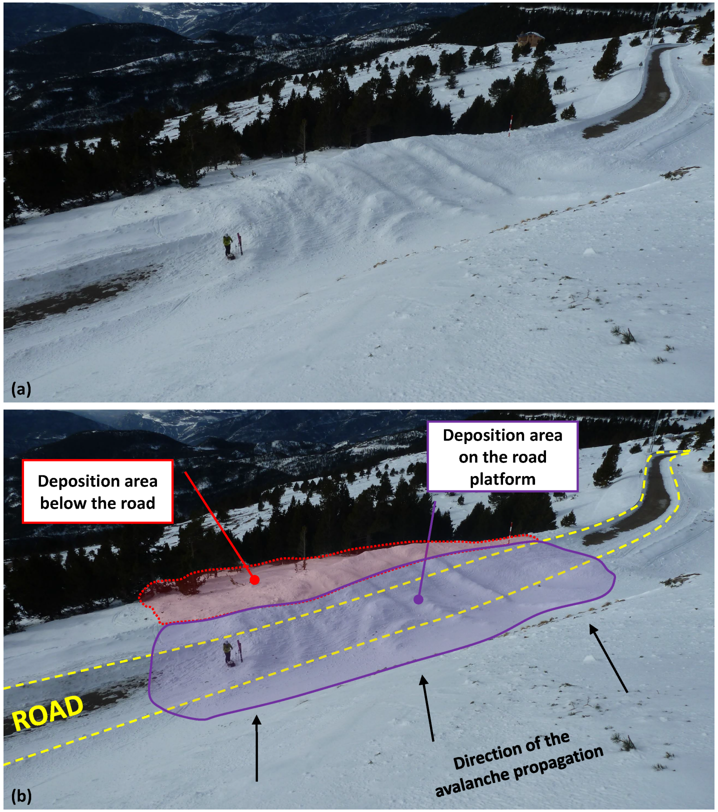

Figure 1. Photography taken few days after the event from the right side of the propagation area, in the upper hillslope of the road (a). The same image, where some aspects of the area and the avalanche have been highlighted.

Fig. 1.

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