Response to Reviewer #2’s comment on “Detrainment and braking of snow avalanches interacting with forests”

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We thank Referee #2 for his or her insightful comments and helpful advice, which help to improve the quality of our paper. The following provides our point-to-point responses to the general comments, specific comments, and technical corrections from the reviewer.

This manuscript discussed the snow avalanche protection efficiency of the forests with applying the 3D MPM combined with the modified cam Clay model. This approach developed by the authors’ group looks very powerful and promising to break through various aspects of snow avalanches from its initiation to dynamics. In actual, 3D MPM provides overwhelming useful information comparing to the previous depth averaged model. Further, once the model has been established it can be applicable under the various conditions. Taking into conceivable factors, the deduced relations in this study will be quite useful for the practical applications, I believe. Although it is out of scope here, I hope the authors proceed the research on the other aspect as well: how the forest stabilizes the snowpack and prevent from the snow avalanche release.

All the methodologies and discussions are well organized and carefully described. So, I believe this article is worth publishing. However, I am glad if the authors can clarify and improve some of the points listed below before the publication.

Reply: We thank this reviewer for the encouraging comments, and we appreciate the reviewer’s interest in the effect of forest on the stabilization of the snowpack, which is indeed very important especially for avalanche release. We have conducted preliminary simulations on this topic and will keep working on it. This has been added to the discussion of the manuscript.

In addition, we have further improved the manuscript thanks to the reviewer’s comments below.

Line 47: K is used for not only the detrainment coefficient but the material bulk modulus in eq. (5).

Reply: We thank the referee for noticing this issue. In the revised manuscript, we use K for the detrainment coefficient only, and we replaced the bulk modulus by its expression: $\frac{E}{3(1-2\nu)}$ with E the young’s modulus, ν the Poisson’s ratio.

Please check fonts of variables. They are not always unified in the text, the figure and figure captions: italic or vertical, such as spacing of trees “e”.

Reply: Checked and corrected.

Line 102: “CFL” means Courant-Friedrichs-Lewy Condition? Since it is not so common outside the numerical simulation field, the brief explanation is preferable, if you dare to use.

Reply: Yes. We have added more details in the revision.

Line 121: M=0.8 snow, defined as warm shear regime, agreed well with the field observations. On the other hand, Case 3 of M=1.2 is also set as the warmer case in Table 2. So, the Case 2 of M=0.8 snow correspond to the snow which is dry but the temperature is close to zero? I am curious whether you have got the information of snow properties by Feistl et al.? Are these reasonably agree with the physical parameters given in this study? In this study snow properties are designated with snow friction coefficient: M, the tension compression ratio: β, the hardening factor ζ, and initial consolidation pressure p0ini. Although the general features of snow defined with these parameters are explained briefly, it will be glad if the authors clarify the relations more specifically with the snow properties we usually observe in the field.

Reply: The snow heights behind the trees given by Feislt et al. (2014) are obtained from observations of 7 different avalanches (dry to wet). Their illustration shows some snow granules (Fig. 2e in the manuscript), which can correspond to snow of intermediate cohesion and friction. The snow properties in Case 2 are calibrated with the data reported by Feistl et al. (2014). Therefore, the snow in case 2 with M = 0.8 can be regarded as moist snow. Based on the calibrated snow properties in Case 2, we have further modified the snow friction and cohesion to get a relatively colder snow in Case 1 and warmer snow in Case 3. This has been clarified in the manuscript.

We recognize that due to the scarcity of snow triaxial tests (Desrues et al.1980; Scapozza and Bartelt 2003), it is difficult to calibrate different types of snow modelled with critical state soil mechanics as in this study. However, the snow properties in our model do have their physical basis and can be determined according to physical properties of snow and/or parametric study (Li et al., 2020). In particular, the snow friction parameter M can be linked to the internal friction of snow as $\phi = sin^{-1}\left(\frac{6M}{3+M}\right)$ (Sadrekarimi and Olson, 2011). The cohesion parameter β is the ratio between the tensile and compressive strength. Given the compressive strength as p_0, the tensile strength of snow can be obtained as βp_0. The hardening factor ξ reflects how fast the load increases with the deformation in the plastic stage. We have further clarified the snow properties in the revised manuscript.

According to our systematic study on the effect of snow properties on the avalanche behavior (Li et al., 2020), it was found that the tensile strength βp_0^{ini} and βM consistently increase from cold to warm avalanches, which suggests that these two terms control the different snow behaviors. By choosing Case 2 as a reference case, βp_0^{ini} =9 kPa, βM= 0.24, we study the influence of the snow properties by modifying this pair of parameters. Firstly, to reproduce the behavior of a dry snow, we decrease the tensile strength βp_0^{ini} =0 kPa and βM=0, this is the case 1 of our study, which corresponds to a cohesionless and low friction snow. And to reproduce the behavior of a wet and warm snow, we increase the tensile strength βp_0^{ini}=9 kPa and βM=0.36, this is the case 3 of our study, which corresponds to a relatively cohesive and frictional snow.

Further, do you have any idea how you can validate that the model is also appliable for the dry snow?

Reply: The snow properties of case 1 give the behavior of a dry cohesionless granular flow, whose free surface is continuous. It would be interesting in the future to compare this model with mass balances in the forest for a cold snow avalanche.

Table 2: The hardening factor ζ is set the same for both dry and wet snow. Is this reasonable?

Reply: We chose the same hardening factor ξ because there is currently not enough experimental data for its calibration for different types of snow. This parameter characterizes the rate of the hardening and its effect on the snow behavior depends on other parameters M, β, p_0^{ini}. We checked that this parameter does not affect the presented results but would affect the densification process (especially behind the trees). In the future, model parameters could be evaluated based on extensive triaxial tests for different snow types.

Figure 2: No snow depositions are found in (b) after the rectangular domain. Does it mean all the snow went through?

Reply: The view for the previous figures 2a&b&c was a side view of the wedge formed behind a group of 3 trees, the snow particles on the surface of the wedges are moving which leads to the confusion. We have changed to a view of a cross section passing through the center of the middle tree in order to have a better visualization of the shape of the formed wedges (Fig.R1).

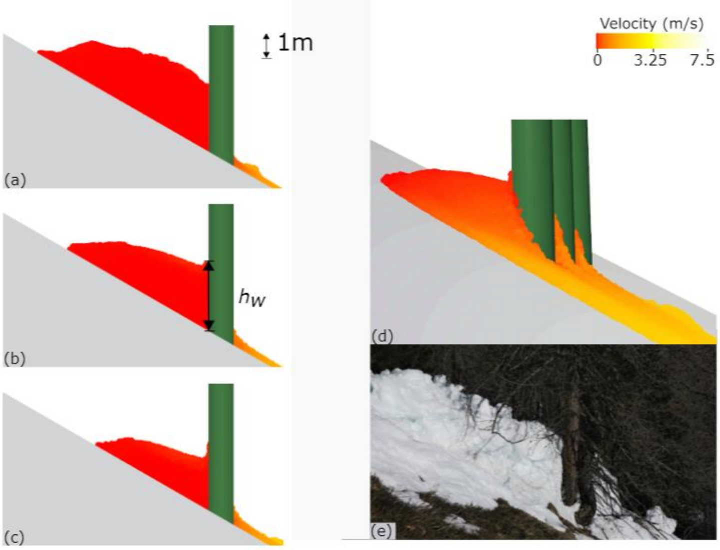

Figure R1. Cross-sectional view at the center of the middle tree of the snow stopped behind a group of three trees for different values of M: (a) M=1.2, (b) M=1, (c) M=0.8; accumulated snow behind a group of tree trees with a diameter of 1 m: (d) warm shear regime M=0.8, (e) observations (Feistl et al., 2014).

Table 2: Density of Case 1 which is explained as cold "dense" regime is still 200 kg/m3?

It is the same as other two warmer cases? I do have an impression that the density of the dry avalanche is usually lighter than the wet one. In particular, densities of granules and blocks in Case 3 are still 200 kg/m3? I wonder densities described here perhaps show bulk densities including air and snow?

Reply: Although the initial snow density is fixed as 200kg/m^3 for all the cases (Li et al., 2020, 2021), the avalanche density changes during the flowing process according to the properties of the snow and the hardening law. It has been further clarified that the 200kg/m^3 is the initial density of the snow.

In addition, as the adopted material point method is a continuous approach and each material point corresponds to a piece of snow instead of a real snow particle, the density is therefore the bulk density including air and snow. It has been further clarified in the revised manuscript that the density is the bulk density of snow.

Line 185: Although it does not matter much, I am a bit worried that authors use the word "impact" frequently. Since this article deals with the avalanche impact on the forest in actual, I prefer rewording such as "action", "affect" and "influence" to avoid the misleading.

Reply: Thanks for the reminder. We have replaced the word "impact" with "affect" and "influence" in the revised manuscript.

Figure 6: Eq. 3.5 and 3.4 in the figure must be Eq. 8 and 9.

Reply: Revised.

Is it just a coincidence that Case 1 maximum and Case 2 final agrees quite well? Further, please specify the reason why Case 3 is not shown here.

Reply:

i) We thank the reviewer for this remark, we have further clarified that this is a coincidence to avoid the confusion.

ii) Initially, Case 3 was not shown here because it gives a similar trend as cases 1 and 2. In addition, we wanted to highlight the comparison between cases 1 and 2 and the model proposed by Feistl et al. (2014) (Eq. 8 in the manuscript). However, recognizing that this may raise concerns from the reader, we have added case 3 in the revision.

The case 3 due to a high internal friction and cohesion, stopped the entire avalanche for large tree diameter, thus a saturation effect is observed, especially for the points with a diameter of 1.2 and 1.4m. This point has also been clarified in the caption of Figure 6 of the revised manuscript, as shown in Figure R2.

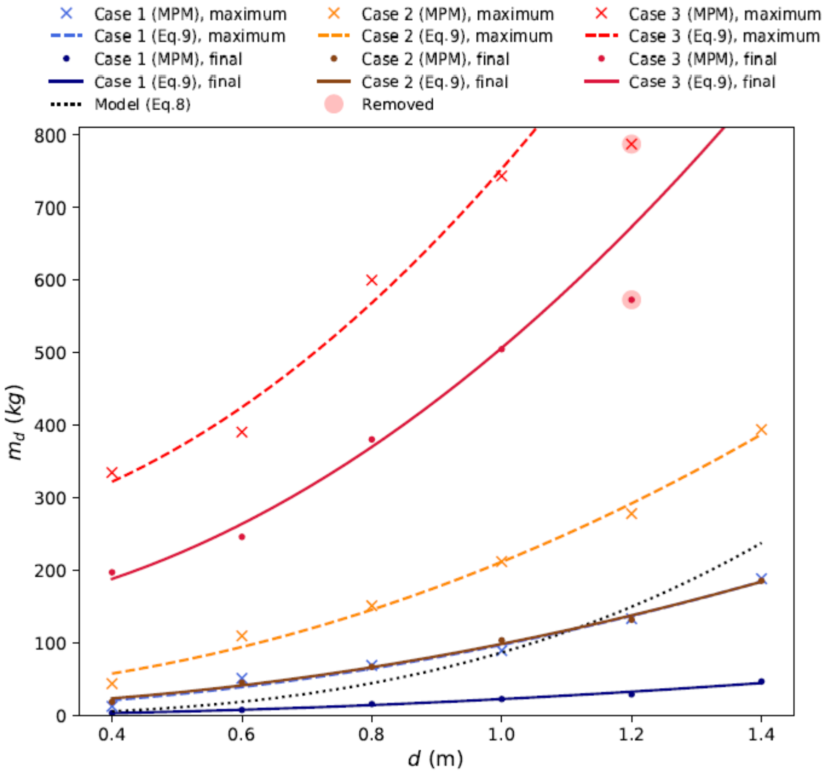

Figure R2. Evolution of the detrainment mass observed for the one tree arrangement and the fits with the tree diameter for 3 flow regimes: Case 1, Case 2, and Case 3 with respectively a front velocity of 12.5 m/s,10.9 m/s and 10.75 m/s, 'maximum' refer to the maximum mass stored behind the tree and 'final' refer to the final mass stored. A slope of 30°, and a top wedge angle of 60° (from measurements Feistl et al. (2014)) are used for the theoretical model (Eq. 8). The removed point denotes a special case not considered in the proposed square relation in Eq. 9, since the entire avalanche in this case is stopped due to the low flow velocity and the high tree diameter. * please note that it is a coincidence that Case 1 maximum and Case 2 final agree well.

Figure 8: Again, here why case 1 is not introduced?

Reply: Similarly, as the previous comment, we initially did not add case 1 as it gives a similar trend as cases 2 and 3. To avoid the confusion, we have added case 1 with an initial speed $v_0 = 10$ m/s in the revised manuscript.

Figure 9: Please explain physically why the detrainment mass is proportional to the third power of the forest density.

Reply: Assuming no interaction between trees, increasing the number of trees would lead to a linear relationship with the mass stopped. However, a second phenomenon is added, there is a collective effect of the trees on the stopped mass. Indeed, the increase in forest density leads to the reduction of the spacing between the trees, which leads to a higher snow jamming and the formation of snow arches between the trees. In addition, this results in a higher gradient of speed between trees which leads to a higher dissipation by friction. The above explanation is in terms of the physical process but does not directly give us the third power law, which corresponds to the best fitting model based on the obtained simulation data. This has been clarified in the manuscript.

Table 3: According to Figure 8, the relation between the detrainment mass and tree diameter for Case 2 and Case 3 are obviously different even though the front velocities are almost the same (11.2 m/s and 10 m/s). Thus, I do not understand the reason why you can use the same p3 and p4 for all three types of snow as shown in Table 3. Further, although no results are not introduced in 4.2.2, I am wondering the snow type does not give any effect on the forest density consequence.

Reply: Indeed, with different snow types, the detrainment mass will be different. However, as introduced in section 4.1.2, it is the parameters p_1 and p_2 that account for the different snow properties, instead of p_3 and p_4. In Table 3, the parameters are for cases with different snow properties, therefore, p_1 and p_2 have different values while p_3 and p_4 are identical for the three cases.

To further explain the same p_3 and p_4 for the three cases in Fig. 8, we normalize the detrainment mass obtained from Fig. 8 (or Eq. 9 in the manuscript) by the detrainment mass with different snow properties and front velocity (Eq. 7 in the manuscript), we obtain a unique curve as shown in Fig. R3, and we identify the same p3 and p4 for all the cases.

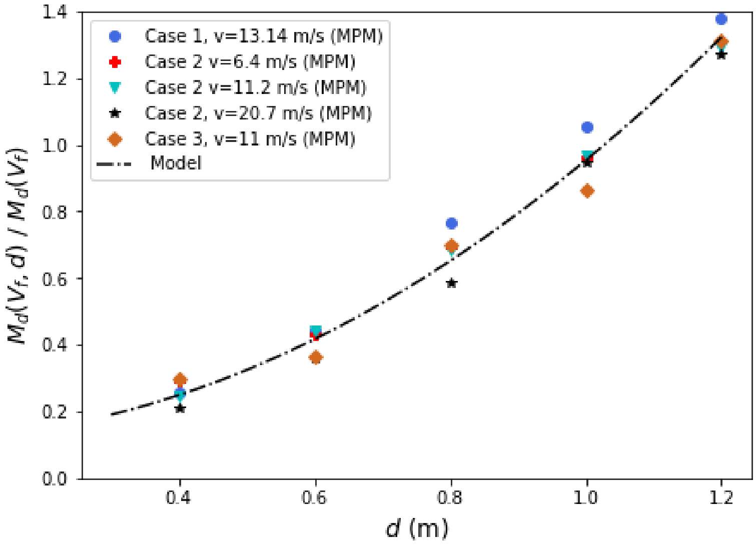

Figure R3. Evolution of the detrainment mass normalized with the front velocity and the snow properties (Eq. 7) for different tree diameters (regular staggered forest, $e =$ 8 m).

Line 273: Please explain why the detrainment energy, when the avalanche reaches the bottom of the valley, is similar between the random and the regular staggered arrangements, in spite of the fact that the final stopped mass with both arrangements are largely different.

Reply: In the case of regular configurations, due to their geometric arrangement, very large deflections are observed. This results in a significant lateral spreading and the delay (or velocity reduction) of part of the avalanche due to numerous collisions (please see supplementary movie 5). Contrary to the random case, this snow is not stopped, but contributes to reducing the velocity/severity of the avalanche. The study of the detrainment energy enables us to both consider the stopped mass and the mass with reduced velocity.

Line 281: Random arrangements is the most effective to stop the avalanche. I am curious how you suggest to people in charge who take care of and manage the forest? Do you say the forest should leave as it is without artificial maintenance? Foresting should be done randomly?

Reply: Indeed, this study shows that in this idealized condition, the random arrangement is the most effective to stop the avalanche. Random foresting can therefore be part of the risk mitigation measures in high-risk area. Another important point of this study is that for planted forests, the staggered arrangement is clearly more effective than the aligned forests. The staggered arrangement can be easily achieved with current devices and without disrupting the forestry sector, to significantly increase the protective effect of forests. However, this study is carried out under idealized conditions and focuses only on the avalanche dynamics. Therefore, further studies are needed, on investigating the effect of the forest arrangement on the release of snow avalanches.

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