Response to Reviewer #1 Frederic Dufour's comment on "The mechanical origin of snow avalanche dynamics and flow regime transitions"

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July 15th, 2020

We want to thank Prof. Dufour for his comments and his constructive suggestions that helped us to improve the quality of our paper. In the following, we provide detailed point-by-point answers to the comments raised by the reviewer.

The paper by X. Li, B. Sovilla, C. Jiang and J. Gaume entitled The mechanical origin of snow avalanche dynamics and flow regime transitions is well organised and written with a short introduction, three sections presenting the different steps of the work and some conclusions and perspectives to finish with.

Reply: We thank the reviewer for this positive evaluation of our paper.

In the introduction, the applicative context is first depicted regarding the necessity of investigating the snow avalanche dynamics for a better understanding and protection of people and human goods. The originality of this study is justified by the need of having a numerical tool to model the dynamics of snow avalanches with snow of different types and different slope geometries.

In section 2, the MPM is briefly described, as well as the constitutive model mainly referring to former contributions by some of the authors but not solely.

Section 3 presents a complete parametric study of five snow types flowing along ideal slopes and arresting on a horizontal plane. The inclination and length of the slope are also part of the parametric study. All simulations fall into four typical snow avalanche groups denoted cold dense, warm shear, warm plug and sliding slab. The front velocity, the velocity profile across the flow, the arresting distance and the free surface shape are part of the output parameters analysed. The results are qualitatively in agreement with the physics and discussed as such. The influence of the snow type is systematically explained. Unfortunately, only macroscopic quantities (see above) as output are studied to distinguish flow types. I would suggest, as in Gracia et al. (2019) [F. Gracia, P. Villard, V. Richefeu (2019) Comparison of two numerical approaches (DEM and MPM) applied to unsteady flow, Computational Particle Mechanics, 6(4), pp. 591-609] which deals with the same topic applied to granular flows, in order to understand the internal physics of the flow that you extract, show and discuss some quantities such as energies (potential, kinetic, dissipated by friction or fracture) to understand their transfers during the flow and to provide an insight to understand which material parameters, including *the basal friction coefficient, are the key ones. Some master curves or should I say master clouds are proposed with dimensionless parameters. Proposition of analytical solutions fitting the simulated results would be an interesting point for further uses towards a quantitative step.*

Reply: The basis for analysing energy is energy conservation. The constitutive model adopted in this study perfectly satisfies the second law of thermodynamics (Line 91-92 in the manuscript). Following the derivation in Gaume et al. (2018), proving that energy does not increase is equivalent to proving the plastic dissipation rate $\dot{w}^P(\boldsymbol{X},t)$ is non-negative. $\dot{w}^P$ can be computed as

$$\dot{w}^P = -\boldsymbol{\tau} : \frac{1}{2}(\boldsymbol{\mathcal{L}}_v \boldsymbol{b}^E)(\boldsymbol{b}^E)^{-1}$$

where $\boldsymbol{\tau}$ is the Kirchhoff stress tensor, $\boldsymbol{\mathcal{L}}_v$ is the Lie derivative, and $\boldsymbol{b}^E$ is the elastic right Cauchy-Green strain tensor. Since we use an associative flow rule, $\boldsymbol{\mathcal{L}}_v \boldsymbol{b}^E = -2\dot{\gamma}\frac{\partial y}{\partial \boldsymbol{\tau}}\boldsymbol{b}^E$ (see Equation 10 in Gaume et al. (2018)), $\dot{w}^P$ can be expressed as

$$\dot{w}^P = -\boldsymbol{\tau} : \dot{\gamma}\frac{\partial y}{\partial \boldsymbol{\tau}} = \dot{\gamma}\hat{\boldsymbol{\tau}} \cdot \frac{\partial y}{\partial \hat{\boldsymbol{\tau}}}$$

Recall that $\dot{\gamma} \geq 0$ in Equation 11 in Gaume et al. (2018). Furthermore, $\hat{\boldsymbol{\tau}} \cdot \frac{\partial y}{\partial \hat{\boldsymbol{\tau}}} \geq 0$ because our yield surface is a convex function of $\hat{\boldsymbol{\tau}}$ which includes the origin. Therefore $\dot{w}^P \geq 0$. Note that this result holds for any isotropic plasticity model that has a convex yield function and associative flow rule.

The evolution of kinetic and potential energy of the flows in the four typical flow regimes (i.e. cold dense, warm shear, warm plug, sliding slab) is shown in Fig. 1. As expected, the potential energy of the flows initially decreases as the flows move down from the slope, and then becomes steady after the flows stop. The kinetic energy of the flows firstly increases and then reduces until it vanishes. It is noticed that the kinetic energy of the sliding slab shows fluctuations in the deceleration phase, due to the interactions between the separating slabs in the flow after they reach the connecting arc zone (see supplementary video 1).

Fig. 2 shows the dissipated energy of the flows in the four cases. The dissipated energy increases before it reaches the final steady state. The growth rate of the dissipated energy varies for the different flows as they have distinct flow behaviours. Nevertheless, the final energy dissipation does not show much difference for the different flows. This is because of the identical initial potential energy and the similar final potential energy of the flows.

The energy dissipation is contributed from 1) internal force of the material and 2) external force on the material from the boundary/slope. As illustrated in Fig. 3, in all the four cases, the dissipated energy from the boundary is much higher than that dissipated inside the material. This is consistent with the results in Gracia et al. (2019).

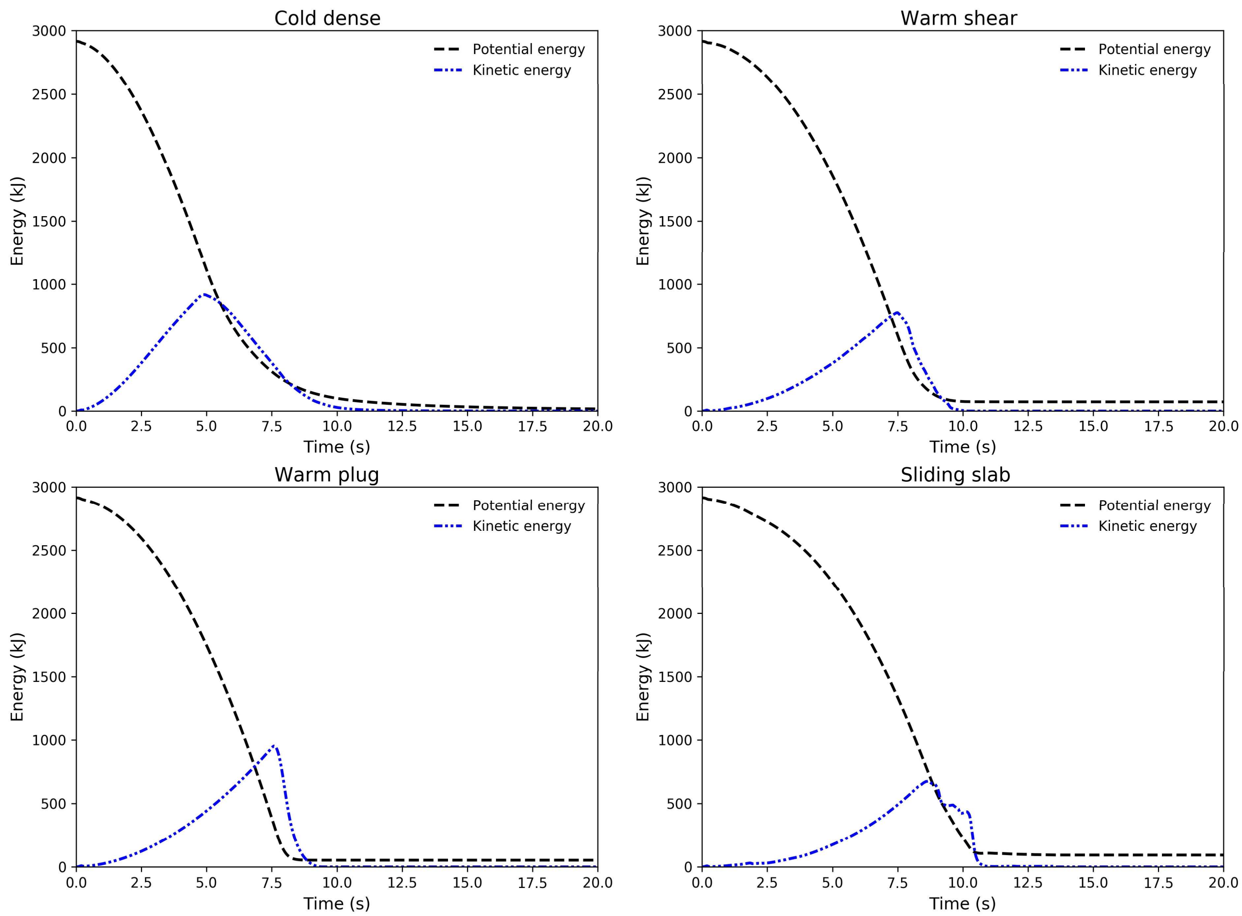

Figure 1. Evolution of potential and kinetic energy of the flows in the four typical flow regimes.

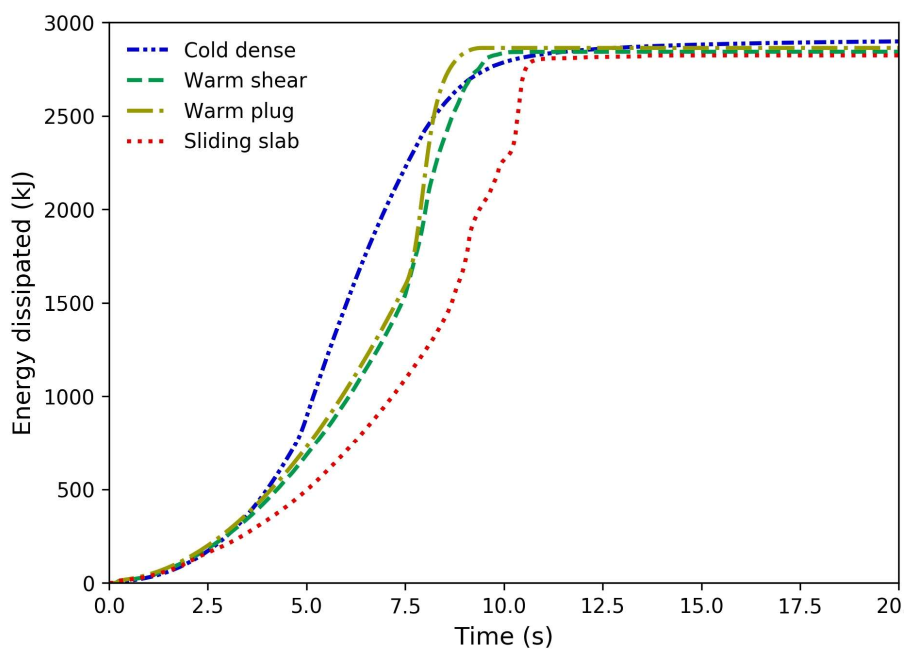

Figure 2. Evolution of dissipated energy of the flows in the four typical flow regimes.

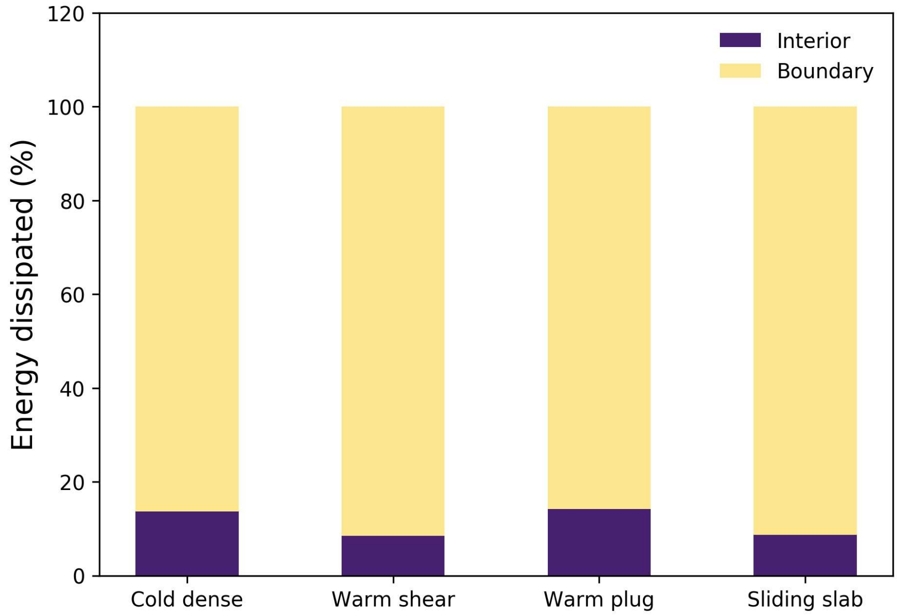

Figure 3. Energy dissipation inside the flow and through the boundary bed in the flows with the different flow regimes.

From the above discussion, we can indeed get more information about the energies. However, we did not find contrasting distinction characterizing the different flow regimes of the flows. Therefore, we put the above discussion as supplement.

References:

- Gaume, J., Gast, T., Teran, J., van Herwijnen, A., and Jiang, C. (2018). Dynamic anticrack propagation in snow. *Nature Communications*, 9(1):1-10.
- Gracia, F., Villard, P., and Richefeu, V. (2019). Comparison of two numerical approaches (DEM and MPM) applied to unsteady flow. *Computational Particle Mechanics*, 6(4):591-609.

Regarding the analytical solutions fitting the simulated results, we indeed thought about proposing analytical relations between the scaled maximum velocity and the scaled deposit height in Fig. 7 in our manuscript as well as between the scaled maximum velocity and the scaled runout angle in Fig. 10. However, the physical processes involved are strongly non-linear and too complicated to develop analytical solutions. For example, the deposit height and the runout distance are greatly affected by multiple processes during the flow and deposition, including breakage and granulation of snow, surging, and piling up. While we propose highly simplified analytical solutions based on the block sliding theory, as a limit case, the development of a complete analytical model taking into account all previously mentioned processes is beyond the scope of this study.

In section 4, the model strategy is applied to real cases with field measurements. It should be more clearly stated in each case what are the parameters that are set a priori and the one used for the calibration process. I suggest setting some stars in table 3 to distinguish calibrated parameters. The results are impressive with a very good *agreement in general with field measures. The discrepancies are explained by the fact that MPM cannot entrain further material during the flow, that the turbulence dynamics in powder cloud is not modelled in MPM (some perspectives are set along this line although the frictional dissipation with air is not mentioned), that the measurement acquisition frequencies are not comparable between field and numerical data (in order to be more precise on this point, data could be presented with points instead of lines, for instance in Fig 14 where the velocity peak is much discussed.)*

Reply: Following the reviewer's suggestion, it has been clarified in the revised manuscript that the bed friction of the slope is the only calibrated parameter. In addition, a star has been used in Table 3 to notify the calibrated parameter. The adopted snow properties are fixed according to the description of the snow type in the literature.

We thank the reviewer for pointing out the importance of frictional dissipation with air in the discussion of powder cloud. The corresponding sentence has been modified to reflect this aspect.

Scattered points connected with a line have been used to plot the real measurement data in Figs 11-14 in the revised manuscript, which indeed offer more information on the measurement acquisition frequencies. Please note we still use lines for the MPM simulation data, since adding points does not differ much from the pure lines because the points overlap with one another as shown in Fig. 4 (Fig. 14 in the manuscript) below.

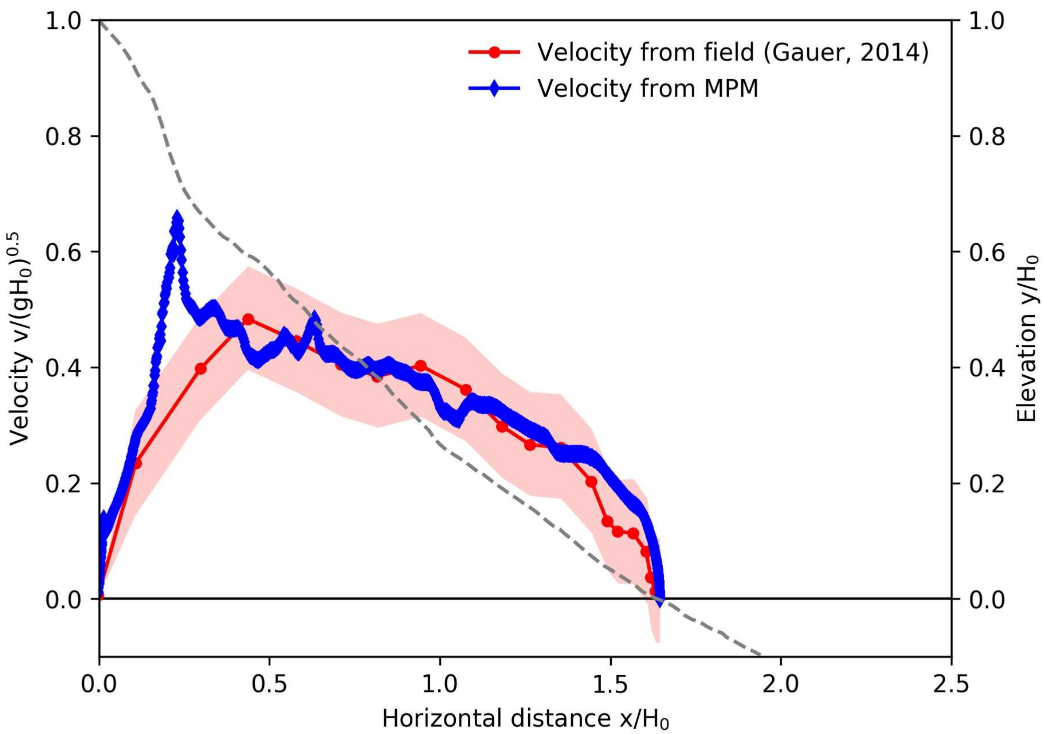

Figure 4. Front velocity distribution along the flow path for Case IV: Ryggfonn 2006-05-02 (Stryn, Norway). Drop height H_0 = 303 m.

The conclusion summarises the main qualitative results. A very interesting discussion is proposed at the end for the future work towards real geometry in 3D (MPM tools already exist in 3D, thus it is mainly a matter of computational time), to introduce in the MPM tool a constitutive law dedicated to powder cloud and its interaction with the dense part (the air friction is not mentioned here).

Reply: The air friction has been added.

Overall the contribution is very well written, clear and well organised. The results and analysis are well documented, except the few points mentioned in bold in this review which need to be addressed for the final version. The work is original and provides an interesting step towards the prediction of snow avalanche propagation conditions.

Reply: We thank the reviewer for his constructive comments that helped us to improve the quality of our paper.