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Interactive comment on "The mechanical origin of snow avalanche dynamics and flow regime transitions" *by* Xingyue Li et al.

Anonymous Referee #2

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Review of 'The mechanical origin of snow avalanche dynamics and flow regime transitions' by Xingyue Li, Betty Sovilla, Chenfanfu Jiang, and Johan Gaume 2018 - tc-2020-83

General comments:

The paper presents a novel application of the authors recently developed approaches, successfully combining experimental findings on the flow regime evolution in snow avalanches and respective modelling approaches. The authors reach the goal of showing the models ability to replicate different flow regimes (and the associated flow

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Discussion paper

characteristics, such as velocity, ...) by tuning the corresponding material parameters.

One point that could be enhanced in my eyes is the discussion of the role and connection between the numerical method/solver and the applied flow/material model. As the title states, the paper aims at the identification of the *mechanical* rather then the numerical origin of flow regimes in snow avalanches. However, the numerical method/solver (MPM) is often highlighted and associated with the success of the modeling results rather than the corresponding material model (see comments below).

Overall the paper is very well written and includes helpful figures with corresponding supplementary material (with some small exceptions mentioned below). This valuable contribution is of high quality, enjoyable to read and fits to the scope of TC.

Specific comments:

- *p2 l 41-51 and section 2.1*: could you include a comment what the main differences (e.g. 2d/3d, depth resolved/averaged, ...) are to the classical, numerical approaches that are used in common simulation software that you also cite throughout your paper (such as Christen et al. (2010)). In particular the similarities and/or differences are to other particle based methods such as SPH (which are also used for classical shallow water 2d avalanche modelling Sampl and Zwinger (2004)) would probably be interesting for the reader to also interpret the future potential of the MPM methods (see conclusions).
- *p5 line 106, Table 1*: here you particularly highlight the parameters for the MPM modeling. To me it appears that this could be misleading. All parameters refer to the material model (section 2.2.). No numerical parameters are discussed - therefore the it would be interesting to: 1) comment the role of the numerical

parameters and how they where chosen and to 2) clarify the role/interplay of the numerical technique and the material model (see comment on paper title above).

- *p7 line 145*: Could you briefly explain a bit more what this threshold means - and if or if not this is connected to the (numerical?) fluctuations that appear e.g. in Figure 3 b) around 5s for the cold dense and 7.5-10s for the warm shear simulations?
- *p15 line 276*: Could you briefly comment on what the plateau stage means and if or if not any of the avalanches reach some kind of final velocity / steady state?
- *p16 l 291, ...To calibrate and benchmark our MPM modeling...*: is this really a calibration or rather a parameter variation/test with respect to the material / flow model rather than the numerical MPM approach?
- *p16 l 307-310*: I think here you have to clarify in more detail: 1) how are the avalanche velocities measures (different measurement techniques will lead to different velocities (front / core), see e.g. Rammer et al. (2007); Gauer et al. (2007)) and 2) if the measurements are comparable are the simulated velocities transformed correspondingly such they can be directly compared to the measurements (see e.g. Fischer et al. (2014))?

Technical corrections:

Generally text and Figures are clear and the supplementary material is very helpful. Possible corrections include:

- *Figure 2 and supplementary material*: Fig 2 is missing a spatial scale and the corresponding video is missing a legend (velocity/epsilon scale) as well as a spatial and temporal scale

- *Figures 11-15 and supplementary material*: absolute scales are missing and prohibit valuable data interpretation (at least total fall height should be stated in a Table or the caption)
- *wording*: α should be referred to as runout angle
- *wording*: H/L and H_0/L_0 should be referred to the other way around (H/L=tan α is usually the convention why H/L refers to the topography inclination in this paper)
- *wording*: what the authors refer to as "benchmark" appears more as a model "test" to me
- *wording*: please check by a native speaker if the choice of plural/singular is appropriate throughout the paper (e.g. behaviours, literatures, terrains, ...)

References

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