Review of 'Modeling the influence of snowcover temperature and water content on wet snow avalanche runout' by C. VERA VALERO et al.'

In their revision the authors provided detailed answers to the issues raised by the reviewers and managed to submit a new, enhanced version of their manuscript. The updated structure allows to follow the ideas of the authors in a better way, but some issues remain a bit unclear (especially section 4.1). Some clarifications that would especially be helpful for the potential readers, are addressed in the answer to the reviewers, but are missing the in the updated manuscript and could be considered before publication. In the following i summarize some main points; page and line numbers refer to the updated version of the manuscript with highlighted changes in red and blue:

- depth and height are still used inconsistently throughout the manuscript
- *3, 70: typo: citepGruber2009*: I am not sure this is completely true, see comment below (parameter α)
- *9, 225*: Here, it would be worth to mention whether the the computational resolution always 3x3 m (besides the resolution variations in section 4.3)?
- *10, 245 ...are sufficiently small...*: Why are they sufficiently small?
- *10, table 1*: I completely agree that this manuscript should not focus on any detailed evaluation of SNOWPACK, however given the observed fractures depths (table 1), it would be very interesting how it compares to the derived release depth - since it is a crucial part of the analysis.
- *15, 340: ..This two-dimensional procedures avoids the problem of defining a one-dimensional measure of avalanche runout.* I completely agree with the authors: However since you already spend the effort of calculating it, it could (but it does not need to) also be included in the analysis? Furthermore it would still be interesting how these evaluation measures compare to other measures used for avalanche simulations evaluation (such as mentioned in the first review, e.g. (Mergili et al., 2017a, for 2d) or (Teich et al., 2013, and refs therein) for definitions of runout).
- *deposition*: For my point of view it is completely appropriate (as done and stated by the authors) to evaluate simulation results of flow depths (above a threshold) at the last time with deposition patterns, without taking into account different, observed depositions depths. However for the purpose of clarity, it should be stated that (i) *deposition* is an interpretation of this distinct simulation result, since the model does not directly cover deposition mechanisms (see e.g. Mangeney-Castelnau et al., 2003; Mergili et al., 2017b) and (ii) how this simulation result is defined numerically (i.e. *15, 352: ...all simulations stopped when 95 percent of the total mass stopped moving* - does the 95% correspond the maximum momentum? what does stop mean (velocity below certain threshold?) in this context?)
- *section 4.1:* : I have no doubt that the thermomechanical model allows to gain different and probably even more suitable simulation results that the classical approach - but i am not convinced that the approach used here is valid to draw this conclusion *18 430: ...the thermomechanical model statistically outperforms the guideline procedure...*: While ad hoc parameter assumptions are *allowed* for the thermomechanical model (*The model has one parameter α ... chosen by the avalanche expert...*) - guideline values (that still seem a bit unclear to me (for *wet snow avalanches* or *13, 323 return period of 10 or 30 years...* or *19, 459: return periods greater than 300 years*)) are used for the guideline-VS model. Also considering that release depth is the most sensitive parameter, it could be discussed in more detail why it is appropriate to perform simulations with the *thermomechanically ad hoc modelled* total mass (release and entrainment) with the vs-guideline model, see *lines 298–323: ...include the entire avalanche mass within the release volume...* - which would yield that the *...total mass in both simulations is similar...* but as it could also be seen: a different setup in initial

potential energy of the flow. That said, it appears to me that the comparison (more specifically this section) is not performed in a *fair* way.

- *section 4.3:* It could be interesting to shortly comment on how the DEM resolution (or computational/numerical resolution) may have an influence on the release volume (in terms of release area etc.) or not since one main outcome of the paper is that very small variations of release volume have a large impact on the simulation results (*28, 624: an underestimation of fracture depth of only 10 cm could lead to significant runout shortening*).

References

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