

## ***Interactive comment on “Modeling the influence of snowcover temperature and water content on wet snow avalanche runout” by Cesar Vera Valero et al.***

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Received and published: 6 September 2017

I commend the authors for the impressive amount of work summarized in this paper: the compilation of data, systematic SNOWPACK and RAMMS simulations, and extensive sensitivity study provide an unprecedented set of results concerning the modelling of wet snow avalanches and the influence of various parameters such as initial mass and snow temperature / LWC on avalanche deposits and runouts. Despite the complex chain of models that is used, the authors made the effort to try to isolate the most influential physical processes, which I find particularly interesting. I am henceforth fully favorable to the publication of this paper in NHESS. I think however that several aspects

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of the paper could be improved to provide a better account of this nice study. First, the paper is a bit lengthy and redundant at places, and the structure of certain sections could be improved. Most importantly, I feel that the choice made by the authors to base most of the discussions on the statistical scores coming from the contingency table analysis, sometimes tend to “soften” the results and “dilute” the differences among the models. Putting more emphasis on more “physical” outputs, such as the raw results shown in supplementary material and the runout distances, would help counterbalance this trend. Finally, I consider that the discussion of the sensitivity analysis needs to be complemented with more quantitative comparisons and discussions. The specific comments below provide more detailed suggestions on these issues.

### Specific comments

1/The introduction would benefit from being more to the point at certain places. The second paragraph, in particular, appears a bit off-topic and overly speculative. If the goal is to explain that wet snow avalanches are characterized by relatively large values of apparent viscosity and cohesion, there is probably no need to discuss the so-called “compactive strength” of snow and its hypothetical relation with viscosity. On the other hand, in the third paragraph, a more in-depth discussion of the advantages and drawbacks of the different approaches used in past studies to model wet snow avalanches would be in order.

2/Section 2, presentation of the model: A clearer structure (e.g., avoiding redundancies and introducing subsections / subtitles to better distinguish between the different elements of the model) would improve the readability of the section. Moreover, certain mathematical notations could probably be simplified, and some physical relations better explained. Some suggestions below:

-Why using the subscript  $\Phi$  everywhere? Is it really useful?

-The variable  $N_K$  present in Eq. (1) would need to be defined earlier after this equation.

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- What is the parameter  $\gamma$  in Eq. (3)?
  - What are the quantities  $h_{\Phi}$  and  $\rho_{\Phi}$  in line 117?
  - Indicate the physical meaning of  $S_{\Phi}$  (shear stress).
  - The sentence starting with “The basal boundary converts. . .” on line 129 is not very clear. This point would maybe be better explained in conjunction with Eq. (7)?
  - What is the relation between the quantities  $\dot{P}_{\Phi}$  and  $R_{\Phi}$ ? Why not denoting the former simply as  $\dot{R}_{\Phi}$ ?
  - Idem: what is the relation between  $\dot{P}_{\Phi^V}$  and  $R_{\Phi^V}$ ?
  - What is the coefficient  $c$  in Eq. (11)?
  - The sentence starting with “Equation (14) takes into account. . .” in line 174 is not very clear.
  - The specific form chosen for the cohesion, i.e. the factor  $(1-\mu)$  and the exponential term, should be commented.
- 3/Section 3.1. It is not fully clear whether SNOWPACK simulations were performed only for the release zones, or also for the deposition zones (in cases where data are available for these zones).
- 4/Section 3.1, Table 3. How is the erodibility coefficient obtained? This parameter is not discussed in the text, although its influence on the results is probably far from negligible.
- 5/Section 3.2. The value chosen for the parameter  $\zeta$  involved in Eq. (7) should also be discussed .
- 6/Section 3.2. Besides data on avalanche release area, the authors probably also have data on fracture depths for at least some of the avalanches. How do these data compare to the fracture depths predicted by SNOWPACK? Where they used in any

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way to optimize the results of SNOWPACK?

7/Section 3.2. Regarding the Voellmy-Salm model, and if I understood well, the authors chose to use the same friction parameters for all studied avalanches. Would not it make more sense to optimize these parameters for each avalanche? I do not see any reason why all these avalanches should be characterized by identical friction parameters. In addition, giving the value of these parameters would also be useful, for the sake of comparison with the parameters used in the RAMMS model.

8/Section 3.5 is not very clear and some redundancies could be avoided. In the first paragraph, in particular, it is difficult to understand what the 432 simulations represent, whereas this issue is better explained afterwards.

9/Section 4.1. While the different statistical scores used by the authors effectively show that the thermomechanical model performs better than the Voellmy-Salm model, this issue is even more evident from observation of the model outputs provided as supplementary material. Hence I would encourage the authors to add, at least, a short description of these raw outputs in the main text prior to discussing the statistical scores. Adding a figure showing one or two illustrative examples of raw results in the main text could also be option. Similarly, moving the runout comparisons (currently presented in 4.4) before the statistical score comparisons could also help to better illustrate the differences among the models.

10/Section 4.1. The sentence starting by “The fact that the difference in ETS score. . .” in line 379 seems in contradiction with what is said just before (lower difference in ETS than in HKS between the two models).

11/Section 4.2, line 407. Why do the authors refer to the friction parameters used in the VS model as “extreme” here?

12/Section 4.2-4.3-4.4. I encourage the authors to provide more quantitative evidences of the conclusions drawn from their sensitivity study. In the current manuscript, it is

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sometimes difficult to relate the assertions made in the text to the presented data. One probable reason is that the authors rely throughout on the same type of figures, whereas alternative representations, such as boxplots or distributions / percentiles, would probably allow for easier quantitative comparisons between, e.g., the different initial conditions (mass versus temperature/LWC) or the different grid resolutions. I indicate below a few examples of overly qualitative statements that would need to be supported by more quantitative evidences:

-line 426: “generally higher”

-line 431: “the simulation with the original initial condition is among. . .”

-line 440: “are more sensitive to”

-line 452-423: “A small variation (. . .) would lead to a large variability” (While Fig. 6 shows that simulations with other initial conditions are sometimes as good as simulations with the correct initial conditions.)

-line 459: “less sensitive”

-line 465: “The variation was strongest”

13/Section 4.2.2: Could the authors also discuss the relative influence on the results of mass in the release area versus mass in the entrainment zone?

14/Section 4.3: The description of the effect of grid size on the statistical scores could probably be shortened, and redundancies avoided. I suggest however to extend the – currently very short – last paragraph describing the interplay between initial conditions and resolution. To me, this latter issue constitutes the real novelty of the sensitivity study conducted by the authors with respect to grid resolution.

15/Section 5: The sentence starting with “Moreover, the connection between friction and initial starting mass” in line 597 is not very clear.

Minor issues

-Table 1. The caption mentions virtual slope, but this information does not seem to appear in the table?

-Line 249-252: The sentence starting with “In case of avalanches with new snow. . .” is not fully clear: does it apply only to the cases where meteorological data in the deposition zone are not available, or to all cases?

-Line 308: The reference to Table 2 seems wrong here.

-Fig. 3, caption: word missing after “the longest calculated”.

-line 477: typo: “courser”

-Fig. 10: Why the asterisk with the specific value corresponding to the CV-1 case?

-line 524: why the “(not shown)”, instead of a reference to section 4.3 where variations of ETS and HKS with resolution are extensively discussed?

-Fig. 11: (a), (b), (c) need to be added to the plots.

-line 609: word missing after “the maximum LWC”?

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2017-36>, 2017.

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