Response to review by G. Chambon

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 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.

ANSWER: We thank G. Chambon for his positive judgment of our work as well as the constructive comments. Please find a detailed response to the issues raised by him below.

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.

- **ANSWER: We agree with you. We removed the speculative part of paragraph. This makes the text clearer and less redundant.**

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.

ANSWER: We broke up the section into different subsections and removed the all the redundancies we could find.

Some suggestions below:

- Why using the subscript Φ everywhere? Is it really useful?
  ANSWER: In order to make this work consistent with previous works it is important to keep the Φ. All variables subscripted with Φ refer to the avalanche dense/flowing part.
- The variable N_K present in Eq.(1) would need to be defined earlier after this.
  ANSWER: Yes, you are right. We define N_K earlier.
- What is the parameter γ in Eq. (3)?
  ANSWER: Yes, you are right. We removed the γ from Eq. 3. Thank you.
- What are the quantities $h_\Phi s$ and $\rho_\Phi s$ in line 117?
  ANSWER: These variables represent the co-volume height and density. The co-volume represents the densest possible packing of snow granules in the avalanche core. We don't want to talk about this too much because it goes into too much detail, so we simply placed a citation in the text.

- Indicate the physical meaning of S_Φ (shear stress).
  ANSWER: We now write "The shearing stress ..." We have created an entire section entitled "Avalanche flow friction" where the shear stress is described in detail.

- 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)?
  ANSWER: yes, you are correct, we placed this text after Eq. 7.

- What is the relation between the quantities $\dot{P}_\Phi$ and R_Φ? Why not denoting the former simply as $\dot{R}_\Phi$.
  ANSWER: The variable Pdenotes the input of energy (source term) whereas the variable R denotes the value of energy after ALL processes (advection, sinks) have been considered. We too would like them to be the same, but this is mathematically impossible.

- Idem: what is the relation between $\dot{P}_\Phi V$ and $R_\Phi V$?
  ANSWER: Please see above.

- What is the coefficient c in Eq. (11)?
  ANSWER: Corrected. It is the specific heat. The subscript Φ is missing in the equation.

- The sentence starting with "Equation (14) takes into account..." in line 174 is not very clear.
  ANSWER: We now write, "Equation ... takes into account the thermal energy contained in the entrained snow." This is better, because we avoid the use of the word "production" which confuses everything.

- The specific form chosen for the cohesion, i.e. the factor $(1 - \mu)$ and the exponential term, should be commented.
  ANSWER: This specific form of the cohesion function is based on results from snow chute experiments. These experiments show that the shear stress increases from zero $S_\phi = 0$ when the normal stress is zero N=0. Basically the form of this function comes from fitting measurements. In the text we write, "The form of Eq. 60 ensures that the shear stress S_μ=0 when N=0, in accordance with shear and normal force measurements in snow chute experiments."

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).

ANSWER: SNOWPACK simulations were also performed when a station in the valley was available (9 out of 12 cases). This is shown in Table 1. For the valley simulations, the virtual slopes were not considered, and only the flat field simulations were used. This corresponds to the fact that deposits area for large avalanches are relatively flat, compared to the release area. We will revise the text in Section 3.1 to make this clearer.

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.

ANSWER: Yes, the reviewer is correct. We selected the erodibility coefficient based on extensive back-calculation of wet snow avalanche events. The selection process is reported in a previous paper. We don't want to clutter up the paper here, but we introduced the sentence in section on entrainment: "The value of the erodibility coefficient depends on snow quality. Values for warm, wet snow are reported in Vera et al. (2015, 2016)."

5/Section 3.2. The value chosen for the parameter ζ involved in Eq. (7) should also be discussed.

ANSWER: **we made a notation mistake here. The parameter ζ does not exist, it should be γ. We also write, "The fluidization parameters α and γ (please see Bartelt et al. (2006) and Vera et al. (2016)), are fixed to a pre-determined values based on the terrain characteristics for each avalanche path. Once these parameters are fixed they are not tuned for the remaining set of simulations."**

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 way to optimize the results of SNOWPACK?

ANSWER: The reported fracture depth data was used to constrain the SNOWPACK simulaitons. This data was therefore very helpful in determing the quality of the SNOWPACK simulations.

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.

ANSWER: YES! This is extremely important for us that WE DO NOT optimize the friction parameters for a particular avalanche. We have a set of "wet snow parameters" that we use for all wet snow avalanches. The initial (release) and boundary conditions (terrain, snowcover) are changed for each avalanche. We emphasize this result in the conclusions.

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.

ANSWER: We rephrased this section completely, also to address the comment by the other reviewer that the motivation for the way the sensitivity study was set-up was not clearly explained.

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.

ANSWER: We agree. We will move the runout comparisons, currently presented in 4.4, to an earlier section.

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).

ANSWER: You are correct, it's in contradiction. The sentence was wrong and referred to an earlier version of the graph. We removed it from the manuscript. The difference in POD between the thermodynamics model and Voellmy-Salm model is larger than the FAR. So in the results presented, the Hanssen-Kuiper skill score is not biased towards the Voellmy-Salm model anymore, as the POD is not higher.

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

ANSWER: extreme refers to avalanche with return periods greater than 300 years. We now state in the text, "The primary result of the preceding section is that guideline-based avalanche dynamics models with extreme friction parameters (avalanches with return periods greater than 300 years) will have difficulty reconstructing individual case studies and that they are not easily linked to snowcover conditions."

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 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"*

ANSWER: We will pay particular attention to providing more quantitative descriptions when revising the manuscript.

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?

ANSWER: The statistical scores show superior scores when the correct entrainment conditions are modelled. However, The results are controlled by the water content/warmth of the entrained snow. The problem is, and we have stated this in the work, that the water content/warmth of the entrained snow did not vary strongly, because we are considering only wet avalanches. The role of entrainment would chnge dramatically, if we were to include dry and wet snow avalanches. We have added the text, "The role of mass entrainment is difficult to identify in the statistical scores because we considered only warm/moist snowcovers. Moreover, the permuations did not include dry, cold snowcovers. This result suggests that the snow quality (temperature, moisture) is more important than the snow amount."

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.

ANSWER: The reviewer is correct. The very short paragraph should not stand alone as it begs for further detail and explanation. In reality this paragraph is the conclusion of the preceeding paragraph. We suggest taking these lines and writing "To conclude, ... ".

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

ANSWER: Yes, we agree. It adds no further information. We deleted it from the paper.

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

ANSWER: The third column in the table should have been interpreted as, for example, KLO3-NE means AWS is KLO3, virtual slope is North-East (NE). We will revise the table caption and table layout to make this more clear.

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?

ANSWER: This indeed is not clear. We only use data from the measurment stations, which are located in the release zones. No data is really available for the deposition zones, which are based on snowcover modelling. Therefore, it applies to ALL cases.

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

ANSWER: Thank you for pointing out, it should have been Fig. 2 instead of Table 2.

Fig. 3, caption: word missing after "the longest calculated".

ANSWER: Thank you for pointing out, changed to: "the longest calculated flowline (red dot)"

line 477: typo: "courser"

ANSWER: Thank you for pointing out, changed to "coarser".

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

ANSWER: For this case study we had a 1m digital elevation model, obtained from a drone flight. It is therefore different from the rest and it is important to identify this case as special. We will explain this in the revised paper.

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?

ANSWER: We removed "not shown" and will make a reference to section 4.3.

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

ANSWER: Thank you for pointing out, the figure will be corrected.

line 609: word missing after "the maximum LWC"?

ANSWER: Rephrased the sentence to: The bulk LWC of the slab above the depth of the maximum local LWC below the snow surface was used to initialize the simulations.