Answer to Referee #1

We thank the referee for their insightful comments. Please find our detailed response to the issues raised by the reviewer below. Referee comments are in italics while our answers are in blue.

General comments:

Congratulations to this very interesting paper! It brings the front of snow hydrological research one step further. The combination of the different modelling approaches is a valuable effort to combine methods, each of which as appropriate as possible for the scale, to ultimately integrate all relevant processes that determine the variability of snow depth in high mountain regions. Step by step we are coming closer to a snow hydrological model which allows robust prediction of snowmelt dynamics and, maybe even more important, of climate change effects on the snow distribution and its melting regime when combined with predictions from convection permitting climate models. This paper is an important contribution to this endeavour.

From my point of view, three issues desire some attention prior to finalizing the manuscript. The rest are minor comments.

The English is very good, I only found few details in the text where I suggest an alternative formulation.

Specific comments:

1) I recommend the authors to add a paragraph for the integration of the models and their timing: how were the submodels parameteriized (wind-induced snow redisribution, avalanches)? Does this parameterization depend on the scale (model/DEM resolution)? What triggers an event (blowing snow, avalanche)? What is the order of the computations in a time step, does this play a role? If yes, why is the chosen order the better one? These are all interesting questions for modellers and should be presented at least briefly.

Following the referee's suggestion, a paragraph has been added in Section 2.3 to describe the sequence of processes of the redistribution modules, and technical choices that can be useful for modellers. More details have also been added in each module's description to explain more clearly chosen parameterizations.

2) To my knowledge, SnowSlide updates the DEM surface elevation after each redistribution event with the accumulated mass of snow, thereby filling depressions and/or building up snow depositions in the runout zones of an avalanche. Isn't this the feature in SnowSlide that controls the runout area size of the snow if another (one after the other, actually) avalanche flows down the same slope/couloir (apart from parameters like maximum accumulation per pixel etc.)? This should be discussed in chapter 2.3.2., together with the new „hysteretic feature" (in a bit more detail).

In its original form, SnowSlide updates the DEM with the updated snow depth at each time step, which enables to update the slope and the order of pixel calculations sorted by decreasing elevations. This version has been tested, which showed no significant visible differences in avalanche deposition areas. However, the calculation at each time step (i.e. every hour) of the new sorted elevation list takes a significant amount of time, superior to the modelling itself for large domains/highest resolutions (complexity of order N log(N) to sort a list of length N). Consequently, with a view to intermediate complexity modelling applicable to operations, we have decided to discard this step. The implementation of the hysteretic features showed a more significant impact on the avalanche extents. These modelling choices have been more clearly explained in the revised manuscript.

3) it would be nice to (make an attempt at least to) to evaluate the results of the single process simulations: solid precipitation amount, the new layering scheme (wetting events, density of the modelled snow layers), the modelled wind-induced lateral snow redistribution and the modelled avalanches as well.

Following the reviewer's suggestion, additional FSM2trans simulations were performed with either only wind-driven redistribution, or only gravity-driven redistribution. In addition, following the other referee's suggestion, a more quantitative validation has been introduced to support statements in the comparison of modelled snow depth maps to the LIDAR datasets. The Structural Similarity Index (Wang et al., 2004) is used in the revised version of the manuscript to quantify the maps similarity combining similarities of luminance, contrast, and structure for each pixel with a chosen Gaussian radius (here set to 150 m). Snow depth maps can be compared as grey-scale images, where the snow depth is the luminance on a scale of 0 to 5 m. This metric has been computed for all simulations (a random snow depth distribution having a score of 0, an identical image having a score of 1), including wind transport only and avalanche only, which allows to evaluate the relative impact of each process simulation.

A figure showing the vertical profiles of snow depth (for LIDAR and for FSM2ref) has been added in the Supplementary Material to support the statement of lack of precipitation at highest elevations. References to results of Mott et al. (2023), showing similar precipitation trends, have been added.

Technical corrections:

- 14-15: „… from the peak of winter to the end of the melt season“: but not before peak of winter? Why? This should be mentioned here

We refer here to our evaluation results which cover the period from peak of winter to end of melt season, which integrates redistribution processes throughout the whole winter. We choose to not add more details in the abstract to keep it concise.

- Figure 1: the left panel should be larger (same size as the right one)

Both maps have been edited to improve readability.

- 95: „mostly in open terrain“: what about forests, are these omitted here? There is probably a good reason for this, but it also should be expressed here

Forests and urbanized areas were excluded from the study to focus on redistribution processes in open terrain. It has been clarified in that section.

- 116, 142 and 151: I recommend to insert a table here with all existing FSM versions, including the original(s) by Richard Essery and all the follow-ups, including their names, references and main differences

A table summarizing all model versions has been inserted in the revised manuscript.

- 152-177: it would be nice for the reader if you show the effect of the two processes by means of an example simulation for a small but typical sub-area of one of your domains

We agree with the reviewer that such local examples can allow the reader to understand more intuitively how the models work. However, in order to maintain the conciseness of the paper and to keep the number of figures to an acceptable level (after revision additions), we have decided not to add this supplementary figure, as the effect of both modules (separated by saltation/suspension, snowdrift sublimation and avalanche processes) is already illustrated by Fig. 9.

- 169-177: are you using a SnowSlide version that updates the DEM surface elevation after each simulated transport event (i.e., adds deposited snow to a new surface elavation so that the next avalanche flows over it) to prevent „endless" increase of snow depth in depressions? See specific comment No. 2.

Please see our answer to the second specific comment.

- 160: are these „adaptions and improvements" that are discussed in the following? Maybe this could be made clear here

Indeed, they are the adaptations and improvements described in the following sentences. It has been clarified.

- 169: maybe better „using" instead of „offering"

Corrected.

- 170: is the „snow holding thickness" a snow depth threshold? The it should be mentioned here. A more general term would be „snow holding capacity".

The snow holding capacity is defined as a threshold in snow thickness (i.e. normal to the slope), dependent on the slope. It has been clarified in the revised manuscript.

- 171ff: how did you tune the SnowSlide parameters? See my specific comment No. 1.

We used the parameterization of the snow holding capacity function of slope as implemented in the SnowSlide module of the Canadian Hydrological Model CHM (Marsh et al., 2020). It has been explicitly mentioned in the revised manuscript.

- 174: are the „few improvements“ the ones presented in the following?

Indeed, they are the improvements described in the following sentences. It has been clarified.

- 176: „extent“: this means a larger deposition area, right? If yes, why not name it like this?

The reviewer is right, it has been corrected.

- 199-209: what can you say about the accuracy of the LIDAR-derived dataset? See my specific comment No. 3.

The four LIDAR snow depth datasets were validated against manual snow depth measurements.

For domain D0 (20 March 2017, 31 March 2017 and 17 May 2017), a validation against more than 11 thousand manual measurements showed a bias of -4 to 0 cm and a RMSD (Root-Mean-Square Deviation) of 4 to 8 cm (Mazzotti et al., 2019).

For domain B0 (17/03/2020), a validation against 79 manual measurements showed a bias of - 2 cm and a RMSD of 15 cm.

This information about the accuracy of the LIDAR-derived dataset has been added in the revised manuscript.

- 203: could you indicate explicitely earlier that you limit simulations to non-forested areas (see comment to line 95)?

As suggested earlier, we clarified it in the “Modelling domain” section.

- 204: 31 March 2017 is also covering the melting period?

Indeed, melt had started at the lowest elevations of the domain on 31 March 2017. It has been clarified.

- 209: evtl. better „aggregated to“

Corrected.

- 216: better „by“ Winstral et al. (2017) and Dujardin and Lehning (2022)

Corrected.

- 220: you have both „snow depth“ and „snowdepth“ throughout the text. The former one is correct

Corrected.

- 225: probably better „for“ subdomain B0 (all through the text where this occurs), and „while Fig. 4 shows subdomain“ …

Corrected.

- 232: what do you mean with „spatialized“ snow depth measurements, an interpolation result?

The wording was indeed wrong. We meant “spatially distributed snow depth measurements”, like the LIDAR snow depth dataset, as opposed to point snow depth measurements. It has been reworded in the revised manuscript.

- 233: better „produces too little snow“

Corrected.

- 236: does „deposit extent“ refer to area or mass, or both? I also think that it would better be „deposition“ than „deposit“

“Deposit extent” referred to the area. We have replaced it by “deposition area” for clarity.

- 238: probably „accumulations“ should better be singular, because it refers to the general nature of the process; or do you mean specific events?

It has been corrected to “accumulation”.

- 241: here „accumulations“ probably means „accumulated mass“?

Indeed. It has been corrected.

- 243: what are the „new hysteretic features of the avalanche model“? Maybe the slope threshold application mentioned in Sect. 2.3.2.? This deserves a more detailed explanation (see comment to lines 169-177 and specific comment No. 1)

Section 2.3.2 has been clarified, following the reviewer’s previous suggestion.

- 246: I think it should be „spring“ (lowercase; everywhere)

Corrected.

- 254: what do you mean with „resolutions … are irrelevant“? How can a resolution be irrelevant? Eventually you mean that the simulation results achieved for these resolutions do not properly reproduce redistribution processes …

The wording was indeed unclear. We meant that going down to high resolutions such as 25 m, 50 m or 100 m does not bring significant added value compared to lower resolutions if redistribution processes are not modelled, given that the most significant part of variability at these scales is due to redistribution. We rephrased it in the revised manuscript: “simulations that do not include redistribution processes cannot represent a significant part of the snowpack spatial variability at 25 m resolution”.

- 258: is the reason for this the precipitation interpolation method the increase with altitude (the lapse rate)?

The precipitation input is derived from interpolated 1 km resolution fields of the Numerical Weather Prediction model COSMO. Snowfall estimates are improved by data assimilation of snow depth measurements through optimal interpolation (Magnusson et al., 2014). However, these measurements are rare at high elevations (typically > 2500 m), which represent a significant part of our study domains. Underestimated precipitation at high elevations has already been noted earlier (Mott et al., 2023). It has been clarified in the revised manuscript.

- 265: find something better than „over the whole subdomains“ (what exactly do you mean with it, areas with TPI≤200?)

The sentence has been reformulated: “The match of FSM2trans with the LIDAR is even better than when all TPIs are considered, with a clear improvement compared to FSM2ref.”.

- 272: what do you mean with „global“, maybe „regional“ or „in general“?

It has been replaced by “the snow depth frequency curve”, for clarity.

- Figure 3: better „Map of snow depth on 17 March…“, „for“ subdomain … and aggregated „to“. An image showing the difference between a) and b) would be very informative for the reader because it shows the spatial pattern…

Corrected.

We deliberately decided not to show the snow height difference map. Indeed, a pixel-to-pixel bias can lead to a double penalty effect. For example, a correct snow transport extending one pixel further than the observation can generate a pixel of strong negative bias next to a pixel of strong positive bias, while the overall process is well represented. The resulting bias map is difficult to interpret and can be misleading on the actual model performance. This is the reason why we show distribution frequency plots and aggregations by topographic classes.

- Figure 4: same as for the caption of Figure 3

Corrected.

- Figure 5: same as for the captions of Figures 3 and 4

Corrected.

- Figure 10: better „aggregated for the whole domain“

Corrected.

- chapter 5.2: see specific comment No. 3.

The result and discussion sections of the revised manuscript have been enriched with a more quantitative analysis of map comparisons, with additional insights on the performance of wind transport and avalanches. Please see the answer to the third specific comment.

References

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