Assessing spatially distributed snow simulations with MEB-Crocus in subalpine forests through modelling experiments – *Mazzotti et al., submitted to HESS*

Reply to REVIEW 1

In their manuscript entitled "Assessing spatially distributed snow simulations with MEB-Crocus in subalpine forests through modelling experiments", Mazzotti et al. compared different configurations of MEB-Crocus and FSM2oshd over a forested domain of eastern Switzerland with complex topography running distributed experiments for 7 winter years. This work reveals important discrepancies between two land-surface schemes that the authors attribute to each model's canopy snow process parameterization and to the quality of the canopy structure database used in the modeling setup. Overall, this manuscript is clear, well-structured and written in a concise and straightforward manner, which makes it easy to read. I especially enjoyed going through the Discussion which is truly insightful. The scientific content is solid, and the methodological framework is also robust. I only have a few general comments and a few more specific and technical recommendations that, I believe, would benefit this manuscript, which is already of good quality.

Many thanks Dr. Benjamin Bouchard for taking the time to review our work and for your positive assessment and comments. Please find our answers below each comment in blue.

General comments:

1. The framing of FSM2oshd.

While the title suggests the prevailing role of MEB-Crocus in this work, the manuscript itself rather presents FSM2oshd as a key component of this work, more than just 'any' benchmark model to assess MEB-Crocus. We see it already in the Abstract (ll. 15-19), in the first research question (ll. 81-82) and in some other instances in the Results section where FSM2oshd is sometimes presented in front of FMS2-Crocus (e.g.: ll. 301, 308, 326-327, 334, and others). From my point-of-view, this work is a comparison of three canopy-snow schemes: FSMoshd (which is has proven to be successful for representing canopy-snow processes a high resolution), MEB-Crocus (which is set to be used within Météo-Frances operational framework) and MEB-CrocusXT (which is an improved version of MEB-Crocus for canopy-snow parameterization). I think that framing the Title and the research questions/objectives of the study accordingly, and adjusting slightly the structure of the Result section would suit better the narrative of your manuscript.

Thank you for pointing out this issue. The focus on MEB-Crocus as suggested by the title is indeed intentional, as the primary objective of the paper is to evaluate its suitability for spatially distributed forest snow modelling in view of its upcoming operational use at Météo-France (and thus in future reanalysis products). We have done several edits to ensure this framing is better conveyed by the text, including: 1) removing the sentence on FSM2oshd in the abstract; 2) rephrasing RQ1 to put MEB-Crocus up front; 3) expanding on the presentation of MEB-Crocus in the methods section, including details on the process parametrizations; 4) reversing the order of how the models are presented in the results section to put more emphasis on MEB-Crocus.

2. Where are the equations?

I must admit that I was surprised not to see a single equation in a manuscript centered exclusively around modeling results. Fair enough – MEB-Crocus (Boone et al., 2017; Napoly et al., 2020 and Lafaysse et al., 2025) and FSM2oshd (Mazzoti et al., 2020; Essery et al., 2025) have already been described in previous papers which are cited accordingly. Yet, given that the respective process parameterizations of each model partly explain some of the discrepancy between simulations, I encourage the authors to include the main equations governing canopy-snow processes in the main manuscript (perhaps in an Appendix). I believe that this would help in the interpretation of some of the observed discrepancies.

As for the parameterization of MEB-CrocusXT, although this is likely going to be described in detail in an upcoming manuscript (Pauze et al. in prep) and is for now only presented in a Supplementary Materials, I recommend moving this content to an Appendix of the main manuscript so the reader can refer to it more readily.

That is a fair point, we understand that further detail on the process parametrizations will make interpretation of the results more straightforward. To avoid too much redundancy with existing publications, we can add a table that contrasts the representation of the processes relevant to this study in the two (three) models and include the most important equations in a Supplement (or Appendix) – still, we will leave a comprehensive description of the canopy-snow interactions to the model description papers as these should remain the main references for the models.

3. Presenting showdowns in the Methods.

Each subsection of the Results presents a comparison between FSMoshd and MEB-Crocus (or MEB-CrocusXT) under different configurations (open, forest, both). These step-by-step "showdowns" are essential to the narrative of the manuscript and each of them have a specific purpose. Therefore, I suggest introducing these comparisons in a dedicated subsection of the Methods which, I think, would make the Result section even more straightforward. This implies, for instance, rephrasing partly and moving ll. 281-285, 318-320, 350-353, 417-419 to the Methods.

We have added showdowns of the comparisons and removed the abovementioned lines from the results instead. We believe these can be integrated into section 2.4 ('Modelling experiments') in the form of a Table rather than requiring a dedicated subsection, see suggestion below:

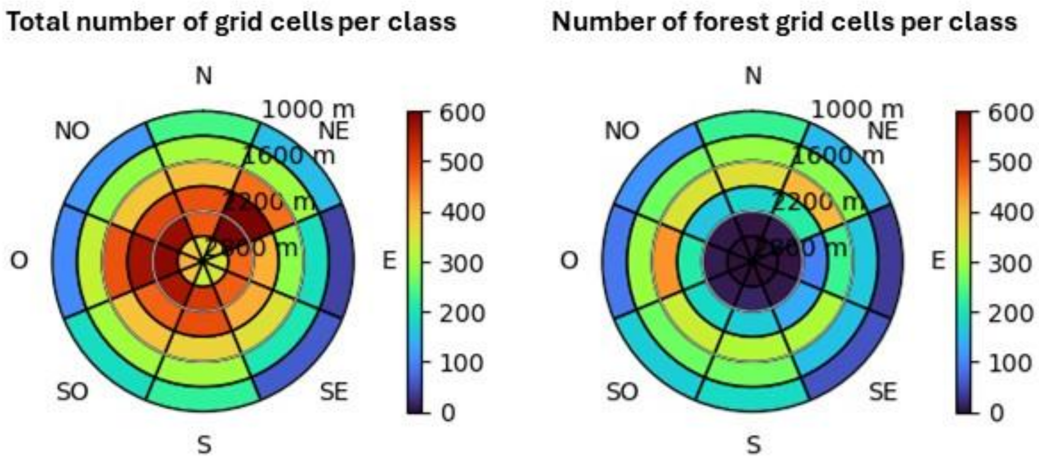
Simulation comparison	Section	Objective
MEBCro_def vs. FSM_all	3.1	Quantify overall model differences between default versions
Cro_opn vs. FSM_opn	3.2	Quantify model differences due to snow cover representation only (no forest)
MEBCro_for vs. FSM_for	3.3	Explore model differences due to forest snow process parametrizations
MEBCroXT_for vs. FSM_for	3.4	Explore impacts of from recent updates of canopy snow process representation

MEBCro_def vs. MEBCro_all vs. MEBCroXT_all vs. FSM_all	4.1	Assess impact of process parametrizations vs. vegetation data

Distribution of the grid cell classes.

If I am not mistaken, results are aggregated into 48 classes (6 elevation bands and 8 aspects). A major part of your results is analyzed based on the simulated SWE in those classes. However, as you show on Figure 1 (right of top row), the distribution of grid-cell elevation is not equal for each elevation range, and I would assume the same for the aspect ranges. I suggest presenting in a polar plot the distribution of all the 48 classes. I think that it would bring extra precision to the interpretation of the results with respect to the influence of topography on modeling results.

This is a good point that we had initially considered, but ultimately dropped given that Figure 1 is already quite crowded. We will add polar plots presenting the distribution of the 48 classes (for all pixels and forest pixels only) to the Supplementary Material, see below for a figure suggestion.



Specific and technical comments:

ll. 17-19. I don't feel like this sentence is necessary in the abstract. I would rather expand on the sentence presented on ll.19-20 which provides information on the methodology.

We have deleted the sentence as suggested and expanded the subsequent sentence as follows: *'Several modelling experiments using MEB-Crocus using vegetation datasets with differing native resolution and varying grid cell tiling strategies were performed to enable assessment of different aspects of the model, such as the importance of vegetation datasets relative to process parametrizations.'*

l.22. Should it be FSM2oshd (instead of FSM2 only)?

Thank you for the catch, this has been corrected.

l.25. I would specify in one short sentence what are the enhancements to the parameterizations.

We have added the following sentence to introduce the parametrization enhancements more precisely: *'including consideration of canopy height and unloading and optimized coefficients for the dependence of unloading and canopy snow melt on wind and temperature'*

ll. 46-67. In the second paragraph of the Introduction, modeling or observation scales as well as resolutions are often mentioned. (i.e. ll. 46, 51, 57, 59, 60, 62. 62). I suggest to give examples of order of magnitudes of the scales (large scale versus landscape scale vs operational resolution, etc.)

We have added specifications on the spatial scales at a few instances to provide concrete examples (slight changes in entire paragraph).

l. 61 and all other corresponding instances. Make sure to refer to the final publication of Haagmans et al (2025).

This has been adjusted, the final publication was not available at the time of submission.

l. 73 and **l. 77.** Maybe it is just me, but I found that refereeing to upcoming sections of the manuscript in the Introduction is slightly peculiar. Can you replace that by existing publications or publicly available reports?

The idea with pointing to these sections was that existing evaluations of MEB-Crocus and FSM2oshd are described more in detail there, while in the introduction we only state they exist. We have replaced this with references to the relevant studies.

l.127, 232, 418. Whether or not you include the detail of the MEB-CrocusXT parameterization in the manuscript, I recommend replacing the reference to Pauze et al paper in preparation by the work that was presented at EGU26 (https://doi.org/10.5194/egusphere-egu26-6561).

The publication is now submitted and with an editor, a preprint is expected to be citable by the time we submit our revised manuscript.

l. 149. Please specify how many 'few (numerical) layers' is.

Three in this study, the information was added.

l. 153. Although, it is mentioned later (l. 201) I would specify the resolution of COSMO here since you mention the resolution of the model runs right before.

The resolution of COSMO has been specified here as well.

Figures. Maybe it is something that will be corrected by the editing team, but make sure that the quality of all figures (resolution-wise) is good enough. Currently, the resolution of most figures is underachieving.

We are sorry about the inconvenience; we will ensure better quality for the final version of the Figures.

Figure1. I would add the lat-long tickmarks on one of the maps.

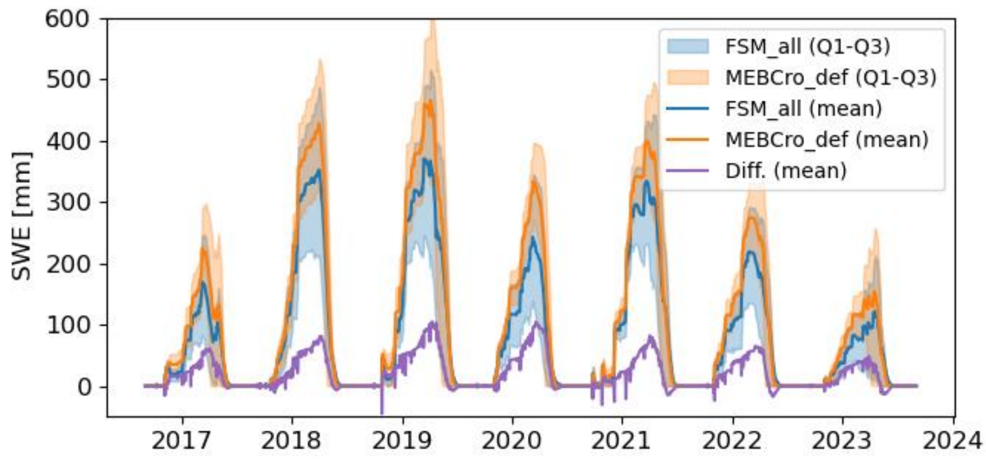
Lat-lon tick marks have been added to the DEM map.

Figure 3. It took me some time to get that the SWE (black curve) was the same on each frame. Please make sure that this is clearer in the revised manuscript. Also, I would recommend specifying the panel ID (e.g. a), b)...) on the figure. This also applies to other figures.

We have clarified that the y axis is not the same in the two subfigures. Note that this figure is only for illustration purposes so whether the two subfigures show the same point is of secondary importance. We have also added panel IDs here and in figure 1, where they were not yet present.

Figure 4. On the timeseries, I would recommend to plot the median instead of the mean, which is more coherent with the envelope that shows the interquartile range. Also, consider showing a time series of the SWE difference between the two experiments. I think that this would allow to grasp more easily the seasonal dynamics (accumulation vs ablation) rather than inferring it from the polar plots above.

It is true that the median would be more consistent with the interquartile ranges, but the mean is more meaningful in terms of quantifying water resources stored in the snowpack, which is why we opted for combining the two in the same plot – we have maintained this and added the mean difference between the two models as suggested (which is more consistent with plotting the model means). Indeed, this better visualizes the seasonal dynamics (see preliminary subfigure below)



l. 299. Just to be sure, is it 13% of the average peak SWE as simulated by MEBCrocus? Maybe it would be worth specifying.

Section 2.5 stated: *'Relative model discrepancies are expressed as percentage of the FSM2oshd benchmark.'* – but we have repeated it here to avoid confusion.

l. 330 (Figure 5). Why don't you show all years? Is there some different behaviors of the models in other years? If you decide to show specific years, I recommend presenting complete time series in Supplementary Materials. This applies to the following subsections of the results.

We show fewer years simply to allow zooming in and seeing the differences between panel e and panel d better. Based on Figure 4e, where all year are shown, we can check that the selection done in Figure 5 covers both high and low snow years. We can of course add the complete time series in the Supplementary Materials.

Figure 6. Was it intended not to present results from simulations configures with a LAI between 3.5 and 4.5?

Yes, the reason being that we aimed to analyze clearly distinguishable sparse and dense canopy classes to single out the impact of canopy density. Including LAI values in the intermediate range would likely blur these differences.

ll. 375-377. I think that this is a good reason to present other WYs (at least in Sup. Materials).

We have included all years in the Supplementary Materials. Again, selecting these two specific years aimed at presenting results for two contrasting (high and low snow) seasons.

Figure 9. I have some trouble reading Figure 9c). This is maybe just me, but I find the solid, the dashed and the dotted line very similar to one another for a given color. Moreover, the legend only presents the solid line. I recommend using specific colors to each line (Perhaps other colors than purple and yellow, which are already used to present WY18-19 and WY19-29 on Figure 7).

The colors in this figure have been adapted for better readability.

Figure 10. I think that 'XT' is missing in the name of the MEBCro simulations in the legend of 10d. Please double-check.

Good catch, this has been corrected.

ll. 425-427. I was surprised by the strong increase in discrepancies in snow duration considering the better match in peak SWE and ablation rate on Figure 10b. The explanation provided here is however satisfying to me. Nonetheless, I feel that this may be due to the definition of the start of the snow season day (SSD) and the snow disappearance day (SDD) which rely on thresholds of 10 mm of SWE (see ll. 251-253). Can you elaborate on the importance of the choice of that threshold in the context of increasingly frequent ephemeral snowpacks at low elevations and how it affects the interpretation of the results.

Thank you for raising this point. The main motivation for using such a threshold is to filter out spurious snow amounts (e.g. thin layers of ~2-5cm lasting only few hours) that are anyway subject to strong uncertainty (both in terms of model numerics and meteorological forcing). The threshold of 10mm was selected to ensure consistency with Haagmans et al. (2026), it was thus set without performing a detailed sensitivity analysis. Considering the model differences in the representation of fractional snow cover between the two models, a higher threshold would likely decrease snow duration more strongly for FSM2oshd than for MEB-Crocus and thus decrease the difference between models.

We agree that it is worth mentioning the impact of this threshold in the discussion to make readers aware of its impact. As it ties well with your next point – which is why we chose to mention it when we address snow covered fraction: *'Snow cover mostly lasts longer in FSM2oshd, but complete snow disappearance is preceded by a phase of partial snow which is likely longer than in MEB-Crocus. This structural feature likely impacts model discrepancies, as does the threshold of 10mm selected to define snow cover presence, in accordance with Haagmans et al. (2026).'*

ll. 466-468. The treatment of the snow cover fraction in distributed land surface models is a major challenge for any modelers aiming to better represent cold region water and energy balance beyond point-scale. In fact, the snow cover fraction in LSM impacts the heat transfer with the atmosphere (or the overlying canopy) through the albedo or the surface temperature as much as the heat fluxes to the ground. I appreciate that you address this important factor in their discussion. I would be interested to learn more about your ideas to improve this critical component in MEBCrocus (or even FSM2!). Can you elaborate on ways to improve the sub-grid representation of the snow cover fractions in the context of this work?

Yes, it was indeed important to us to raise this issue that we feel is often overlooked. We have added the following sentence: *'Especially as, to our knowledge, snow cover fraction parametrizations specifically designed to represent forest snow are still lacking to date. Parametrizations incorporating forest structure metrics might be a worthy avenue for future model improvements.'*

A potential approach to develop such parametrizations would include point-scale simulations (and subsequent upscaling) for varying forest structure configurations – and interesting potential validation data was presented by Kostadinov et al., 2018 (10.1016/j.rse.2018.11.037) but has, to our knowledge, not been picked up for this purpose. Airborne lidar-based forest snow mapping is obviously interesting in this context as well, however very frequent acquisitions would be required to establish a dataset that can be exploited for this purpose. We decided not to add these detailed considerations as we feel they deviate too much from the storyline/key message of the paragraph.

ll.774-777. This reference appears twice in the bibliography. Please revise the corresponding entries in the manuscript.

Thanks for the catch, the reference has been corrected.

As already mentioned, this was a pleasure to read your manuscript. I hope that my suggestions, if judged relevant, will contribute to enhance this work (which is already quite strong). If the authors have any questions regarding my comments, or if they would like to discuss in more detail some of the challenges inherent to land surface models applied to cold environments, they should feel free to reach out.

Benjamin Bouchard

We are happy to learn that you enjoyed reading our manuscript! Thank you once again for the constructive remarks and suggestions that certainly helped improve it!