Exploring the potential of forest snow modelling at the tree- and snowpack layer scale – Response to reviewer # 1

Summary and recommendation

Mazotti et al. develop and present a new physics-based, multi-layer, hyper-resolution snow model (FSMCRO) that can represent high spatial and vertical resolution snow properties including grain type, density, temperature, and other snow parameters. This was achieved through a one way coupling between the FSM2 canopy model and the ensemble Crocus model, with the added benefit that ensemble simulations provides a mean for assessing uncertainty. The paper focuses on introducing and demonstrating the model at two well studied snow sites (Finland and Switzerland), with only qualitative validation ("plausibility"). The model shows reasonable representation of snow depth patterns in Switzerland (focus in the main paper) but less so in Finland (supp. material). Overall, the model shows realistic spatial variations in key snow properties (grain size, SSA) and their evolution in time along a transect spanning a forest gap with variable radiation and interception dynamics. Through the use of the ensembles and spatial simulations, the study also finds that snowpack variability (due to canopy effects on snow processes) is more important than model uncertainty.

Overall, I find this paper potentially offers a significant advance in our ability to resolve very localized snow properties which will be of interest and use to research in snow-forest interactions, wildlife ecology, and possibly avalanche studies. I think the scientific and presentation are generally of high quality, though I offer some comments and suggestion for further improvement. My main concern is about the minimal validation effort and the apparent deficiencies in snow depth simulation at one of the sites (See #1 below), and therefore request the authors consider these before publication. I emphasize this paper should be published following attention to these comments.

We would like to thank the reviewer for the careful review, the overall positive assessment of our work, and the constructive comments. Please find our answers to each comment below in blue.

MAIN COMMENTS

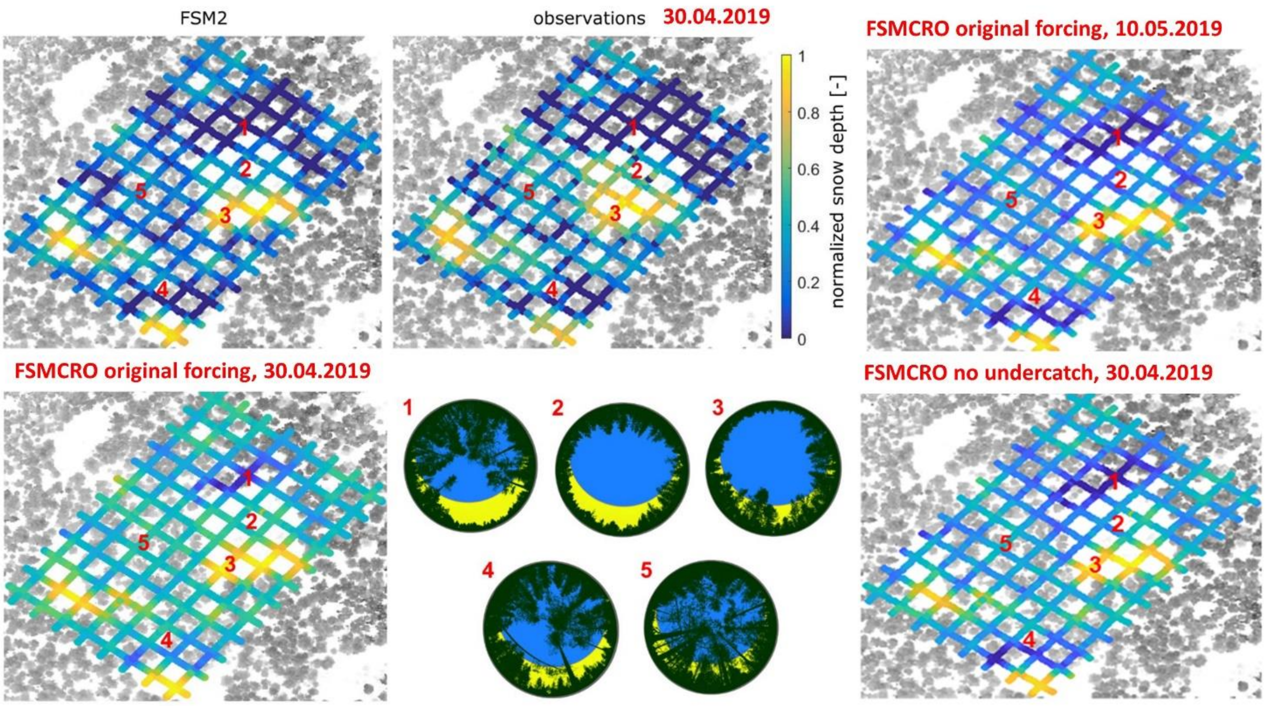
1. While the paper does not present a detailed validation but rather a demonstration of the new model, it seems there is still an opportunity to provide additional analysis to understand the "plausibility" of the model and needs for future improvements. For instance, the paper references weekly snow pit data at the Finland site, but does not make use of them due to issues with geolocation. I would argue that the geolocation issue with the pits does not preclude such a comparison, as multiple location from the domain could be selected, along with the ensemble members in order to understand the range of possible snow profiles simulated by FSMCRO. I think that a comparison between the FSMCRO ensemble and the snow pit data (grain type, density, etc.) could still be informative, even if done on a qualitative basis given the recognized challenges in comparing multi-layer snow models to snow pits. This might help to identify the plausibility of the model as well as possible deficiencies and areas for future development in the model. At the same time, this may require attention to the prominent errors in FSMCRO snow depth that are apparent at the Finland site (Figure S2, where even normalized snow depths are quite different from observations). As noted by the authors: "an adequate reproduction of observed snow depth patterns is a prerequisite for a meaningful subsequent analysis of snowpack vertical properties" (L. 285-286). Comparing to the Finland snowpit data might be helpful for diagnosing possible reasons for the deficient snow depth representation (e.g., bulk snow density?).

Indeed, validation should not be a major component of this study, yet we understand the reviewer's point and have further investigated the two issues mentioned. Regarding the relative snow depth patterns in Sodankylä (Fig S2), we have identified two reasons that have contributed to the differences in simulation between FSMCRO and FSM2:

1) For both simulations we have used a precipitation undercatch correction that was determined based on FSM2 simulations at the open site. This is a standard procedure in case of FSM (e.g., Essery et al.

2017, https://doi.org/10.5194/gi-5-219-2016), but not for Crocus (e.g., Nousu et al., 2023, https://doi.org/10.5194/egusphere-2023-338). Since this work was intended as a first model demonstration, with thorough model validation and tuning to follow separately, we left this issue unattended. However, we repeated the simulations of FSMCRO without undercatch correction, which improves the match with observations (see Figure below, lower right panel).

2) The ablation rates of FSMCRO are somewhat lower compared to FSM2, again, owing to the lack of any specific model tuning of FSMCRO at this point. Consequently, snow distribution patterns that better match those observed are only attained a few days later (see Figure below, upper right panel). The below-canopy albedo or the subcanopy turbulent exchange parameters may require some tuning to fix this issue, as noted in the discussion.


FSM2

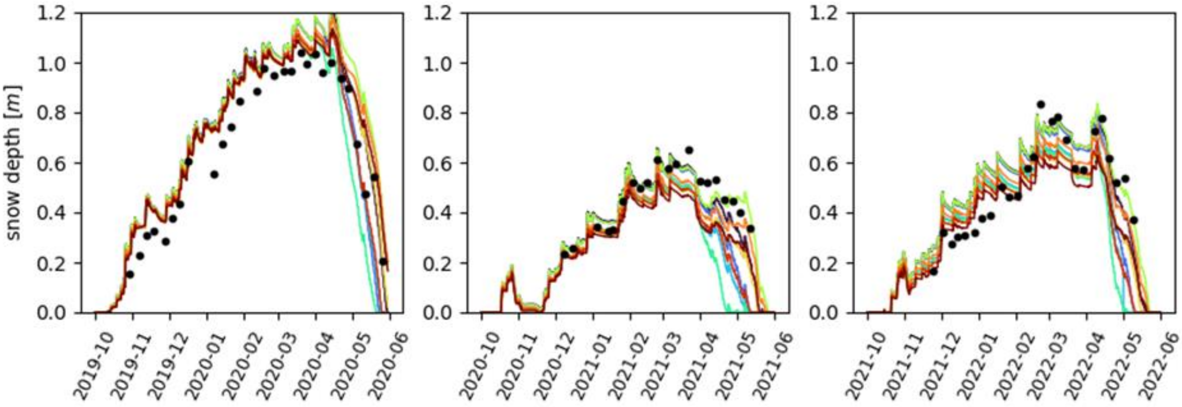
observations **30.04.2019**

FSMCRO original forcing, 10.05.2019

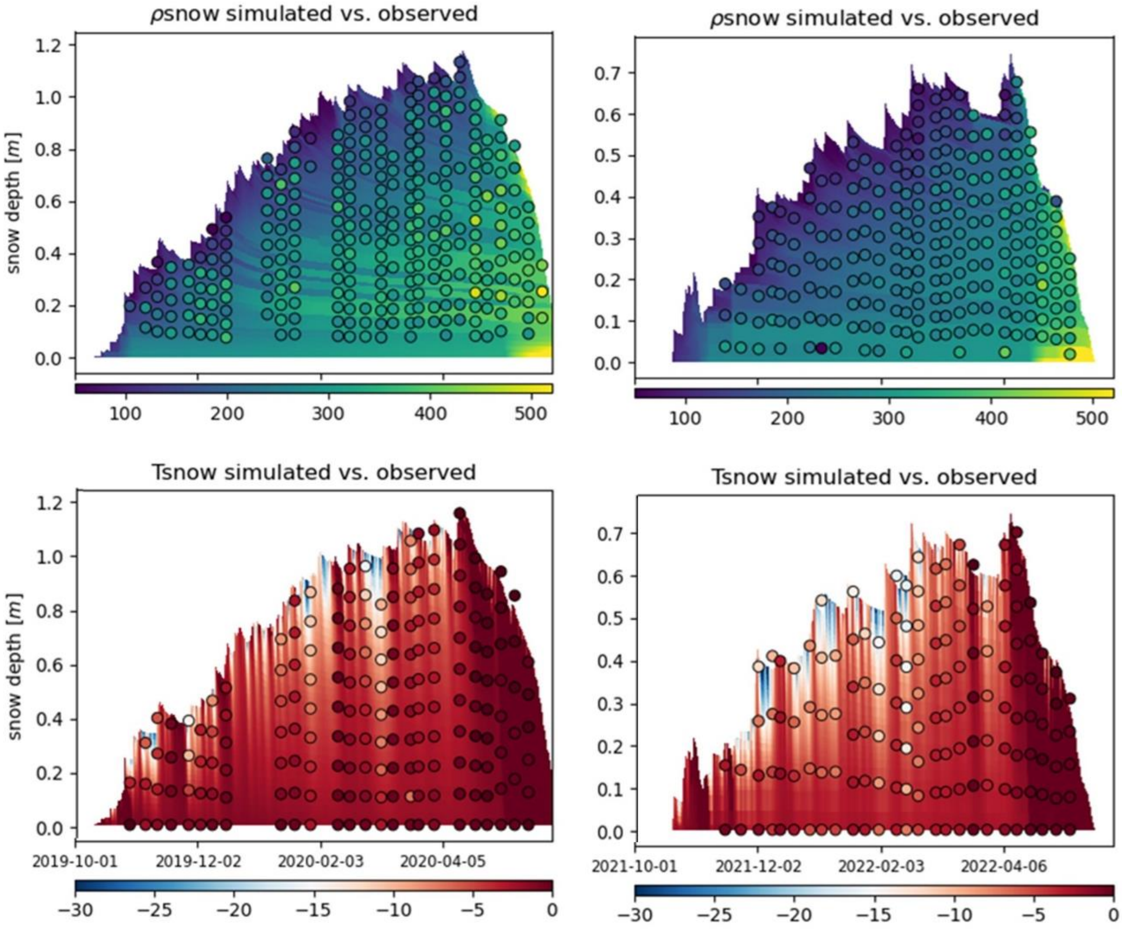
FSMCRO original forcing, 30.04.2019

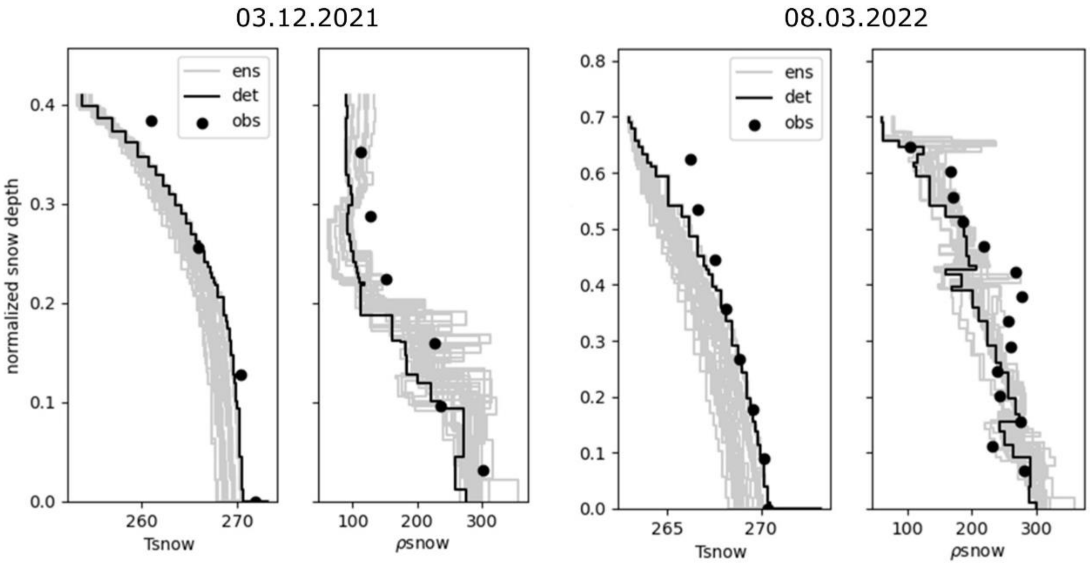
FSMCRO no undercatch, 30.04.2019

Regarding a comparison of FSMCRO simulations with snow pit data, we believe this deserves full attention in a follow up paper, as shown by various examples of dedicated studies involving snow pit validations (Bouchard et al., 2024: https://doi.org/10.5194/tc-18-2783-2024; Calonne et al. 2020, https://doi.org/10.5194/tc-14-1829-2020; Leppäenen et al. 2017, https://doi.org/10.3189/2015JoG14J026). Yet, to address your comment we explored first test simulations. Note however that forest snow pits were only available since WY 2019, which does not correspond to the study period considered in the paper. We therefore had to perform simulations for additional years at a range of locations that appear to be qualitatively 'similar' to the area where the snow pits were located (which is outside of our study area). Comparison of these simulations (lines) with snow depths recorded in the snow pits (dots) is shown in the figure below.



First exemplary comparisons of FSMCRO simulations with snow pit observations are shown below. We compared simulated and observed density and temperature profiles (for WYs 2020 and 2022, corresponding to above average and average snow conditions), as well as ensemble simulations for individual survey days (see below). These analyses will be added and discussed in the Supplementary Material. A comparison of grain type however was beyond the scope of this additional analysis and shall be left for a dedicated follow-up study. As noted by Leppänen et al. (2016; https://doi.org/10.5194/gi-5-163-2016), this measurement is the most prone to observer-related bias. Moreover, the frequent use of subclasses and mixed grain types as well as the conversion to a quantitative metric would require substantial quality control and postprocessing.



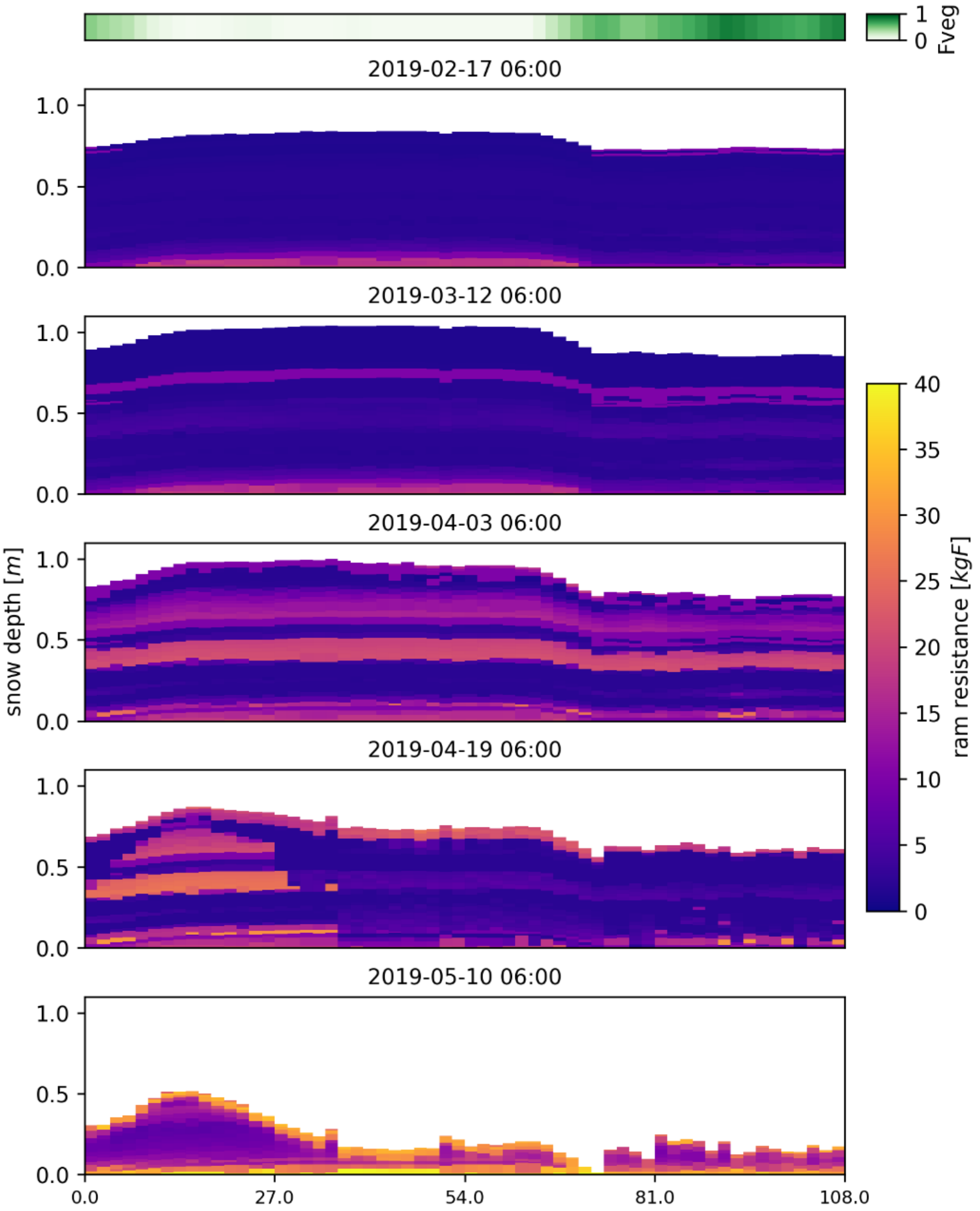


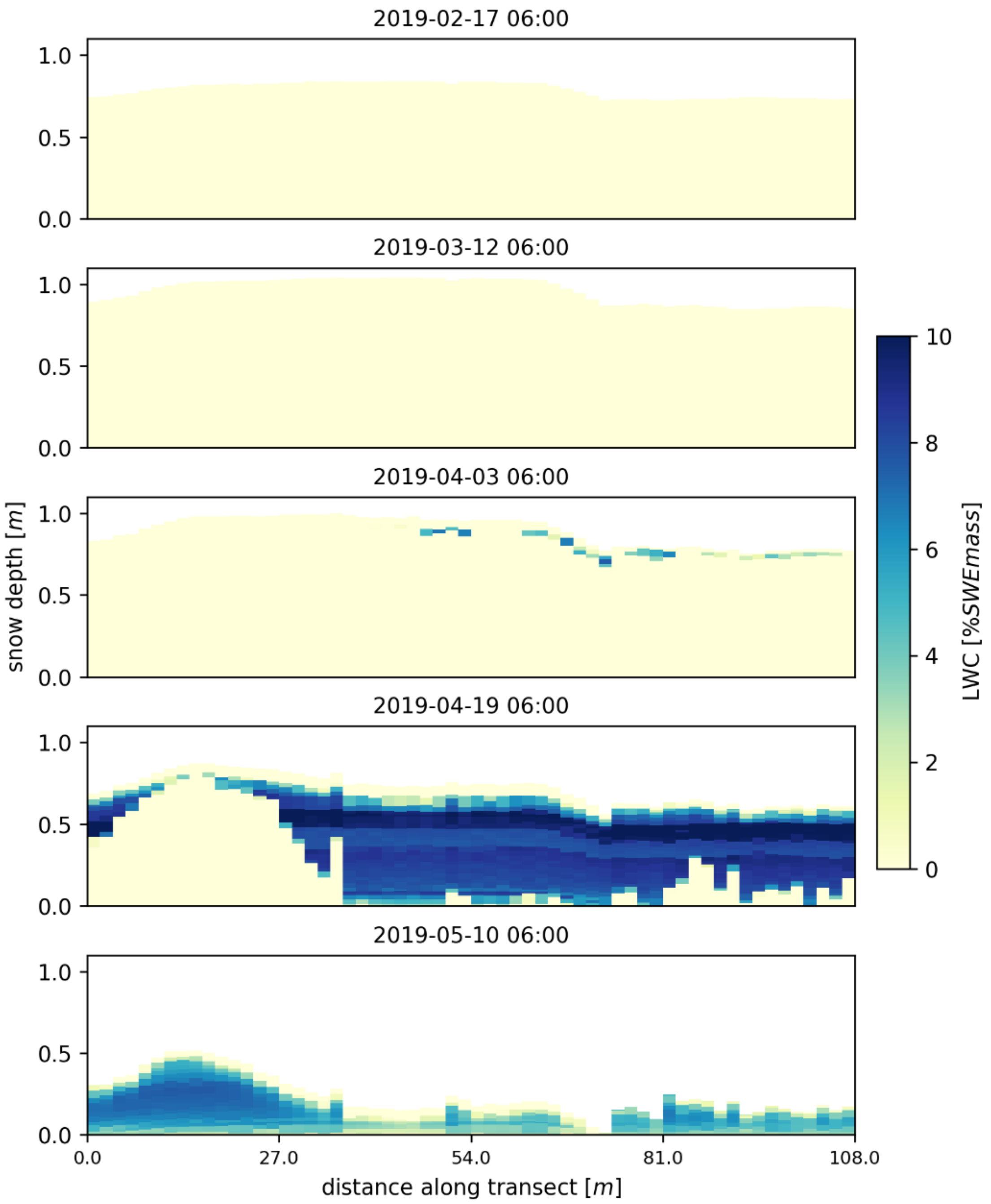
2. Several figures in the paper are not readable for someone with a red-green vision deficiency. As such, those readers may not be able to distinguish (for instance) the different snow grain types (e.g., melt forms vs. precipitation particles). I recognize this is not the fault of the authors as they are following the conventions from the Fierz et al. (2009) international snow classification report. However, I would suggest the authors consider whether something can be done to help these readers (e.g., adding a small hatch pattern to the green colors).

Thank you for pointing this out, we recognize that our figures showing grain types are not adapted to red-green vision deficiency. However, this colormap is an international convention (see Fierz et al. (2009)) that required substantial work to be established and is now widely accepted in the community. We believe it is important to be consistent with this convention and will therefore not change the figures in the main article. If providing an alternative colormap is a required addition rather than an optional suggestion, we will add an adapted version of all relevant figures in the Supplementary Material, please let us know.

3. I recommend adding snow hardness and snow liquid water content (LWC) as new figures in the supplement (similar to Figures S3-S4), as the capability for mapping these variables spatially may be of high interest to other researchers. The paper references wildlife ecology, and for that the snow hardness is a relevant parameter. Likewise, snowmelt studies and microwave remote sensing (e.g., GPR) may benefit from a model that can resolve spatial variations in LWC.

We will add figures showing liquid water content and ram resistance (preliminary version see below) to the Supplementary Material and refer to it in the main text, thank you for the suggestion. The ram resistance is available as a diagnostic variable in Crocus and a commonly used proxy for snow hardness (see Fierz et al. 2009).


1
0
Fveg
2019-02-17 06:00
2019-03-12 06:00
2019-04-03 06:00
2019-04-19 06:00
2019-05-10 06:00
snow depth [m]
1.0
0.5
0.0
0.0
27.0
54.0
81.0
108.0
40
35
30
25
20
15
10
5
0
ram resistance [kgF]



Line Comments

- L. 27: This should be “tools”. Thank you for the catch, this will be corrected.

- L. 30-35: The opening sentence is rather long and cumbersome. I recommend breaking it into two or more sentences. The sentence will be broken down into five shorter sentences as follows: ‘Seasonal snow takes many roles in the Earth’s systems. As part of the land surface, it acts as reflective and insulating material, substantially influencing the Earth’s energy budget (e.g., Thackeray and Fletcher, 2016; Colman, 2013; Sturm et al., 1997). Snow further constitutes an important seasonal storage of water, shaping the hydrograph of snow-dominated catchments (e.g., Barnhart et al., 2016; Bales et al., 2006; Viviroli and Weingartner, 2004). Snow is also a crucial ecosystem and habitat component in many regions, affecting animal movement, food accessibility, and soil thermodynamics and biogeochemistry (e.g., Boelman et al., 2019; Gilbert et al., 2017; Stark et al., 2020; Zhang et al., 2018). Lastly, snow avalanches are a common natural hazard in many mountain regions (Schweizer et al., 2003).’

- L. 215: Add “an” after “as”. Will be added.

- L. 256: The sentence begins with awkward wording. Please rephrase. The sentence will be rephrased as follows: ‘Merely, the snow metamorphism options based on Flanner and Zehnder (2006), which are not yet available in the standalone version of Crocus, were replaced by an improved metamorphism parametrization recently developed at Météo-France (‘B21’, unpublished but used e.g. in Dick et al., 2023).’

- L. 287: This is somewhat subjective and I think the sentence would be stronger if you cited the quantitative metrics here. We will list the bias values which are currently included in the Supplementary Material, only.

- L. 289: The phrase “not exactly recorded locations” is awkward wording. Please rephrase. We will change this to ‘the locations of these pits change over the winter and are not exactly recorded’.

- L. 311: Should be “Sturm”. Will be corrected.

- L. 452: Add “a” before “main”. Will be added.

- L. 475-482: Can you please clarify whether blowing snow is simulated in the model or not? I think wind redistribution should be noted here as an important process for spatial variability of snow. Blowing snow redistribution is indeed not simulated by the model. Based on our experience with measurement at these sites (see, e.g., Mazzotti et al. 2020, https://doi.org/10.1029/2020WR027572), we do not expect wind-driven snow redistribution to be a major driver of variability given the relatively low wind speeds in the forest. Yet, we will include some considerations on the implications of this missing process in the discussion.

- L. 494: The sentence has awkward wording (“did not allow to evaluate”). Please rephrase. The sentence will be revisited.

- L. 506: Should be “prey” instead of “pray”. This will be corrected, thank you for the catch.

- L. 518: Add “a” before “benchmark”. Will be added.

- L. 520-521: The sentence begins with awkward wording. Please rephrase. The sentence will be rephrased as follows: ‘This approach would enable ensemble simulations that better capture the unresolved spatial variability arising from heterogeneous canopy structure. Moreover, such ensemble simulations could also provide an estimate of model uncertainty when applied to forest sites where canopy descriptors are poorly constrained.’

- L. 531-534: Could the new snow density and snow compaction parameterizations also be impacting the snow depth overestimation?

Available evaluations of Crocus total density (Lafaysse et al. (2017); Viallon-Galinier et al. (2020); Ménard et al. 2021; https://doi.org/10.1175/BAMS-D-19-0329.1) do not exhibit a systematic bias in alpine environments suggesting that mechanical compaction is accurately parameterized in the model. However, some studies (e.g. Helfricht et al. 2018; https://doi.org/10.5194/hess-22-2655-2018) suggest that the density of new snow is overestimated by the Crocus parameterization of falling snow. This could lead to a temporary underestimation of snow depth after recent snowfalls while on the contrary our results exhibit an overall snow depth overestimation. Therefore, this parameterization is not expected to be the main reason for the overall snow depth overestimation. Recently, Wooley et al. (preprint, : https://doi.org/10.5194/egusphere-2024-1237), showed that snow density may be underestimated by the default Crocus configuration in Arctic environments due to the unsimulated impact of wind-drift induced compaction, yet wind speeds are low at our site. See also our reply to your main comment #1 above.

- L. 531: This focuses on one of the evaluations of the modeled snow depth, however, I think it is best to also acknowledge the prominent deficiencies in modeled snow depth at the Finland site in April (Figure S2). See my first major comment above. The deficiencies of the model at the site in Sodankylä will be specifically mentioned in a revised version of the manuscript, see our answer to the main comment #1 above.

FIGURES

- Figure 2, Figure S1, and Figure S2: Please add a scale bar. Scale bars will be added to all these figures.

- Figure 2: Please clarify in the caption what blue represents in the hemispherical photos. I believe it is in the sky portion outside the solar track but it would be helpful to state this in the caption. It is correct that blue represents the sky portion in the image. We will clarify this in the caption.

- Figure 4: I wonder if it would be useful to show a plot of mean direct beam transmissivity at each location on the transect? This could go just below the Fveg and could have similar dimensions/scale. This is not a required revision but merely a suggestion if it helps to show the shaded area in the open gap on the left side of the figure. Thank you for this suggestion, we will consider this addition to the Figures showing the transect.

- Figures 4, 5, 6, S3, S4, … : It could be helpful to add “S” on the left and “N” on the right at the top to indicate the south-to-north orientation of the transect. We will add these labels as suggested.

- Figure 7: I suggest adding a map on mean canopy transmissivity, which I suspect might aid in interpretation of the spatial patterns here. Thank you for this suggestion, a map of mean canopy transmissivity will be added to the figure.