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Reply on RC2

Giulia Mazzotti et al.

Author comment on "Canopy structure, topography, and weather are equally important drivers of small-scale snow cover dynamics in sub-alpine forests" by Giulia Mazzotti et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-273-AC2, 2022

Author's reply to Reviewer Comment #2

Author reply in italics

In the paper, the authors mainly presented an application of a new snow cover model FSM2 in a sub-alpine forest, and tried to understand the snow spatio-temporal distribution and its drivers mostly with the modeled results. Given still lacks wonderful snow models that can perform the complex snow dynamics for the mountainous terrain, especially with high resolution simulations, the topic in this paper is very interesting and important. The authors integrated the datasets of four observed snow depth data, Lidar-based canopy structure and elevation with the 2-m grid cell simulated results for 6 winters, and examined the snow energy balance affected by canopy cover and tograph. The paper fits with the scope of HESS and the results are overall well presented, however, the paper missed a lot of information on the study site, and the methods and modeling approaches are not clear. I have some following issues for the authors to consider.

Thank you for the overall positive assessment of our work. Please find our detailed replies to major and minor comments below. We are confident that these address your main concerns, and we thank you for pointing out any lack of clarity. We would, however, like to note that this study aims at a scientific application of a model that has been introduced and discussed in previous work by the authors, especially Mazzotti et al. 2020a,b (doi:10.1029/2019WR026129, doi:10.1029/2020WR027572). While we will add missing methodological information where necessary in the revision, we believe that the manuscript is already too long to accommodate detailed descriptions of the modelling approaches, and believe directing reviewers to the previous two publications to be sufficient.

- Canopy structure: Canopy structure refers to horizontal canopy cover and vertical canopy distribution (such as vertical layers, leaf area index LAI). Here in the study, the authors only focused on the canopy cover fraction, but did not pay a lot attention to other structure factors, such as canopy height and LAI. To avoid the confusion, I recommend the authors use 'canopy cover' to replace the word 'Canopy structure'. Did the authors find the impacts of canopy height and LAI on the snow interception? It is better to provide that information in the revision. I am not familiar with the sub-alpine forests. If you can provide more site information on the tree species (coniferous or deciduous species?) and forest structure (such as canopy layer and understory structure), it will help the readers better understand the background of the canopy cover and LAI. In addition, the simulation of snow dynamics in forests mainly depends on the radiation transfer model of canopy. In the paper it seems that the canopy radiation transfer model used is a single layer canopy model. Since the canopy height can be 5-35m mentioned in your supplement, the single-layer hypothesis will probably cause the model bias, especially for the understory snow dynamics. The authors should clarify those uncertainty and limitations.

The reasoning behind the choice to use canopy cover fraction when quantifying relationships between local canopy and snow descriptor metrics is explained in the methods (L202ff., Section 2.4). As we focus on the combined impact of canopy, topography, and meteorological conditions, one metric to quantify the quantity of canopy was sufficient for our purpose. Detailed analysis of the relationships between individual processes, snow distribution, and sophisticated canopy descriptors, has been subject of earlier studies (e.g., Moeser et al. 2015, doi:10.1002/2014WR016724, for interception; Lawler and Link 2011, doi:hyp.8150, for sub-canopy incoming radiation; Hojatimalekshah et al. 2021, doi:10.1002/hyp.8150, for snow distribution) and is therefore beyond the scope of ours. However, note that FSM2 does include more canopy descriptors than just canopy cover fraction (see Section 2.2). Therefore, the use of 'canopy structure' instead of 'canopy cover' is necessary due to the context of this study, where 'canopy cover' is used as an input variable to the model, while 'canopy structure' as a term to describe the general static state of the canopy/trees/forest that is represented by the different canopy descriptor variables (or 'canopy structure metrics'). Thank you for pointing out this source of confusion: we will carefully reconsider all instances of these expressions in the revision to ensure that the correct one is used.

Site information (e.g. tree type and heights) is included in Section 2.1; the understory mainly consists of grasses and blueberry bushes. We did not provide this information as it does not affect the model simulations but will add a note in the revision.

Finally, as you correctly note, the canopy in FSM2 is represented with one layer. Heterogeneities in canopy height are captured by including canopy structure metrics that integrate the hemispherical perspective at each modelled point, which accurately represent the three-dimensional features. The calculation of diffuse and time-varying direct transmissivities for shortwave radiation both rely on this hemispherical perspective and are thus much more detailed than if a homogeneous 'bulk' canopy layer was used (e.g. as characterized by a site-averaged LAI). A detailed discussion of this radiation transfer modelling approach and its accuracy is provided in Jonas et al. 2020 (doi:10.1016/j.agrformet.2020.107903), and the LiDAR-based version is presented in Webster et al. 2020 (doi:10.1016/j.rse.2020.112017). The integration of hemispherical-image-based radiation transfer model and FSM2 and the associated model improvements achieved with this approach is subject of Mazzotti et al. 2020b. The reader is referred to this publication for further detail, where a thorough discussion of the accuracy of individual process representations has already been carried out and is therefore beyond the scope of this present study of the application of the model. Please note that the most relevant model limitations are addressed in the discussion (Section 4.3).

- Wind:

In this paper, the authors mostly considered the meteorological factors of air temperature and solar radiation. However, for mountainous terrain, wind is widely recognized as one of the dominant controls of snow accumulation and distribution, including the alpine regions. Wind patterns interacting with topography determine the patterns of final snow distribution on the landscape, while the surface wind and turbulence also control the snow sublimation and melting processes via energy exchange. Unfortunately, I didn't find any relevant results and discussions in this paper. The authors should provide the above corresponding content in their revision.

Topographic influences on wind and precipitation fields are (at least partially) accounted for by the meteorological input fields, as described in Section 2.2, L147ff. The effects of fine-scale topography (e.g. ridges) are not captured, and FSM2 does not (yet) include wind-driven snow redistribution processes. While snow redistribution is largest close to ridges (e.g. Mott et al. 2018, doi: 10.3389/feart.2018.00197), the tree line is often at lower elevations in alpine valleys. At our study sites, within-forest wind speeds are generally low and canopy structure controls on snow distribution outweigh signals of re-distribution of snow within the canopy by wind (see e.g. Mazzotti et al. 2020b, Koutantou et al. 2022, doi: 10.1016/j.coldregions.2022.103587). Impacts of wind-induced snow redistribution have been reported in some forest configurations in the Western U.S. (e.g. Currier and Lundquist 2018, doi:10.1029/2018WR022553, Dickerson-Lange et al. 2021, doi: 10.1029/2020WR027926). We will comment on wind effects on snow distribution patterns and consequences on the transferability of our results in Section 4.3 of the discussion (Assets, limitations, and outlook) in the revised manuscript (see also reply to Reviewer #1). In the same section, we will further mention ongoing efforts to couple FSM2 to 1. a high-resolution atmospheric model specifically suited for the simulation of small-scale wind and precipitation patterns in Alpine terrain (an upgrade of Gutman et al. 2016, doi:10.1175/JHM-D-15-0155.1) and 2. a snow redistribution scheme (Liston et al. 1998, doi: 10.3189/s0022143000002021). Corresponding publications documenting these developments are currently in preparation.

The impact of wind on turbulent exchange within the canopy is included in FSM2. Details on the relevant parametrizations (which are based on commonly used flux-gradient relationships) are provided in the publications documenting FSM and FSM2 (Essery 2015 doi:10.5194/gmd-8-3867-2015, Mazzotti et al. 2020a,b). Mazzotti et al. 2020b showed that within-forest wind speeds are less dependent on local canopy structure than radiative transfer processes, therefore spatial patterns of sub-canopy snow sublimation are assumed to have a relatively small impact on snow distribution patterns. We will comment on this aspect in Section 4.3 as well.

- Topography:

The authors used elevation, slope direction to represent the impacts of topography on snow dynamics. This is generally sound, however, in very steep terrain in particular, topography mostly can affect the evolution of snow water equivalent SWE, and thus patterns of meltwater generation. How did the author consider the gravitational snow redistribution during winter? I recommend the authors also provide the site information on slope angle to help the readers understand this.

This is a good point, thank you for addressing it. We will add maps of aspect and slope to the Supplement S2 and appropriately refer to them in the revised main article. The modelling framework accounts for slope angle for the calculation of incoming solar radiation, but gravitational snow redistribution is not included. Note that very steep slopes are rare in our study domain, which limits the impact of omitting this process in our simulations. As in the case of wind-induced snow transport, established models do exist to represent these processes (e.g., Bernhardt and Schulz 2010, doi:10.1029/2010GL043086), efforts to couple such a model to FSM2 are ongoing at SLF and a corresponding publication is in preparation. We will comment on these model limitations and current developments in Section 4.3 of the discussion (Assets, limitations, and outlook).

Specific comments:

Methods

1. What's the time step or temporal resolution of your simulation? How did you intercept the data for your periodic results?

The FSM2 model runs at 1h time steps and outputs daily results (it could output hourly results as well, but daily results were sufficient for the purposes of our work). We will make sure to specify this in the methods section of the revised manuscript.

2. How did you define the water year?

A water year (also referred to as 'hydrological year') is defined as the 12-month period between September 1st of any year and August 31st of the following year (e.g. water year 2022 started on Sept 1st 2021 and ended on Aug 31st 2022, see Wikipedia). The term is commonly used in hydrology, and we found it more convenient to refer to our simulations than a calendar year, as Northern-Hemisphere snow seasons (and therefore our simulations) usually begin at the end of a calendar year and continue through the first months of the next.

- How about the uncertainties caused from the input datasets in your study?

As for every modelling study, uncertainty in the input data is unavoidable and doubtlessly affects model accuracy. Yet, we use the best available meteorological forcing and include downscaling methods that were specifically developed for complex terrain. The meteorological input fields can thus be regarded to be realistic, and spatially explicit simulation outputs are consistent with this meteorology. Model errors arising from uncertainty of the meteorological inputs are not relevant for this study because we do not aim at assessing model accuracy but rather at analyzing spatio-temporal patterns in the results, which generate from the modelled impact of canopy and topography. A discussion of input data uncertainties is thus beyond the scope of this study; but note that it is the subject of a very interesting publication by Günther et al. 2019 (https://doi.org/10.1029/2018WR023403). We will refer to this work in the revised manuscript.

- Section 2.4, Regarding the SWE and Snow cover, were they the total of canopy interception and through canopy snowfall? Did you distinguish opening and canopy areas when identifying the SWE peaks and changing periods?

SWE and snow cover refer to snow on the ground, i.e. under the canopy. We will specify this in the definition of the snow metrics in Section 2.4 and thank the reviewer for identifying this lack of clarity. In FSM2, as in any process-based mass- and energy-balance forest snow model, it is the result of snowfall, minus interception, plus unloading of snow previously intercepted in the canopy (c.f. Essery et al. 2009 doi: 10.1175/2009BAMS2629.1 for a general overview of forest snow processes included in such models, and Mazzotti et al. 2020b for the description of the parametrizations used in FSM2). Peak SWE was evaluated at each modelled point individually as maximum value achieved in a water year. The same algorithm to extract this value from the simulation results was applied for openings and canopy areas. Note however, that timing of peak SWE differed depending on a point's location and canopy cover (in fact, peak SWE timing is one of the variables we analyzed, see Section 2.4). Due to these within-site differences in peak SWE timing, also the timing of accumulation and ablation periods differed between points - but they were calculated in the same way regardless of whether a point was located below the canopy or in the open. We hope that these explanations answer your question, as we are not sure what you refer to by 'changing periods'.

Results and Discussions:

l159: What is the data collected frequency?

Snow depth data at stations is collected daily, we will specify this in the revised manuscript where the data is introduced (L159). Planet imagery is available at approx. weekly intervals (see L161). Airplane and UAV borne LiDAR acquisitions occurred campaign-wise, i.e. at just a few points in time. This is mentioned in Section 2.3. and we will refer the reader to the respective publications for further detail on these campaigns.

L159-165: Which place does the snow depth data represent for forests? On the top of the canopy or for the whole ecosystem?

If not specified otherwise, snow depth refers to the depth/height of snow on the ground (i.e. under the canopy or in canopy gaps). This is the general case for LiDAR derived snow depth datasets and thoroughly described in the referenced publications, which is why we did not explicitly mention it. We will do so in the revision.

L163, What does ASO stand for?

ASO is the Airborne Snow Observatory, an airborne LiDAR mission in the Western US (see respective citation in the paper and L135), but it is true that a proper definition of the acronym was missing. We will add it for clarity, thanks for catching this.

L263, Please provide the supporting materials for the statement on under canopy vs open vs gap areas.

This statement refers to the data shown in Figure 5b, expanding on some of the detailed peak SWE distribution patterns shown. We will add a reference to Figure 5b at the end of the sentence to clarify this.

L268, Please provide the supporting materials for the statement on correlation.

The correlation coefficient was calculated based on peak SWE and canopy cover data over the entire modelled domain. Figure 5 shows a subsection of these data, the full domain is shown in the Supplement (Figure S 2.2). We will add full-domain data of the snow cover descriptors for all modelled years to the Supplement for completeness.

L308-309, Please clarify how did you conduct the canopy structure and topographic settings specifically?

These points are chosen as examples that serve to illustrate a systematic comparison of snow cover evolution and underlying energy exchange processes at contrasting canopy structure and topographic locations. We thus selected a set of points that covered the full range of canopy covers (0.02-0.9) and included both north- and south-exposed locations, as well as vicinity to north-and south-facing forest edges (selected manually without quantitative criteria). The selection of these specific points rather than another set of points with the same set of characteristics is arbitrary.

L406, How did you define 'semi-open conditions'?

This formulation is meant to be descriptive rather than exact and aims at contrasting the examples shown in Figure 6 to those discussed in this section and Figure 10. The former represent the end members of the canopy and topography ranges (see reply to the previous comment), hence their comparison would reveal rather consistent snow cover and energy partitioning pathways throughout the simulated years, while the latter can exhibit between-year differences (illustrated in Figure 10). Please see our reply to the next comment for the quantitative range of canopy cover included in this example, but also note that, as pointed out in the reply to the previous comment, the exact choice of these *points is arbitrary to some extent.*

L410, What is the exact range of canopy cover?
The points shown in Figure 10 feature fractional canopy covers of fveg = 0.25-0.75, and sky-view fractions of 0.2-0.5.

L427, Here the citation of fig 10a may be not correct.

The reference to Figure 10a is correct. The sentence serves as introductory sentence to the following ones where the difference in melt out shown by the different lines in different years is explained.

L505-506, Snow water input is also affected by the slope angle in the mountain regions.

This is correct. As mentioned in reply to an earlier comment we will specify the lack of consideration of slope angle as a limitation of this study and subject of future work in Section 4.3 (Assets, limitations, and outlook) of the revised manuscript.

Figures and tables:
All figures: Please point out if the data is from observations or simulations.

Besides Figure 3, all results figures only show simulations. This is already mentioned explicitly in Section 2.4 (Analysis approach, L180), but we will highlight it again in the introductory paragraph of the results chapter (3).

Fig 1, What does SLF stand for?

The SLF is the Snow and Avalanche Research Institute in Davos, Switzerland (Institut für Schnee und LawinenForschung). This is specified in L114, but we understand that the definition of the acronym was not clear. We will specify this for clarity in the revised manuscript.

Fig 2, What do SSD,PSD,SDD stand for?

The acronyms are defined in Section 2.4 where all our metrics for the analysis are listed. But it is true that the Figures should be stand-alone and we will add the acronym definition to the caption of Figure 2 in the revised manuscript. Thank you for pointing this out.

Fig 3, It will be better to add an ANOVA test for fig3e. Why didn't you also show the south-exposed slope for Fig3e ? In fig3f, what are the differences between left and right panels?

As mentioned in the text (L157ff), the purpose of Figure 3 is to provide a model plausibility check that confirms what previous publications (Mazzotti et al. 2020a,b) have already shown: that FSM2 is an adequate tool to explore forest snow process variability and resulting snow distribution patterns. Figure 3 illustrates and summarizes this plausibility with examples including a variety of observational datasets, which allowed us to verify several aspects of model 'performance' (spatial patterns and temporal evolution). The figure is not meant to report the exhaustive evaluation that was done, and more subfigures would detract from its overall message, which is why we included only one of the slopes surveyed with the UAV LiDAR.

The omission of quantitative goodness-of-fit measures (such as an ANOVA) is conscious. Discrepancies between the model results and the data do exist and generate from a variety of factors: 1. uncertainty in meteorological forcing; 2. uncertainties in the data *(e.g. influences of ground roughness on measured snow depth distributions, inaccuracies of the LiDAR datasets), and 3. uncertainties in the model itself. All these would impact the goodness-of-fit measures, but the first two are not relevant for the design of this study and the use of FSM2 for its purpose. As noted in reply to one of your earlier comments, discussion of simulation error sources is beyond the scope of this study. To ensure that the manuscript storyline is streamlined and serves its scientific goal, we will therefore not include goodness-of-fit measures in the revision.*

Fig 6a, The last two legends have the same titles.

The last two legend entries are the same on purpose, because both points are located under canopy and in a south slope. However, we understand that this might have been confusing. We will add a distinction between the two points in the revision (canopy – S slope – sun exposed vs. canopy – S slope – shaded).

Fig 8, What do SWR, LWR, SHF, and LHF represent?

These acronyms stand for the different energy balance components (net short and longwave radiation, sensible and latent heat fluxes), and you are right that this was not explicitly mentioned. We will ensure that this is specified clearly in the text and the figure legend, thank you for catching this.

Fig 10b-d, I didn't find any legends.

Good catch. The legend is the same as for the equivalent plots in Figure 6, but we need to add the legend here as well and will do so in the revised manuscript.

Fig11a, Does it refer to the total melt depth or melt rate.

This is the total amount of depletion in mm water equivalent. We will clarify this in the text and figure caption of the revised manuscript.