Reply to reviewer comments

***Black**: reviewer comments – **Blue**: author replies*

Line numbers refer to the revised manuscript with tracked changes.

Reviewer 1

Line 530 Giulia addressed my comment that other tiling models did account for topography but then did not change the paper sentence at all: "Our findings indicate that tiling strategies could be further refined by additionally accounting for topography." The papers you cite did account for topography in the terms of different meteorological input, so I'm confused by your wording here. I'd suggest to reword as "When interpreting forest-snow patterns across any scheme, one should also consider local topography." That's a true statement and I think still gets at what you mean without saying that the other studies neglected topography. Thank you.

Thank you for pointing this out, our sentence was indeed not meant to imply that existing strategies disregard topography. We have removed the misleading sentence entirely.

Reviewer 2

For the manuscript, I read it carefully and provided many detailed comments for the authors to improve their work. In the responses, the authors provided some explanations for my comments and accepted some of my suggestions. However, some of my key concerns were not well addressed.

We are sorry that the reviewer found our responses unsatisfactory. We would like to note that our reply letter was six pages long and that we addressed all the reviewer's comments. Wherever we disagreed with the reviewer's suggestions we provided extensive argumentation, including references to relevant literature. However, we realize that some of our replies might not have been ideally formulated, and hope that the revisions detailed in the replies to the reviewer's individual comments below now better address their concerns.

To my concerns about the methodology and modeling approach, the authors simply responded to that they have "discussed in previous work by the authors, especially Mazzotti et al. 2020a,b (doi:10.1029/2019WR026129, doi:10.1029/2020WR027572)." I understand that the methodology should be described in a concise way, but at the same time, it isn't easy for the readers to read several scientific articles to know how the research was carried out and whether the results are convincing. Additionally, I've investigated the papers by Mazzotti et al. 2020a, but unfortunately I still haven't find all relevant discussions/explanations of my concerns.

While we did adapt several sections in the revisions to add details about the methodology and took the effort to provide the reviewer with detailed responses to all questions related to methodology, we acknowledge that readers that are not familiar with our earlier work might need even more background information to fully understand this study. Upon further discussion with the editor, we have added a paragraph that summarizes the main assets of FSM2 as established in Mazzotti et al. 2020a,b (doi:10.1029/2019WR026129, doi:10.1029/2020WR027572) to the introduction (L 73 ff). This information should clarify why the chosen modelling approach constitutes an adequate tool to investigate our research questions. We have further expanded the model description in Section 2 (L 139 ff.) and added a new section to the Supplementary Material that illustrates the concept of process-specific canopy structure descriptors, including an additional figure (Figure S2.1).

Since canopy cover fraction is only a horizontal factor of canopy structure, I suggested that the authors use specific 'canopy cover' instead of general 'canopy structure' and provide more site information to support their hypothesis in modeling canopy structure. However, the authors declined. They responded that the impact of canopy structure on the snow interception are beyond the scope of this manuscript, and this site information would not be helpful for the model simulations. These responses are not satisfactory to me. You know that the title of this manuscript is "CANOPY STRUCTURE, topography and weather are equally important drivers of small-scale SNOW COVER DYNAMICS"! And the tree species (coniferous or deciduous species), tree hight and understory tructure definitely can strongly affect the simulation of snow cover, as reported in most studies.

We realize that the reviewer likely did not understand that FSM2 includes more canopy structure metrics than just canopy cover and that it resolves subcanopy radiation by accounting for the surrounding canopy structure in three dimensions. In fact, the radiation modelling approach based on hemispherical images accounts for the shape, density, and fine structure of every single tree in the model domain, resulting in ray-tracing like accuracies in the radiation transfer calculations. We may have missed an opportunity to better highlight the merits of our modeling approach (given these were extensively discussed in previous publications), but we believe the title is an adequate representation of our study which is in line with the capabilities of our model framework. Note that we did change instances of 'canopy structure' to 'canopy cover', reconsidering every individual occurrence, and explained why some were left unchanged. Also, site information was added as requested. As mentioned in our reply to the previous comment, we have now further extended the model description (Section 2) and added a section with a conceptual figure in the Supplementary Material (S2) to include additional detail on the computation and use of process-specific canopy metrics, and in particular on the radiative transfer model used in tandem with FSM2 for our study.

Another concern I had is the uncertainties and limitations of their simulations. The authors responded that "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". I have to ask how can the readers trust the results if they don't know/understand about the model biases or limitations. Also, for the modeling work, it is important to quantify the discrepancies between the model simulations and the observations, however, the authors declined to do additional statistical work, such as ANOVA tests, again because 'discussion of simulation error sources is beyond the scope of this study'.

We understand and apologize if our response left the impression that we don't care about model uncertainties. Rather, our reply was meant to be seen in the context of section 2.3, where we explicitly explain that the model has been extensively tested and validated (in the same area, with the same meteorological input data, and with the same canopy structure datasets, c.f. Mazzotti et al, 2020a), that we did not see the point of repeating such a validation. Yet, we offered a plausibility check of the current model use case, comparing the model output against a range of independent validation datasets (satellite imagery, crewed and uncrewed aerial lidar scans, c.f. Figure 3). To our knowledge, FSM2 is the only forest snow model for meter-resolution simulations worldwide, that has ever been validated at the level of individual processes, in an explicit effort to constrain uncertainties of the entire model chain, and after many years of developing methods to collect appropriate validation datasets. The last paragraph of the introduction, which states the purpose of our study, has been adapted to better convey this information. We acknowledge that we missed to express that we replied to rebut the need for further validation, and not for model validation in general. To better address concerns about model uncertainties, we now provide a summary of previous model validation efforts from Mazzotti et al. (2020a) and Webster et al. 2020 (doi:10.1016/j.rse.2020.112017) in a new Section of the Supplementary Material (S3), including three figures (Figures S3.1-3.3).