This study evaluated model parameterizations and configurations determining turbulent exchange and soil and vegetation and their influences on snowpack and surface energy fluxes using station-based eddy-covariance based energy flux data, snow depth, and soil temperature observations in two boreal and subarctic peatlands and forests in Finland. They found that a stability correction function is a key part to simulate sensible and latent heat fluxes over snow. Also, a realistic soil texture (soil organic carbon) parameterization led to improvement of the soil temperature simulations in peatlands.

The study presents insightful results describing model configuration in boreal peatlands and forests where are the most challenging regions to estimate snow energy balance budget. While the findings from this study should be tested by other snow model and LSMs to be widely generalized, as a case study, this study provides useful implications for choosing suitable turbulent flux parameterization and model structures to better estimate snow energy fluxes in soil-vegetation-snow interactions.

Even though I feel this manuscript is lengthy as a single manuscript, the paper is generally written well, and the presentation quality of the figures is great. In my opinion, however, concerns need to be addressed upon before publication is warranted. I have given a few suggestions below that may contribute to the improvement of the manuscript.

General comments

1. It is unclear to me about differences in MINERAL vs. SOC model runs to assess for assessing soil thermal regimes. Is the two model runs' difference soil texture only? In Table 2 of peatland sites, the authors assumed 100% SOC of soils. But in Figure 8, MINERAL was run as well. Does it mean the authors used "mineral soil" instead of 100% SOC with the same model? I don't know clearly how the ISBA parametrize soil texture but if "mineral soil" is continuous values (not categorized) within the model, it would be much more useful if you provide sensitivity of soil temperature and energy fluxes to different SOC percentages, e.g., 0, 20, 40, 60, & 100%).
2. Even though the MINERAL vs. SOC comparison showed a more realistic soil texture (SOC 100%) provided a better performance, there were still unexplained portions of the modeled soil temperature as compared to observations. I would recommend including some discussions regarding the unexplained portions – how the model could additionally improve soil thermal regimes (e.g., soil moisture contents).
3. I found "not shown" in several places. I understand the authors may want to keep the manuscript's length readable. However, "not shown" may not be helpful for potential readers who may want to keenly read this paper. I would recommend including any additional figures and tables that can support the findings from this study as Supporting Information (or Appendix).
4. I wonder if there are any specific reasons focusing on 2018-19 winter. Was the winter a representative year in terms of snow climatology (e.g., moderate snow condition)? I would suggest providing a justification regarding that.

Detailed comments

Figure 1. I think the current version of the figure does not extremely useful. Any detailed photos at the sites with positions of flux towers and soil temp & snow depth sensors may be needed. That would be helpful to better understand observations for those who are not familiar with those

environments. Additionally, please include the site points on aerial photos with a scale bar. If the authors need more space, I recommend including them into Appendix.

L160 transpiration from the vegetation, Etr

L209 It would be good to include a citation (Vionnet et al., 2012) here

L222-223 My understanding is that at the bottom of the snowpack, Crocus is coupled to the soil components of the ISBA LSM. Please describe it explicitly.
What does "a mass and energy-conserving semi-implicit solution" mean? A detailed description of this would be helpful to understand.

L223 "Is there a specific equation of the heat conduction G related to the temperature gradient? I would suggest including this or citing a relevant reference.
Did you use the heat conduction G as the same concept to the surface heat flux into the soil-vegetation composite (G; Eq 1)? If not, please use a different acronym.

L291 What does "100% of SOC" mean? How much is this when it's converted as kg/m2? In Table 2, this should be expressed as kg/m2 for consistency.

Regarding the assumption, I would strongly suggest providing any justification of this assumption with sufficient references (e.g. why this is reasonable).

L360 For model evaluation, have you considered daily-maximum and/or minimum values in addition to the daily-average values particularly for surface energy flux and soil temperature? Because of strong diurnal patterns of these variables, I think it would be useful.

Figure 7 please change the color of HS (obs) to black or others to make a difference from HS (mod).

L425 Should "N-FOR" be corrected as "N-WET"? Figure 7 doesn't provide N-FOR time series

L426-426 I believe the authors used the ISBA-VS approach only on the peatland sites. But I'm curious how the results will be changed with ISBA-VS because this statement indicates that the uncertainty would be improved when the model considers fractional snow cover. Please consider

In Figure 7, during the melting period, there were different model errors in LSA between two sites (e.g., N-WET: underestimation but S-WET: overestimation). Please include some discussion regarding this.

L441 Please reword this sentence. It's unclear to me.

L443-447 The authors mentioned "On both sites" but a figure of N-WET wasn't presented here. I would recommend including the N-WET results as well (even in Supplementary Info) for those who are interested in. I'm not a big fan of "not shown".

Did "SOC" simulation mean "M98" simulation (L345-347)? Because the acronym SOC also means "soil organic carbon" itself, it's a little bit confusing. Please use it for either one. Also clarify what the two simulations are fundamentally different in Sec 2.5. To my understanding, in MINERAL, the soil was characterized as "mineral" while SOC (or M98) as "100% SOC". Is this correct?

L456 I don't think Figure 9 shows the ISBA-VS model version heavily overestimates accumulation, except 2021 for N-FOR.

L456-458 Can you provide a figure comparing two runs with between Wsw = 5 and 0.2? What does it physically mean? In section 2.3.1, the author mentioned "The coefficient Wsw relates to vegetation characteristics" but I don't think it would be enough to physically understand the parameterization and the results here. Please provide more details.

L462-463 How did the authors know that the results are due to overestimated compaction and uncertainties in snowmelt? For example, have you compared snow density (as a proxy of compaction) with observations? If so, why did the errors of compaction and melt processes occur in terms of model physics - particularly snow energy processes?

L464 Again, please include your relevant result in Supporting Info instead of "not shown".

I think the authors would be able to discuss more in this section (3.3.2) because there are lots of interesting patterns such as the cold biases in fall. Can you add air temperature in this figure for comparison purposes? I'm guessing that the modeled soil temp seems to be much more sensitive to air temperature. Also, I would suggest adding discussion about the differences in soil temperature between ISBA-FS and MEB.

L489-491 So how would the roughness length and turbulent exchange coefficient (CH) be used differently for soil and vegetation to minimize the overestimations of soil evaporation and sublimation?

Figure 12, for N-FOR, were the LSA observations missing or larger than 0.7? Please justify. To me, it is questionable why the LSAs from models were constant throughout the season, likely regardless of the existence of snowpack, which don't seem to make sense. Please discuss the potential causes.

L498 "neglect" -> "do not estimate"?

L547-549 I don't think this statement is sufficiently supported by this study because the snow depth patterns from the models highly depend on year-by-year (Figures 5 & 9). "Wind erosion is higher in peatlands" is generally correct, but have you seen the wind data in the sites to see any wind effect?

Table 3 "Weakly stable" sounds somewhat weird. Please consider an alternative.

L584-587 Does Crocus itself provide soil temperature simulation? I believe it only estimates snow and is coupled with ISBA to estimate soil parts. Thus it would be better to state something like "ISBA coupled with Crocus"

L597 I don't think the authors explicitly showed soil evaporation from the results. Please rephrase it.

L717-718 Please reword.