Anonymous Referee #2

The manuscript uses one new dataset over forests and peatlands situated in Northern and Southern Finland to evaluate different model configurations against surface energy fluxes, albedo, snow depth and soil temperatures. The authors emphasize the importance of testing these variables over organic layers and suggest which model configurations should or should not be used for some applications.

In its current form, not only is the manuscript too long, but it manages to be far too dense as well as too vague. There is not only too much information (e.g. there may "only" be 13 Figures in the main text and 3 in the appendices, but these 16 figures in fact include 86 windows altogether!), but also not enough detail where detail is needed. For example, many results seem to be relying on poorly understand parameters that are adjusted one way or another with little justification. I would not support the publication of this manuscript as it is, but I would strongly support it being split between a data paper and a modelling paper. This would not be "salami slicing"; the authors' express their intention for the data to be used in future modelling exercises and by other modelling groups. As such, the data can stand on their own. So can the modelling study which, despite needing more work and clarifications regarding the adjustments mentioned above, has the potential to convey important messages to the growing community of SURFEX (and other LSM) users on how not to misuse models.

We thank the Anonymous reviewer for the detailed and critical opinion on our study. We are glad that the Reviewer appreciates our model evaluation and sees this study as a potentially significant contribution to the LSM community. We also recognize the comment that the dataset itself has value for other modelling studies. Instead of a data paper, we have decided to publish the datasets and metadata to open access data repository.

As requested, we will attempt to reduce the number of figures/panels in the main manuscript. However, please note the comment of Reviewer 1 who specifically appreciates the completeness of the results and points out the high quality and information content of the Figures. Thus, we prefer to move some panels into Supplement rather than fully removing them. In respect to both reviewers, we have considered alternative ways to shorten the manuscript without deteriorating the quality of the content.

Alternative (1)
Our first and preferred alternative to shorten the main manuscript is as following:

- *As the LE flux was shown to have a minimal role in Fig. 4 we are going to remove the panels showing LE in Fig. 6. The current Fig. 6. will be provided in the Supplement.*
- *As the sensitivity of snow depth to SOC is not major, we are going to remove the height of snow panel in Fig. 8. However, in reply to a request from Reviewer 1, we will provide plots with HS sensitivity to SOC (both sites) in the Supplement. In Fig. 8 we are also going to reduce the presented soil depths to include only 5cm, 25cm and 70cm. Plots with other soil depths will be moved to the Supplement.*
- *The impact of the soil-vegetation representation can be demonstrated with fewer soil depth panels. Thus in Fig. 10, we are going to keep only N-FOR 5cm and S-FOR 5cm soil depths in the main manuscript. The current figure will be moved to the Supplement.*

This way we are going to reduce the number of panels in the main manuscript by 12. In addition, we will attempt to shorten the text when preparing the revised manuscript.

(Alternative 2)
The second alternative that we have considered is to move all scatterplots to the Appendices and to present these results as tables of performance metrics in the main manuscript. However, we believe that model behaviour is more difficult to understand from the metrics alone, and that this change would have a slightly deteriorating impact on the overall quality of the paper. For instance, the metrics alone do neither represent the scale nor the range of the fluxes.

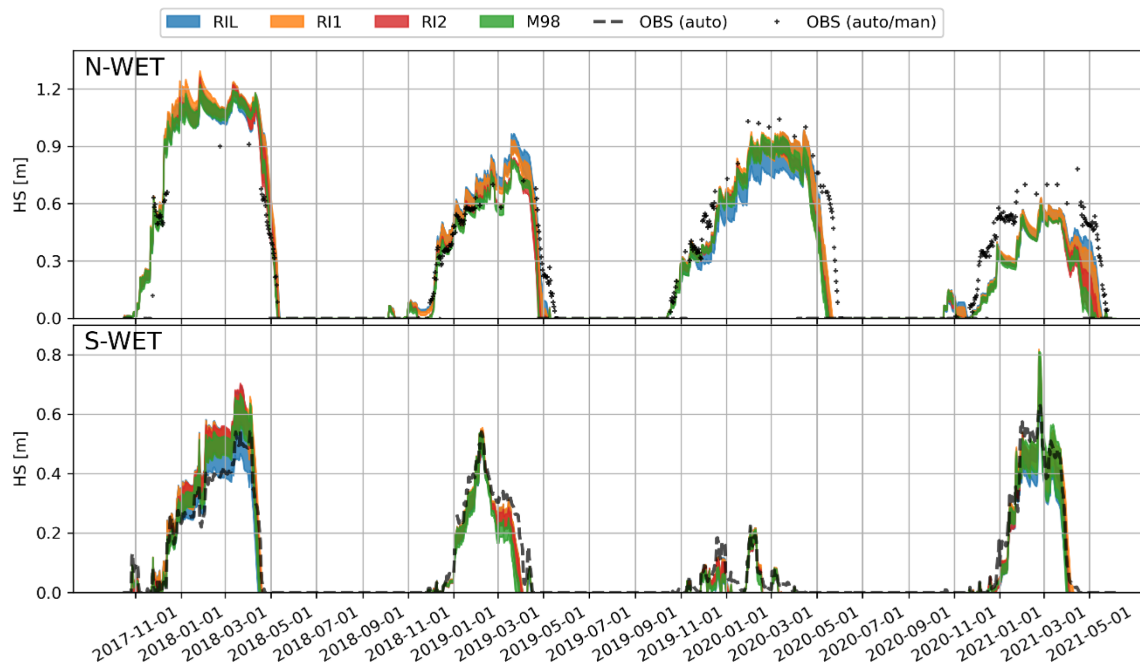
Therefore, we kindly ask the editor to choose between these two alternatives to best respect both the reviews.

We will also even more critically point out the 'free' parameters / poorly represented processes in the evaluated model combinations. We agree with the reviewer that the changes suggested below will improve the manuscript and increase its value for the SURFEX and LSM modelling communities.

We would like to point out and apologize for two mistakes found in the data processing during an additional quality check motivated by the reviews. Thus, we would like to thank the reviewers for boosting our data quality control process.

First, we found 2h discrepancy in time zones between different data sources, notably between the main observations used for forcing and the data used for gap-filling. We have converted all the forcing and evaluation data into UTC time zone and ran new simulations. We will update the figures to include the corrected simulations in the revised manuscript. These changes do not have any major impact on the results of the study.

The second error was in the wind forcing reference height used for the S-WET site. The S-WET wind reference height had been assigned as in the contiguous S-FOR site (16.8 m), whereas in reality wind was measured at 3 m height. This means that simulated wind speed near the surface of the low-vegetated S-WET site was unrealistically low in our simulations. We have found that this reduces the sensitivity of turbulent fluxes (mainly sensible heat exchange) simulations to different turbulent exchange parameterizations, as the site experiences stable conditions less often than previously simulated. This correction does not change the results showing that M98 turbulence option is superior to classic stability correction function (RIL). However, this correction does change the snow depth simulations on S-WET. Figure 5. changes in a way that all the turbulent exchange parameterizations succeed in simulating the snow melt (see the revised Fig. 5 below). Results related to these changes will be corrected in the revised manuscript.



Revised Figure 5. Time series of snow depths simulated by ISBA-ESCROC. The 35 ensemble members are grouped by their turbulent flux parameterization, and the spread of each group is presented in colored ranges. Observed snow depths are presented in black dots and dashed lines.

The major comments are separated into data and model:

Data:

- L311-312. How similar/different are the contiguous sites? And the meteorological stations? How much of the radiation data were missing? By R_g and R_A? How many timesteps were filled by ERA5? Ideally, either the timeseries of all meteorological observations or scatterplots showing how much these different sources differ when we do have overlapping data should be included. As this manuscript promotes a brand new dataset used for model evaluation, the gap filling in the dataset cannot be brushed off in two sentences.

We agree that there was not enough detail on the dataset. We are going to publish the datasets used for model forcing and evaluation (see also our answers to your comments about data publication later).

In the published model forcing metadata, we are going to add summary of how much each site variable was filled with different datasources. Citation to this dataset will be added to the revised manuscript.

The contiguous sites refer to the sites used in this study (e.g. contiguous site of N-FOR is N-WET and vice versa). Although they are different ecosystems, locations are close to each other and meteorological conditions are similar. The "other nearby meteorological station" used in gap-filling are farther; Sodankylä (ID 101932) is ~120 km from N-WET/N-FOR and Ähtäri (101520) is ~80 km from S-WET/S-FOR.

In case of N-WET, N-FOR and S-WET, ERA5 data in forcing is very minimal (less than 10 hours). S-FOR uses more ERA5 data due to lacking and discontinuous radiation observations. Specifically, the *downward longwave radiation observations were lacking in 2008 to 2010 and again in 2012. As S-FOR forcing data includes more ERA5 data than the other sites, we are going to include a comparison of ERA5 against site observations on S-FOR in the Appendice of the revised manuscript. Note that we did not focus on those years with ERA5 data in the manuscript, but they are still included in the scatterplots and performance metrics.*

We are going to revise lines L310-L311 as:
"The data gaps in meteorological observations were first filled by the contiguous sites (e.g. contiguous site of N-FOR is N-WET and vice versa) and the remaining gaps by other nearby meteorological stations (IDs: N-WET/N-FOR 101932, S-WET/S-FOR 101520)."

- What area does the footprint of the eddy covariance towers cover? Does the vegetation cover or topography vary within the footprint? May this have consequences on the measurements? These questions may have been answered in the two cited papers, but such information is needed here.

We agree it is imperative that eddy-covariance (EC) fluxes used for model evaluation are both representative of the system measured and meet the fundamental micrometeorological assumptions (i.e. turbulence stationarity and horizontal homogeneity of the source-sink function). In the data used here, these are ensured by the original authors of the datasets, who have conducted both standard Q/A routines and footprint analysis. For respective sites, these details have been reported in earlier publications and data descriptions (Aurela et al. 2015, Mammarella et al. 2016, Mammarella et al. 2019, Alekseychik et al. 2022). As the data quality is ensured, and follows the standards of the FluxNet community, we believe that as data users, we should be able to trust the data and rely on the uncertainty estimates provided by the data authors.

Considering that the main goal of our paper is a detailed model evaluation, we have decided to not include flux footprints and/or deeper insights of the data processing etc. in the current paper. The relevant information is available in the cited publications, and in the current manuscript we focus on the modelling part. In the revised manuscript we will further improve this part by pointing out the key papers and dataset descriptions relevant. Note also that uncertainties associated with EC measurements were considered and discussed in our manuscript based on the content of these papers.

- Much of the appendices could be transferred to a paper describing the data. Same for Section 4.1.
- L665-676. This could be added to a data paper.

As noted above, we prefer not to split the paper.

- L735-737. The dataset presented here will not be widely re-used unless it is published in a data repository and a separate manuscript details the data. In a research landscape where open access to datasets and models is required by many funders and is often a pre-requisite for publication (I am surprised it is not compulsory in TC), a sentence like "Data are available upon request from the authors" raises red flags. You have done all the work on the dataset; with FAIR guiding principles becoming the norm rather than the exception, not publishing it suggests that the data are perhaps not as solid as should be.

We thank the reviewer for this comment. As the sites we consider in this study belong to several international and national research infrastructures (e.g. ICOS; S-FOR, S-WET, N-FOR, N-WET), the *data used here is mostly already citable and openly available in databases. For instance, the data for S-FOR and S-WET can be accessed through https://smear.avaa.csc.fi/download under fair-use principles. The ownership of the data is by the original data provider organizations and groups. However, we fully agree with the reviewer that sharing the particular dataset used in this paper (including measured fluxes, state variables, site properties and model forcings) is likely to catalyst further use of the data in land-surface and hydrological model development, by saving time and effort of other modellers – and reducing potential mistakes in data processing/filtering/use. This is not least because compiling the dataset required specific insights into the observations, sites and vivid communication with the researchers responsible for the measurements. This is clearly the strength of the author team in this study. Therefore, we got permission to publish the dataset specific to this study, accompanied by relevant metadata descriptions, in open access data repository Fairdata.fi as an electronic supplement to this article with an assigned DOI for the dataset.*

Model

- As acknowledged by the authors, not only is w_sw important, but it is very poorly defined and, arguably, poorly understood. Table D1 clearly shows that w_sw is what I would call a "tweaking parameter"; it can range from 0.1 to 5 for no particular reason it seems. In addition, there does not seem to be any explanation given by the authors as to why different w_sw make so much difference in N-FOR but not in S-FOR. This needs to be explained, and preferably not in an Appendix (in fact, I am unclear as to what criteria were used to choose appendices over core text).

We agree with the reviewer that the parameter w_sw is, indeed, a tweaking parameter with very limited physical interpretation and seems not to be well related to any measurements. However, existing literature has never emphasized its high sensitivity on simulation results and its inconsistent settings among various applications. This is exactly what we pointed out in this paper and tried our best to explore the sensitivity of model results to the seemingly arbitrary choice of w_sw in the earlier studies. Please see also our answer to Reviewer 1.

The snow cover fraction, influenced by the w_sw parameter, affects the turbulent fluxes and the ground-snow heat flux. This approach commonly reproduces unrealistic snowpacks and soil thermal regimes in forested areas (Napoly et al. 2020) as the turbulence of the rough vegetation directly alters the soil-vegetation temperature which then have a cooling or warming impact on the snowpack. Although the w_sv parameter do make a difference on both sites, the sensitivity of w_sv parameter for the soil thermal regime is higher on N-FOR (see Fig 1. below). During some warm events on N-FOR, the higher snow cover fraction simulation (w_sv = 0.2) retains freezing soil temperatures while the soil of the lower snow cover fraction (w_sv = 5) simulation is no longer freezing, melting the snowpack. In the revised manuscript, we will discuss this differing feedback.

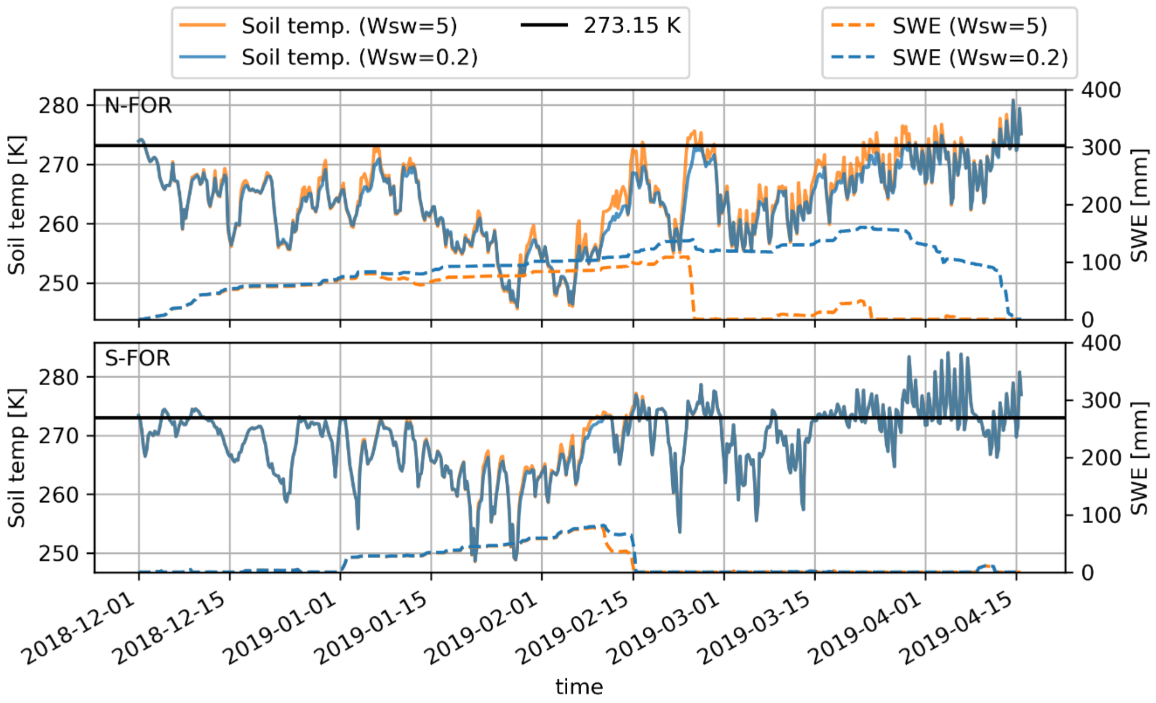


Figure 1. The sensitivity of w_sw parameter on 6-hour surface soil temperature simulations and snow water equivalent (SWE). Simulations are represented by one member of the ESCROC-ISBA VS as described in the paper.

We agree that many of the supporting results, now placed in Appendices, could have been placed in the main text. However, our study contains already a lot of figures, as the Reviewer has also pointed out. Our approach was to include supporting results as Appendices. For instance, Fig. B1 supports our discussion of snow in forests versus snow on peatlands while Fig. C1 is supporting our discussion about uncertainties of winter vs. summer energy fluxes (we obviously focused on winter in the main manuscript). When it comes to the results of sensitivity w_sw parameter sensitivity test, we believed it to be very useful information for the expert model users, but not essential for the main manuscript.

- One of the premises of this manuscript, with which I agree, is that peat and SOC are generally overlooked (l78-89) in snow modelling studies. L90: "The goal of this study is to evaluate the ability of SURFEX LSM (Surface Externalisée, Masson et al., 2013) to describe the surface energy balance and its drivers in boreal and subarctic peatlands and forests". Yet, l290, we learn that the authors will not reach their goal by using a dataset fit for purpose, but instead "assumed the top 1 m of the peatlands to consist 100 % of SOC". How can your whole study rely on an assumption?

Boreal and sub-arctic peatlands, as the fens in our study, have typically a rather deep (down to several meters) peat layer overlying the mineral soil or bedrock (Marttila et al. 2021). The peat consists purely of organic plant residues decomposed to different degree. For instance, Väliranta and Mathjissen (2021) measured mean loss on ignition value 96,6 % in the first meter of the peat profile at S-WET. Further, Muhic et al. (2023) also measured organic matter to be nearly 100% for the first 40 cm depth at a location just next to the N-WET peatland (see Muhic et al. 2023 Fig. 1b "FP2").

Thus, in this respect our assumption of '100% SOC' is realistic. We are going to revise the manuscript by adding these citations to justify the use of 100% organic matter in model parameterizations.

However, we recognize that particularly hydraulic characteristics are likely to vary across the peat profile and this variability is not detailed in the used model schemes. The thermal characteristics (heat capacity and conductivity) in the organic, saturated or near-saturated peat soils with porosity >0.8-0.9 are however dominantly determined by liquid water and ice contents.

- Table 2 suggests a fixed vegetation for ISBA, but nothing for MEB. How does MEB know how much of the grid box is vegetation and how much is vegetation air space? Does it change over time? What are the implications? Please clarify.

In MEB, the vegetation input includes vegetation type, LAI and canopy height. Vegetation properties used in the different canopy process parametrizations are derived from these inputs.

In the paper, L281-283: "Summer LAI and vegetation height were obtained from literature, while winter LAI was estimated according to the proportion of deciduous and coniferous vegetation on each site.". It is possible to provide monthly LAI and canopy height values to account for seasonal variability in foliage density. We have estimated the monthly LAI cycle between the LAImin and LAImax (Table 2.). Indeed, such estimation, together with the known challenges in in-situ LAI estimates brings uncertainties. We are going to publish the parameter-files (so called namelists) as electronic supplement of this article.

The 'vegetation fraction' parameter as in ISBA, does not exist in MEB. However, the part of the vegetation through which light passes without hitting the leaves is computed using a so-called sky view factor which depends on LAI and a vegetation dependant -constant (see Boone et al. 2017 Eq. 45). This means that the larger the LAI, the less light goes through (i.e. the vegetation air space is smaller).

We will revise the manuscript to be clearer in these regards.

- In Section 4.4., the authors acknowledge that the lack of internal water vapour causes errors in heat exchanges between the snow and the soil and therefore potentially affects modelled soil temperatures. Knowing this, can the authors demonstrate that accounting for (an assumed) SOC is not a way to compensate for errors in the soil thermal regime that are caused by other processes that are badly or not represented?

The implementation of organic content in the solving of thermal diffusion was done considering the known thermal properties of this material, without any specific calibration of parameters to reproduce soil temperature observations. This way, we can reasonably assume that the improvement obtained in soil temperature simulations is obtained for an appropriate physical reason. Obviously, errors and error compensations due to unresolved processes always remain in any numerical model and it is difficult to demonstrate their influence or absence of influence without implementing these unsolved processes, (and vapor transfer is an unsolved process in most state-of-the-art LSM). This intrinsic limitation of any numerical model will be mentioned in the revised manuscript. See also our response to reviewer 1.

- There are far too many plots that are not even referenced in the text. Then there are performance metrics in the plots. The more is not the better. Please reconsider whether you need 86 windows/plots in 16 figures and consider presenting your results in line with what you are highlighting in the text. For example, the different parts of the energy budget are important in different seasons, so why not have seasonal plots? You are asking the reader to compare seasonal plots (l516-517), it is your role to facilitate this if you believe this is important.

We thank the reviewer for pointing this out. Please see our answer earlier regarding our effort to reduce the information content of some figures. However, again, please note the comment of Reviewer 1 appreciates the completeness of the results and the high quality of the figures. Therefore, we also prefer to move some Figures in the Appendix and/or Supplement rather than removing information that we believe is necessary to fully understand the model behaviour for the most expert readers. Further, we believe the performance metrics and plots support each other. The performance metrics provide quantities to otherwise qualitative illustrations of the figures, important in understanding the model performance. See also the answer below regarding Figure 7.

We are going to revise the manuscript at L516-517 as:

"The relative uncertainties in simulated and observed energy fluxes are significantly greater in winter than in summer. Performance of the simulated summer energy fluxes is very good (see Appendix C Fig. C1). ~~(compare Fig. 13 vs. Appendix C Fig. C1).~~"

Minor comments:

The list of symbols and acronyms is huge and it makes it very hard to follow the manuscript, having to go back to previous pages to remember what is what. I would strongly advise the authors to make it easier for readers by having one or multiple tables describing the abbreviations, acronyms etc. It may also help the authors catch some that are not described (e.g. rho_sng).

We are going to add a Table describing the abbreviations and acronyms.

Abstract: I disagree that the model included a "realistic" soil texture; the SOC values were assumed, not "real". Please re-phrase.

Considering these peatland soils as 100% organic matter is indeed realistic as we have explained in the 'Model' comments.

L85: Incorrect reference. Krinner et al. (2018) do not provide any information at all about soil texture at ESM-SnowMIP sites; Menard et al. (2019) do.

Thank you, this will be corrected.

Section 3.3.4: The models neglect LSA increases due to intercepted snow, but does intercepted snow sublimates? I could not find the answer in the manuscript even though a large percentage of snowfall is known to sublimate in coniferous forests (see Essery and Pomeroy, 2001 http://www.merrittnet.org/Papers/Essery_Pomeroy_2001.pdf for references).

This is a good point, and yes, intercepted snow can sublimate in MEB. This will be mentioned in the revised manuscript.

Eq 9: Given how important snow density (rho) is to the calculation of the snow effective thermal conductivity, it would be helpful to know how rho is calculated.

You are right, snow layer densities are the main driver of snow thermal conductivity. However, it is not possible to provide a comprehensive description of all snow processes in this paper focused on *model application and evaluation. To not make the manuscript longer, we are going to add detailed citations to the model papers and equations numbers describing falling snow density, compaction, and associated interacting processes.*

Figure 1: What is the point of the top plot? We can hardly see where the sites are located in Finland, which would be more interesting than knowing where the boreal land biome is in the whole world. Also, could you please indicate the scale of the aerial images? Do they cover the EC tower footprint? If the scale is larger than the footprint, then, again, what is the point? The images should be proportional to how the sites are used. The manuscript presents site simulations, therefore we should have an idea of what the sites looks like. Otherwise, scrap Fig 1 altogether and simply present the sites as the parameters that represent them in the model.

The goal of the top plot is to contextualize our study sites inside the boreal land biome and to illustrate the extent of this biome around the Northern hemisphere. We have revised Fig. 1 to include site locations inside Finland and the aerial images to include scale bar (see Fig. 3 at the end of this document). In addition, we are going to add site photos to the Supplement (see Fig. 2 at the end of the document)

Fig. 3: Add "Observed" at the start of the caption. Also, G is hardly visible

Thanks for pointing out, will be corrected. G is hardly visible because its magnitude is very low compared to other fluxes. This will be highlighted in the revised manuscript.

Fig 7 and others: Do we really need scatterplots, qqplots and timeseries? They are not all referenced in the text. If you want to use them all, please explain why, but I would advocate choosing.

We believe that the use of time series plots and scatterplots support each other: time series qualitatively illustrate part of the simulations (when and what are the errors) while scatterplots gather all the data points for the full simulation period (whether or not time series patterns are consistent or relevant for the full simulation). Qqplots clearly identify the mean biases for the full obs-mod sorted distribution. We understand that it may take a while for reader to process all this information, but we do not think this would be a valid reason to remove some of them. Indeed, the impact on snow cover simulations can be very different between systematic errors and time-varying errors of energy fluxes.

Fig 7: Why did you choose that specific year for the timeseries?

This winter represents typical snow conditions for both sites, and good availability of upwards radiation observations. This will be mentioned in the revised manuscript.

L442: Same as in the abstract. I disagree that the model included a "realistic" soil profile; the SOC values were assumed, not "real".

See our answer in 'Model' comments.

L493: "the summer energy fluxes were majorly improved by simply assigning the vegetation fraction to unity". Is there a legitimate reason to assign the vegetation fraction to 1, or is it a tweak to "improve" the energy fluxes albeit for the wrong reasons?

You are right, this is a way to tweak the ISBA composite approach, and it was done by e.g. Vernay et al. 2022. Vegetation fraction is commonly applied as 0.95 in ISBA, which is by no means physical either. The benefit of an accurate vegetation fraction depends on the way interactions between *vegetated and non-vegetated parts are represented. However, assessing this was beyond the scope of this study.*

Our point is that such composite approach is only very lightly linked to physical relationship between soil and vegetation. We presented alternative ways to optimally apply such model to these four environments.

L566: Do you mean Menard et al (2021)?

Yes, we apologize for this incorrect reference which will be corrected in the revised manuscript

L584-592: This is a very important paragraph on how not to misuse or repurpose models. Splitting this manuscript into one data description and one model simulations paper would prevent such an important message from being buried deep under too much information. I would also like to see this message somewhere in the abstract.

We thank the reviewer for the importance he/she has identified in this paragraph. We have explained earlier in this response why we prefer to not split the paper in two parts, but indeed we will insist on that point in the conclusions and abstract to better emphasize this message.

Sections 4.3.1 + 4.3.2.: What I called "tweaking", the authors call "compromise". These sections are very honest about how some of the results were "improved" and are, in my opinion, the best in the manuscript. Would the authors consider be this transparent earlier in their manuscript?

Thanks for the comment; we agree the importance of identifying and openly communicating the critical 'tweaked' / poorly constrained parameters in LSM's. Throughout the revised manuscript, we plan to be even more transparent and clear on such inadequately described processes, and parameters that cannot be readily derived from observations / available data. These include, for instance, the alternative formulations for turbulent exchange in stable conditions (Sect. 2.3.2, Figs. 4, 5, 6). We believe being more critical to the model schemes, as implicitly suggested by the reviewer, will further improve the manuscript and make it more useful contribution to SURFEX, snow and LSM model communities.

Figure 12: This is very confusing. The legend convention is the opposite of Fig 4. where solid colours are the model, dashed are observations... Please be consistent.

As requested, we are going to change the dashed lines to solid.

L682-683: Does this mean that you may have achieved "satisfactory model performance" for the wrong reasons i.e. because one badly represented process compensates for another not represented at all (e.g. overestimating snow density may cause snow depth to be as low as if the model had accounted for lateral snow transport).

With the available processes implemented in the model, it is again not possible to be affirmative on error compensations. Here, the parameterization of wind impact on snow density is designed to replace the fact that snow transport is not explicitly simulated. Regarding snow density, error compensation with falling snow density, and mechanical snow compaction are obviously possible but difficult to identify without more detailed observations. Regarding snow height, error compensation between snow density and snow mass are indeed also possible, with a part of snow mass errors that can be explained by the absence of erosion/accumulation in the model. Our goal in our discussion is *to help the reader to easily identify the unresolved processes we can imagine having significant impacts on our results, and to be aware of most possible error compensations.*

Despite these limitations, we believe that it is also fair to present results as satisfactory when the magnitude of snow height and energy fluxes is realistic, even if not always for the good reasons, because realistic estimates of snow extent and energy fluxes is an important criteria to have realistic boundary conditions in NWP systems and GCM.

L699-701. Information about the footprint of the EC tower should be given earlier in the manuscript.

Please see our response to the general comments earlier.

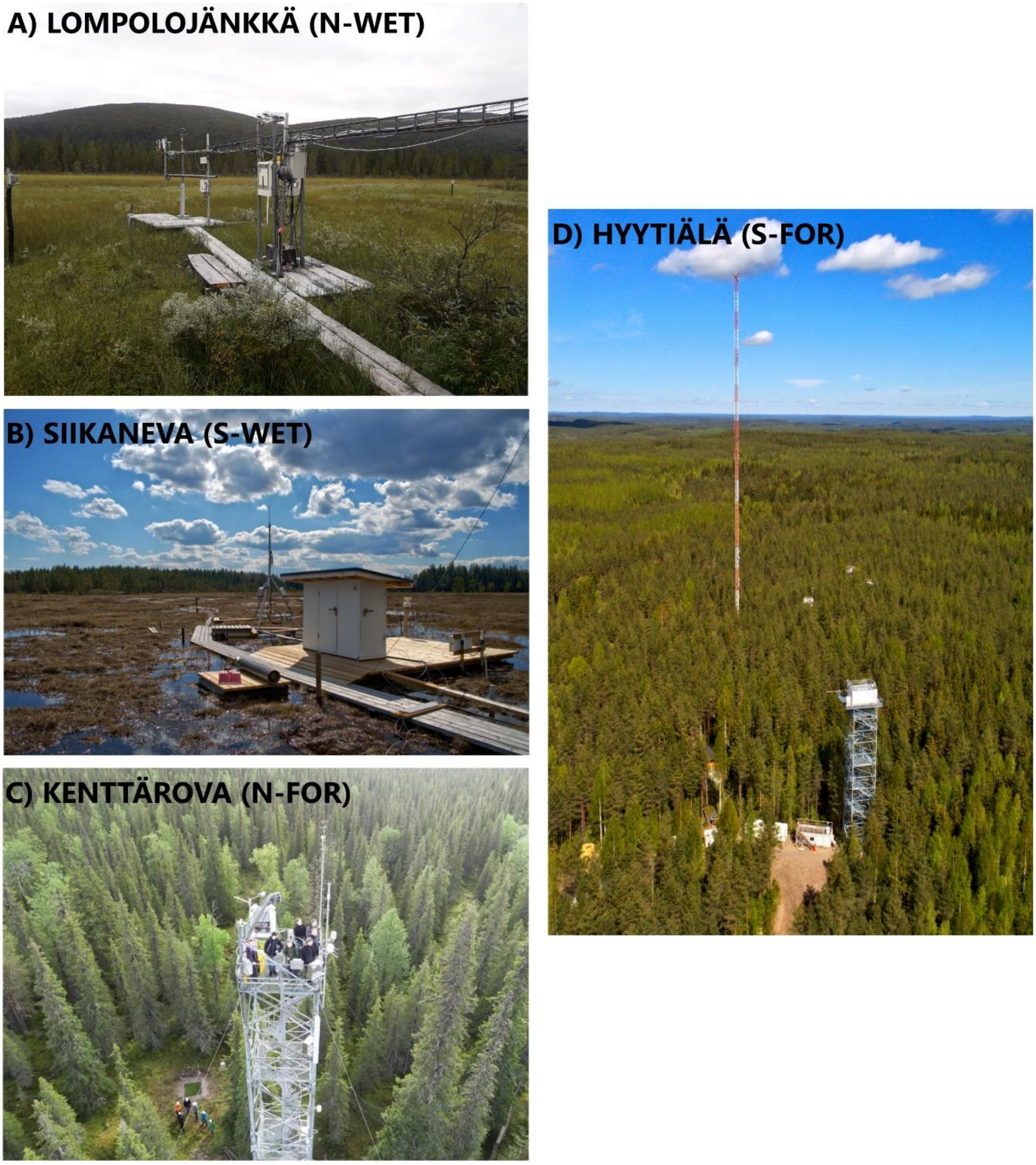


Figure 2. Study site pictures (Lompolojänkkä (Pertti Ala-Aho), Siikaneva (Alekseychik et al. 2022), Kenttärova (Bastian Steinhoff-Knopp), Hyytiälä (Kolari et al. 2022)).

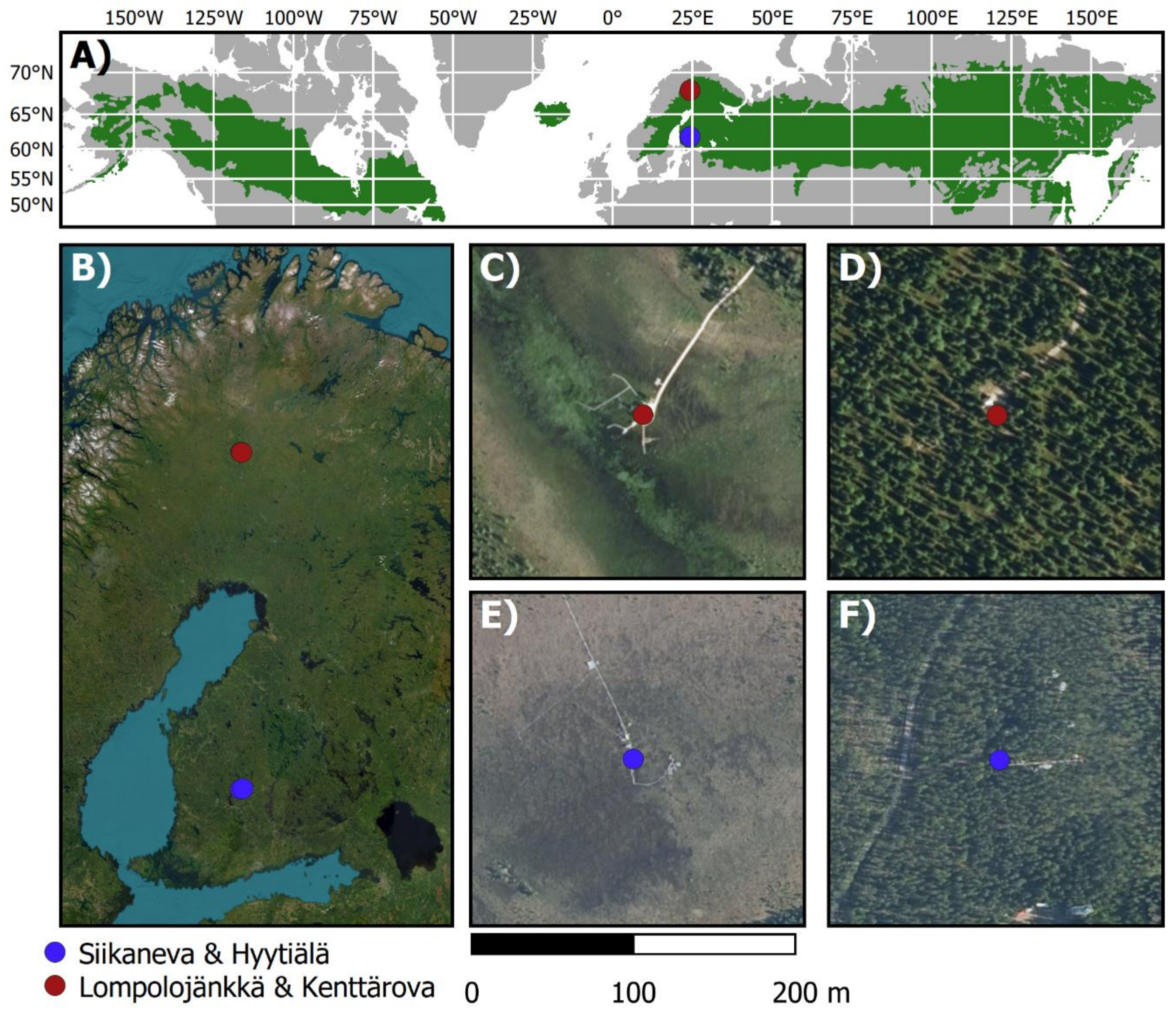


Figure 3. A) Study area locations inside the boreal land biome (green area, Olson et al., 2001), B) closer study sites locations in Finland (Source: Esri), and aerial images of each site: C) Lompolojänkkä (N-WET), D) Kenttärova (N-FOR), E) Siikaneva (S-WET) and F) Hyytiälä (S-FOR) (NLSF, 2020).

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