We thank the reviewer for taking the time to read through our paper, for their detailed review and for their insightful comments. Please find our replies below as inserted in red text.

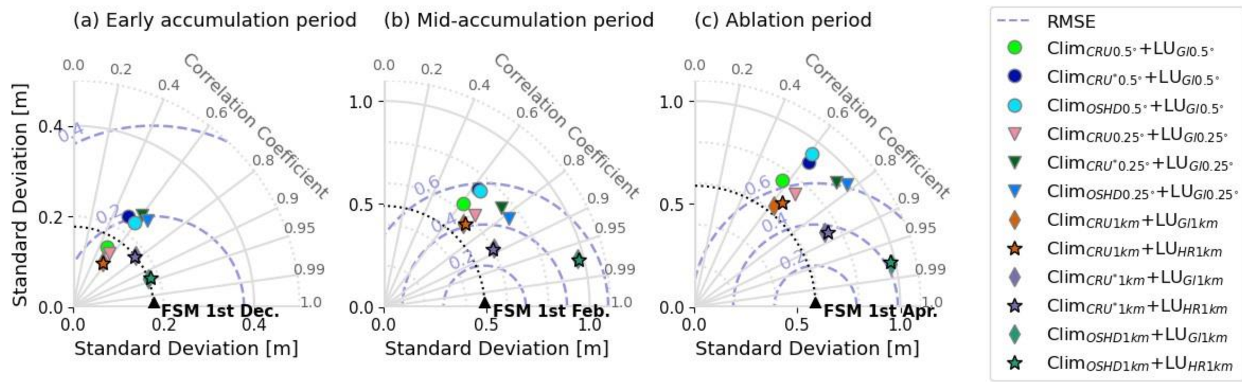
Although land surface modeling has evolved from simple biophysical parameterizations to complex frameworks in recent years, large uncertainties remain especially in mountainous regions and areas with complex terrain. This study uses a multi-resolution modeling setup to investigate the impact of meteorological forcing data, spatial resolution, and land surface data on the simulation of snow cover and ecophysiological variables. The authors perform simulations using the Community Land Model version 5 (CLM5) over Switzerland. They found that increased resolution not only improved the representation of snow cover in CLM5 but also propagated through the model and affecting the gross primary productivity (GPP) and evapotranspiration (ET).
Overall the manuscript is well written and of interest to the land surface/earth system modeling community. However, the CLM5 model setup and the model evaluation method do not appear to be appropriate in the current manuscript. In specific, (i) the CLM5 is setup in prescribed satellite phenology mode (~ fixed growing season), yet one of the main focus of the study is to investigate the link between snow cover duration and growing season length (and GPP/ET); (ii) CLM5 simulations in the study were conducted at three resolutions: 1km, 0.25deg, and 0.5deg, but the evaluations were performed at 1km resolution (Figure 3), which are not fair comparisons in my opinion.
I suggest the authors choose the prognostic biogeochemistry mode for CLM5 simulations, and perform model evaluation at the resolutions of the respective simulations. In addition, I have some minor suggestions outlined in the comments below that will hopefully improve the future version of the paper.

Thank you for this comment.

Regarding (i): Initially, we opted to use CLM5 with prescribed vegetation phenology to represent the vegetation of Switzerland today in the most accurate manner, which would be very difficult to achieve when running in prognostic biogeochemical mode, at least without data assimilation. We also did/do not have the computational resources to perform CLM5 simulations at high resolution in bgc mode, especially not the required spin-up. We acknowledge the problems arising from this setup when focusing on the link between snow cover and growing season length and have decided to reframe the paper following a valid suggestion from Reviewer 1. The revised manuscript will be more focused on input data, resolution, and snow, while removing in-depth discussions of links of all that on GPP/ET estimates. For this purpose, we will move Figure A1 from the appendix to the main paper, and further include more detailed performance assessments of the various CLM5 model setups along elevational bands.

Regarding (ii): This is a very valid point. We have redone evaluations of gridded snow simulations at 0.25°, as we believe that given the complexity of the topography across our modelling domain and its relatively small size, and considering today's ever-increasing computational resources, 0.25° should be a fair target for the main analysis.
As seen from the updated version of the Taylor's diagram below (Figure 3 in original manuscript), the difference between different land-use datasets with regards to simulated snow depth remains small and increasing spatial resolution in isolation only has a marginal effect on accuracy of simulated seasonal snow cover. Upscaled 1km simulations with highest quality meteorological forcing datasets ($Clim_{OSHD1km}$) perform best during the early accumulation period

but performance is matched/exceeded by the lapse-rate corrected global dataset ($Clim_{CRU*1km}$) for mid-accumulation and ablation period, underlining the effect of a relatively simple lapse-rate based downscaled temperature input to better account for sub grid variability. We will discuss these results in greater detail in an updated version of the manuscript.



Specific comments

L164, the sentence does not read well. We will reformulate this sentence in a revised version of the manuscript.

L169-170, do you assume values in the original dataset (ClimCRU1km) are at sea level, and apply the temperature lapse rate based on the mean elevation of each 1km grid from the CRUJRA data? Which elevation data do you use? I would suggest include a map of elevations in Figure 1. We do not assume that the values in the original dataset are at sea level. Rather, we use a global DEM at 0.5deg to first bring temperature to sea-level temperatures by applying negative lapse rates. We then use a high-resolution DEM of Switzerland at 1km to re-lapse temperature. We will add this information to the methods section of a revised manuscript. We will also include a map of both the high and low resolution DEM to the appendix of a revised version of this manuscript.

L172, it would be helpful to add a description of the snow/rain partitioning method in CLM5. CLM5 partitions total precipitation into rain and snow according to a linear temperature ramp, resulting in all snow below 0 °C, all rain above 2 °C, and a mix of rain and snow for intermediate temperatures. We will include this information to the methods section of a revised manuscript.

L181-182, do you just aggregate the 1km data to 0.25deg and 0.5deg? Please try to describe what exactly is being done. The ClimOSHD forcing data would be useful for other modelers, is the data available?
Yes, here the 1km data was upscaled to 0.25 and 0.5deg using a conservative regridding approach. Thanks for your note, we will consider publishing the $Clim_{OSHD}$ forcing data, but certainly as a separate data paper given the enormous effort required to prepare such datasets Simulation results and updated surface data from this study will be made available as part of this paper.

L234, given that the met forcing and landcover data etc. are all at coarser resolutions, it is not fair to evaluate coarser resolution (0.25deg, 0.5deg) CLM5 simulations using finer resolution (1km)

observations. I suggest the authors regridding the 1km observation data to the 0.25deg and 0.5deg first, then redo the comparisons and Figure 3.
As mentioned in our comment above, we have followed your advice and redone the evaluation at 0.25deg, which we believe is a fair target.

L241-245, it would be helpful to show or discuss which variables in the met forcing data contribute to the different CLM5 simulations.
We will include a section in a revised version of the manuscript where we discuss and show differences in meteorological forcing variables between the various datasets.

Figure 3 is an important figure in the paper, but the Taylor plots and labels/legend are too small, and hard to read.
Thanks for pointing this out, we will improve the quality of the Taylor plots.

In the captions of all the figures, a summary of main results is also included, which is not necessary and makes the captions too long.
We will refrain from giving a summary of our results in the figure captions.

L256, supplementary material is not found.
This was a mistake and should read Figure A1, thanks for spotting it. Figure A1 will be moved to the main manuscript in a revised version of the manuscript.

L265, Figure 3 needs to be cited here.
Yes, we will cite Figure 3 here.

L273-275, I suggest the authors redo these evaluations at the resolutions used for each CLM5 simulations.
We have redone all evaluations at 0.25deg, which we believe is a fair target for our model analysis.

L290-291, the sentence doesn't read well.
We will reformulate this sentence.

Figure 4, note the 3rd panel are labeled as effect of climatological forcing instead of meteorological forcing.
Thanks for catching this, we will update the label.

L316-317, the sentence does not read well.
We will omit this section from a revised version of the manuscript.