Reviewer 1

General comments

This manuscript uses an existing parameterization for sub-grid topographical effects on solar surface irradiance to show how accounting for sub-grid topography, instead of assuming a flat surface, affects the albedo, energy balance and temperature of the surface. The authors find that accounting for sub-grid topography can have large effects on the land surface model, depending on season, elevation and grid resolution. Furthermore, the authors use MODIS data to evaluate this newly implemented parameterization and find that accounting for sub-grid scale topography generally improves the representation of the surface.

The paper is interesting within the scope of the journal. However, I think the presentation of the manuscript can be improved. First of all, I would suggest to switch the order of sections 3.1 and 3.2, e.g. start with the effects of sub-grid topography on the land surface and then show to what extent this improves the results with respect to MODIS. Furthermore, I suggest to strongly reduce the number of figures in the manuscript. Figures 2-7 and 9-14 fill up a lot of space and are a bit repetitive. It certainly helps the reader to keep a few maps, but I would suggest to use a more concise visualization (e.g. such as in Figure 8) of the data.

I recommended major revisions based on the main comments above and the specific comments and technical corrections listed below.

Thank you very much for your useful suggestions/comments. As suggested, we reorganized the order of the sections and in the revised manuscript, the results sections include **3.1 Sub-grid topographic effects on surface energy budget, surface temperature and snow cover/depth, 3.2 Contributions of different factors, 3.3 Sensitivity to elevations, 3.4 Sensitivity to spatial scales, and 3.5 Comparison with MODIS data**. We also reduced the number of figures by using more concise figures to show the results clearly in the revised manuscript. A detailed response to your specific comments is provided below.

Specific comments

1. p. 2, ln 66-68: Mention clearly where your evaluation and analysis of the sub-grid scale topography effects differs from Lee et al. (2019), besides the use of a different land surface model.

Lee et al. (2019) focused on winter at a horizontal spatial resolution of 0.9° × 1.25°, while our study analyzed the topographic effects at various spatial scales from high resolution (0.15°) to coarser resolution (about 2°), for all four seasons. Lee et al. (2019) compared the surface downward solar flux, surface upward solar flux, surface net solar flux, surface albedo, and air temperature of the model simulation with those of the CERES observations and CMIP5 models, while our study focused on land surface processes and comparison of high-resolution MODIS data and model simulations for direct albedo, diffuse albedo, snow cover, surface daytime/nighttime temperature, and latent heat flux. In addition, we also analyzed the sensitivity to elevation and the contributions of different factors. We clarified these in Line 69-72 of the revised manuscript.

2. p. 3, ln 85-87: State more clearly that you use this parametrization to investigate for different seasons how the large the effects of using a sub-grid scale topography parametrization on solar irradiance are. And that both the simulation with and without this parametrization are compared to MODIS to investigate to what extent using this parametrization improves the land surface.

We stated these more clearly as suggested in Line 89-94 of the revised manuscript as below: The sub-grid topographic effects on surface energy balance, snow cover/depth and surface temperature were investigated based on the ELM simulations. The contribution of different factors to the sub-grid topographic effects and the dependence of the sub-grid topographic effects on seasons, elevations and spatial scales were also analyzed. A suite of remotely sensed data from the Moderate Resolution Imaging Spectroradiometer (MODIS) were used to compare with the ELM simulations with different parameterizations for solar radiation in different seasons.

3. p. 4, ln. 99. "multi-scattered radiation": what is this term exactly, isn't it also part of the diffuse irradiance?

We replaced it with 'coupled radiation that represents surface reflected radiation that is further reflected or scattered by atmospheric particles', in Line 107-108 of the revised manuscript. The incoming solar radiation for a flat surface is composed of direct radiation (F_{dir}^{PP}) from sun, diffuse radiation (F_{dif}^{PP}) from sky, and coupled radiation (F_{couple}^{PP}). ELM-v1.0 assumes flat surfaces and accounts for F_{dir}^{PP} and F_{dif}^{PP}, while neglecting F_{couple}^{PP}.

4. p. 5, ln. 151: "TP, known as the Third Pole" may suggest TP is the abbreviation of Third Pole, which is confusing. Suggestion: "The Tibetan Plateau (TP), also known as the Third Pole,"

We revised the manuscript as suggested in Line 170.

5. p. 5, ln 161: I suppose that with "offline" you mean that ELM is not coupled to the atmospheric/oceanic model? If so, how do you derive the radiative fluxes at the top of the canopy, i.e. what is the atmospheric input for these RT calculations and are these similar for TOP and PP?

Yes, we only ran the ELM model and will do the coupling run in the future study. The 3-hourly Global Soil Wetness Project meteorological forcing data set version 1 (GSWP3v1) (Dirmeyer et al., 2006; Yoshimura and Kanamitsu, 2013) with 0.5°×0.5° spatial resolution was used to drive all the model simulations. The bilinear interpolation techniques were used to downscale the GSWP3v1 data into the required spatial resolution, and the coszen (i.e., the cosine of the solar zenith angle)-based, nearest neighbor, and linear interpolation methods were used to downscale the solar, precipitation and other data to the half-hourly temporal resolution, respectively. We clarified these in Line 180-190 of the revised manuscript.

6. p. 6, ln. 188: What is the reason for these two times (10:30, 22:30)?

The MODIS sensor onboard the Terra satellite has an overpass time of 10:30 am (local solar time) and 10:30 pm (local solar time), respectively. Thus, we selected those two times for model evaluation. We have revised the manuscript accordingly in Line 235 to include

the MODIS instantaneous surface temperature data was derived for daytime and nighttime corresponding to the MODIS overpass time: 10:30 and 22:30 (local solar time), respectively.

7. p. 6, ln. 181: "Relative difference" suggests something like (TOP-MODIS)/MODIS and (PPMODIS)/MODIS, whereas the computed quantity seems to be the change in the bias with respect to MODIS between TOP and PP.

We revised the manuscript as suggested in Line 238.

8. p. 6, ln. 193-195: How is the evolution of the snow cover determined in ELM? Besides snow cover, wouldn't snow depth be interesting as well?

As suggested, we added the related introduction about the snow processes in ELM in Line 97-101 as below:
ELM (Version 1.0) is based on the Community Land Model Version 4.5 (CLM4.5) (Golaz et al., 2019). ELM calculates canopy radiation flux using the two-stream approximation methods, snow albedo using the Snow, Ice, and Aerosol Radiative Model (SNICAR) model (Flanner et al., 2007), and snow cover fraction based on snow water equivalent (Swenson and Lawrence, 2012). ELM also represents the snow hydrological processes including snowfall accumulation, melting, refreezing, compaction, aging, water transfer across layers, etc.

We also added the comparison of snow depth in Line 255-256 of the revised manuscript. In ELM, snow depth has a positive correlation to snow cover fraction which is calculated based on snow water equivalent (Bonan et al., 2019). The results show that larger net solar radiation over mountainous regions increases sensible and latent heat fluxes and decreases the snow cover fractions and snow depth due to increased snow melt and possibly snow-albedo feedback (Figure

4), which may alleviate the snow depth overestimation over the TP in ESMs (Wei and Dong, 2015).

Wei, Z. and Dong, W.: Assessment of Simulations of Snow Depth in the Qinghai-Tibetan Plateau Using CMIP5 Multi-Models, Arctic, Antarctic, and Alpine Research, 47, 611-625, 2015.

Bonan, G., 2019. Climate change and terrestrial ecosystem modeling. Cambridge University Press.

9. p. 6, ln. 196: How valid is that assumption, given that you have seasonal changes in vegetation and snow cover as well as snow cover differences between the simulation.?

In ELM, snow emissivity is fixed as 0.97, soil emissivity is fixed as 0.96, and vegetation emissivity is around 0.98 which depends on the specific leaf/stem area index. For a grid with different land cover types, the assumption that the grid emissivity is 1.0 is reasonable and is also used in the land-atmosphere interaction of E3SM. In addition, sub-grid variability of emissivity is not considered in the sub-grid topographic parameterization for solar radiation and we only aim to compare the differences between TOP and PP rather than the absolute accuracy of the calculated surface temperature.

10. p. 7, ln 205: Is all data used to construct the random forests?

Yes. Combined with the driving variables, all the ELM-derived seasonally-averaged data was used to train the random forest model and measure the relative importance of different factors in controlling the sub-grid topographic effects. We clarified these in Line 211-212 of the revised manuscript.

11. p. 7, ln 220-221: Can you explain why diffuse albedo is worse with sub-grid topographic effects?

We provided some possible reasons to explain the inconsistencies between ELM simulations and MODIS data, especially for diffuse albedo and nighttime surface temperature in Line 422-454 of the revised manuscript as below:

1) **For the ELM model:** The inclusion of sub-grid topographic parameterizations for solar radiation in ELM improves the representations of surface energy balance to some degree, but many shortcomings in ELM's existing radiative transfer modeling scheme limit the potential for further improving the ELM simulations. The 1D two stream approximation method used in ELM represents the vegetation canopy as a homogeneous "big leaf" (Yuan et al., 2017) and neglects the vertical multi-layer structure (Bonan et al., 2018) and the horizontal leaf clumping (Bailey et al., 2020; Braghiere et al., 2020; Li et al., 2019a). In the snow-covered regions, the ELM parameterizations for the effects of snow impurities (i.e., black carbon and dust mixing) on light scattering and absorption processes need to be refined to account for internal mixing and non-spherical shapes of snow grains (Dang et al., 2019; He et al., 2018).
2) **For the remote sensing data:** MODIS data also has some uncertainties related to the retrieval algorithm over rugged terrain, sensor calibration, atmospheric correction, etc. Uncertainties of remote sensing data may affect their reliability as ground truth for evaluating the ELM simulations. The MODIS land surface albedo products have shown good consistencies with ground measurements (Moustafa et al., 2017; Wang, 2004), but the semi-empirical kernel-driven-model-based algorithms used to derive the MODIS land surface albedo do not account for topography explicitly (Schaaf et al., 2002; Hao et al., 2020), which leads to large errors over rugged terrain (Hao et al., 2018a, 2018b). MODIS snow cover data has shown relatively poor performance when compared to ground measurements, especially over the regions of TP with higher elevation and shallower snow depth (Pu et al., 2007; Yang et al., 2015; Zhang et al., 2019). The accuracy of MODIS surface temperature products depends on the accuracy of land cover products and the prescribed surface emissivity values (Duan et al., 2019). The MODIS evapotranspiration product is sensitive to the algorithm used to account for the environmental stresses over the TP, as well as, the atmospheric forcing data used to generate the product (Li et al., 2019b).

12. p. 7, ln. 227 & ln 232 (and a few other locations in the manuscript): What does significant mean here?

We replaced this somewhat subjective word as ‘small’ or ‘large’ throughout the revised manuscript to make it clearer.

13. p. 7, ln. 231: Looking at figure 6, the difference between TOP and PP in some regions actually does not seem to be very small in winter and spring.

We rephrased this sentence as “the difference between TOP and PP is small in summer and autumn but large in winter and spring” in Line 364-365. In addition, we also give the possible explanation in the discussion part (Please also see our responses to Questions #11).

14. p. 8. ln 242: Does significant mean statistically significant or that the bias difference is small compared to the mean surface temperature? Also, the fact that the nighttime surface temperature in the upper two elevation bands performs worse with TOP for most of the region is interesting and worth mentioning. Do you have an explanation why it may be worse?

We replaced “significant” as “small” or “large” throughout the revised manuscript to make it clearer.

We mentioned these results in Line 378 as: “For nighttime surface temperature, the difference in biases increases with elevation”. In addition, the related processes in ELM still have some uncertainties (see our responses to Question #11). These may partly explain the inconsistencies between ELM simulations and MODIS data, especially for diffuse albedo and nighttime surface temperature (Figure 10).

15. p. 10, ln. 272-273: “, while the” You compare a difference in W/m2 to a relative difference (%), which can be confusing.

We revised the manuscript for clarity in Line 244.

16. p. 10, ln. 279-280: Can you elaborate on this? Does it mean that the standard deviation of elevation can explain most of the sub-grid scale topography effects on the albedo?

Here we want to show that the sub-grid topographic heterogeneity is related to the spatial patterns of the difference between TOP and PP. We revised this in Line 250-252 as "the spatial pattern of the difference in land surface albedo between TOP and PP is similar to the heterogeneous spatial pattern of topography (Figure 2)".

17. p. 10. ln. 282-283: "the absolute .. and -20%" The units and signs are a bit confusing here. First of all, I suggest to express snow cover fraction as a number between 0 and 1 instead of a percentage, to make the distinction between absolute and relative difference more clear. Also, "larger than" can be confusing because the difference is negative. You could rephrase as "the decrease in .... can be larger than ..."

As suggested, we expressed the snow cover fraction as a number between 0 and 1 instead of a percentage, and revised the manuscript to avoid the confusion in Line 255.

18. p. 11, ln. 296: "in winter", how are the elevation-dependent patterns in the other seasons compared to the winter?

As suggested, we analyzed the elevation-dependent patterns in four seasons, taking the land surface albedo as an example and included an additional figure in the manuscript (Figure 6). The results show that these elevation-dependent patterns are similar for all seasons, although the differences between TOP and PP are larger in winter than in summer.

19. p. 11, ln. 304: Shouldn't "relative differences" be "absolute differences", given the ±0.1? Also, it is not really clear to me what the exception ("except") described in this part of the sentences exactly is.

We revised “0.1” as “10%” to represent the relative difference, and replaced “except that” with “and” to avoid the confusion in Line 315-316.

20. p. 11, ln. 308: “sensitivity analysis” can you elaborate on this (e.g what kind of analysis)?

We replaced “sensitivity analysis” with “variable importance analysis” in Line 286 to clarify that here we aimed to analyze the contributions of different factors to the differences between TOP and PP.

21. p. 16, ln. 365: since the relative difference in snow cover fraction is negative (-20%), “maximum value” can be somewhat confusing for this quantity.

We deleted this expression in the revised manuscript. Please see the responses to Question #17 for our revisions.

22. P. 16, Figure 17 (and Figures S2-S6): the location of the red line (r0125) is important because it is serves as the ‘reference’, but the line is sometimes hard to see.

We have adjusted the order of different lines in these figures in the revised manuscript, to make the ‘reference’ line to be seen easily.

23. p. 17, ln.398-408 & sections 2.4/2.5: Are you able to compare typical errors estimates of MODIS with the differences between TOP and PP?

As suggested, we have compared the typical errors of MODIS data with the differences between TOP and PP in Line 431-440 of the revised manuscript, as below:

However, the topography-induced differences between TOP and PP can be comparable to the errors of MODIS data. For example, Wang et al. (2004) reported that compared to ground measurements, MODIS albedo had a maximum error of 0.036 in a semidesert region on the TP, which is smaller than the maximum difference of 0.1 between TOP and PP (Figure 4). Wang et al. (2007) showed that the mean and maximum errors of MODIS surface temperature were 0.27 K and 2.61 K, respectively at a semi-desert site on the western TP, which is comparable to the maximum difference of 1 K between TOP and PP (Figure 4). Salomonson and Appel (2004) showed that using the Landsat 30 m observations as the benchmark, the mean error of MODIS snow cover fraction was smaller than 0.1, which is comparable to the difference of 0.1 between TOP and PP (Figure 4). Mu et al. (2007) showed that the 8-day MODIS latent heat flux had a mean bias from -5.8 to 39.9 W/m^2, possibly larger than the difference between TOP and PP in our study (Figure 4).

Technical corrections

24. p. 7, ln 205: calculations -> calculation
25. p. 7, ln 221; p. 10, ln 284; p. 10, ln 285 and more: “difference(s) of” -> “difference(s) in”
26. p. 7, ln 222: : “regions”->”region”, or perhaps: “the whole rectangular regions” -> “the whole domain”
27. p. 10, ln. 277 & ln. 289: “different seasons” -> “all seasons”?
28. p. 10, ln.291: this 20% has already been mentioned in line 289.
29. p. 11, ln. 297: “but” -> “therefore”?
30. p. 11, ln. 300: “With the increase of elevation bands” -> “At higher elevations”
31. p. 11, ln. 301: “to larger” -> “to a larger”
32. p. 16, ln. 357: Figure S7 is referenced before figures S2-S6.
33. p. 17, ln. 379 & ln. 383: Why are “larger” and “and thinner atmosphere” in bold?
34. p. 18, ln 427: “the study” -> “this study”
35. p. 18, ln 432: “on solar radiation” -> “for solar radiation”

We have revised the above sentences carefully as suggested and checked/revised the grammar/typo issues throughout the revised manuscript.