Comments to the author:

The reviewers are clearly very enthusiastic about the manuscript. GMD has a potential audience with broad interests and I have a couple of comments to make as a generalist climate scientist who is not an expert in snow/albedo modelling.

We appreciate your comments and suggestions, and we have revised the manuscript accordingly.

I am a little disappointed that while implementing new features into an ESM, you only test the results on one part of the globe. It seems very dangerous. Can you comment on this? Have you looked at other regions? Does it all go horribly wrong, or did you choose the Tibetan plateau because actually it highlights the differences better?

We selected the Tibetan Plateau (TP) as a testbed in the study, based on several reasons:

1) As a hotspot region under climate change, TP is characterized by complex topography and frequent snow cover. TP is also characterized by a thin snowpack, making its albedo effects particularly susceptible to climate warming (Liu et al., 2000). Besides, the radiative impact of light absorbing particles (LAP) (e.g., black carbon and dust) may be most climatically relevant in the TP (Skiles et al., 2018). Thus, TP is an ideal region for us to analyze and compare the impacts of snow grain shape, mixing state of LAP in snow, and topography. We presented these in Section 3.1.
2) Two MODIS snow property products (i.e., STC-MODSCAG/STC-MODDRFS and SPIReS) provide good references for evaluating our model. However, unfortunately, both products are not globally available yet and only include few typical regions (e.g., TP).
3) There are many previous studies that have studied the radiative forcing of LAP over the TP, which make it possible to compare our results with the existing studies. We presented the comparison in Section 4.2.

We are extending our study to other regions. In a follow up study that is currently under review in *The Cryosphere* (Hao et al., 2022; https://egusphere.copernicus.org/preprints/2022/egusphere-2022-1097/), we have applied the newly developed snow albedo model to Western US for evaluating simulated snow processes in E3SM land model. Further applications and evaluation over the Arctic and Antarctic would be a good future direction, as more and more field measurements and remote sensing data become available.

Besides, we are currently conducting land-atmosphere coupled experiments with the new snow albedo model to analyze the LAP impacts when considering the land-atmosphere feedback. Thus, we stated the limitations of this study and stressed future extended study at a global scale with the consideration of land-atmosphere interactions in Line 544-546 of the revised manuscript.

In terms of overall context, I need another anchor. You clearly state in the abstract that the inclusion of internal mixing of dust (and other stuff) is a smaller effect than changing the shape of the snow

grains, but what about the total influence of dust (and other stuff)? Am I right to be thinking that this is, in practice, is a much bigger effect than changing snow grain shape? Please add a sentence to the manuscript to clarify.

Good point! Indeed, the impacts of LAP on snow may be comparable to or even larger than the impacts of snow grain shape, which depends on the LAP concentration in snow. Figure 5(e-f) shows that the snow albedo reduction induced by LAP can be close to 0.1 over the western TP. Figure 7(a-c) shows the difference in snow albedo between non-spherical and spherical grain shapes. Comparing Figures 5(e-f) and 7(a-c) shows that the impacts of LAP can be comparable to or even larger than the impacts of non-spherical shape in spring, especially for the western TP where the LAP concentration in snow is high. Similar conclusion can be drawn from He et al. (2018).We have now added such discussion in Line 460-465 of the revised manuscript.

The last sentence of your author contributions paragraph can be removed. Or maybe I have misunderstood what you are trying to say. All 12 are accounted for in by name in the preceding sentences where it also states that they all edited the manuscript. I think we always assume that authors improve the work even if they do so by disagreeing with some of the content. The author contributions paragraph is especially useful for highly collaborative sciences such as climate science, as it enables all authors to accept responsibility for their own contributions, even though they do not fully comprehend all the details of the whole manuscript.

We agree with you. We have deleted the last sentence in the section of author contributions.

References

Hao, D., Bisht, G., Rittger, K., Stillinger, T., Bair, E., Gu, Y., and Leung, L. R.: Evaluation of snow processes over the Western United States in E3SM land model, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2022-1097, 2022.

He, C., Flanner, M. G., Chen, F., Barlage, M., Liou, K.-N., Kang, S., Ming, J., and Qian, Y.: Black carbon-induced snow albedo reduction over the Tibetan Plateau: uncertainties from snow grain shape and aerosol–snow mixing state based on an updated SNICAR model, Atmos. Chem. Phys., 18, 11507–11527, https://doi.org/10.5194/acp-18-11507-2018, 2018.

Liu, Xiaodong, and Baode Chen. "Climatic warming in the Tibetan Plateau during recent decades." International Journal of Climatology: A Journal of the Royal Meteorological Society 20, no. 14 (2000): 1729-1742.

Skiles, S.M., Flanner, M., Cook, J.M. et al. Radiative forcing by light-absorbing particles in snow. Nature Clim Change 8, 964–971 (2018). https://doi.org/10.1038/s41558-018-0296-5.