

Geosci. Model Dev. Discuss., referee comment RC1  
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## Comment on gmd-2022-67

Anonymous Referee #1

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Referee comment on "Improving snow albedo modeling in the E3SM land model (version 2.0) and assessing its impacts on snow and surface fluxes over the Tibetan Plateau" by Dalei Hao et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-67-RC1>, 2022

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The mixing state of LAPs in snow has large impacts on LAP-induced snow albedo reduction and surface radiative forcing (RF). However, most land surface models assume that snow grain shape is spherical and LAPs are externally mixed with the snow grains. On this background, the authors improved the snow radiative transfer model in the land model (ELM v2.0) of the Energy Exascale Earth System Model version 2.0 (E3SM v2.0) by considering non-spherical snow grain shapes (i.e., spheroid, hexagonal plate and Koch snowflake) and internal mixing of dust-snow and systematically evaluates the impacts on surface energy and water balances over the Tibetan Plateau (TP). In general, this study is well written and can advance understanding of the role of snow grain shape and mixing state of LAP-snow in land surface processes and offer guidance for improving snow simulations and RF estimates in Earth system models under climate change. I think this manuscript can be accepted if address the following questions.

1. The authors spent much of this manuscript on comparing the modeling results with remote sensing snow data under the control simulation using the previous settings, which was not the main concern of this manuscript. This analysis only demonstrated the capability of the original land model in snow cover fraction or SAR simulation, but can not reflect the improvement of your work. So, I suggest the results based on the original land model and your improved version can be simultaneously compared with remote sensing data to highlight the improvement of your simulation.

2. In the Abstract, the authors said "Compared with two remote sensing ....., the control ELM simulation with the default settings of spherical snow grain shape, internal mixing of BC-snow, external mixing of dust-snow and without TOP can capture the overall snow distribution reasonably." So my question is what is your contribution? From your abstract, I can not find better simulation results after improving these snow microphysics properties. I think the authors should highlight the advances in reproducing the satellite observations after the improvements.

3. The two used remote sensing data of ASR have larger differences themselves, so I wonder whether it is appropriate to use these two data in comparing with your simulations. Maybe only using MODSCAG/MODDRFS data is better from your results.

4. From the results, I found the snow grain shape and TOP have larger impacts on snow cover fraction or SAR simulation, while the impact of mixing state is relatively lower. So my question is why you spent most of your content on snow grain shape and mixing state, but the impact of TOP was only simply discussed in the last Section 4.4.

5. When discussing the impacts on water cycles, the authors only spent little content while this part was very important. In addition, this is no figure in the manuscript in this part. I think more discussions are needed because the impact of snow grain shape and mixing state of LAP-snow on SAR or RF have been fully discussed in previous studies while the impact on water cycles is little mentioned.

6. In the Discussion, the authors spent much time on discussing why the snow cover was underestimated in spring. As mentioned in the Discussion, the underestimation may be caused by the use of a constant snow accumulation ratio, empirical snowmelt shape factor, and the complex vegetation-snow interaction processes of snow interception and dynamical removal from canopy. So I think this was not closely related to the contributions of this manuscript. While the discussions from Lines 468-517 are the main limitations in your work and the directions the future work moves on. I suggest the authors can discuss the limitations in the first place, then simply discuss the underestimation.

