Review Comments for "Snow-darkening versus direct radiative effects of mineral dust aerosol on the Indian summer monsoon: role of the Tibetan Plateau" by Shi et al.

The authors conducted a set of GCM simulations to quantify dust SDE and DRE over the Tibetan Plateau and its impacts on Indian monsoon onset. They found that dust SDE and DRE exert opposite effects on Indian monsoon onset and proposed a possible mechanism. The results are interesting and the authors did a generally good job in writing the manuscript. However, some parts of the manuscript still need to be improved, particularly for model descriptions and evaluations. Please see my following comments.

Major Comments:

1. Section 2 (Model and Experiments): Since this work is a modeling study, the model descriptions require more details. Here are some examples. (1) What is the new dust size distribution used in CAM4-BAM? How many size bins are used and what are the values for these bins? (2) For dust optical properties, what have been updated? (3) The model simulations did not include aerosol indirect effect but used prescribed CCN. Does this mean that aerosol wet removal through in-cloud process is not included? If so, this could cause large uncertainty in simulations. Please clarify how the prescribed aerosol in-cloud process would affect aerosol wet deposition. (4) The authors used the SNICAR model to deal with snow darkening processes. How does the model handle aerosol-snow interactions? To my understanding, SNICAR assumes external mixing between aerosols and spherical snow grains. But recent studies have suggested that aerosol-snow internal mixing and nonspherical snow shape could significantly affect aerosol-induced snow albedo effects (e.g., Flanner et al., 2012; Liou et al., 2014; Räisänen et al., 2017; He et al., 2018), which may introduce some uncertainty in the simulations here. (5) How does the model deal with the aerosol removal in snowpack? Does it assume a fixed removal efficiency? (6) The way to calculate SDE and DRE by computing the difference between EXP1 and EXP2 and between EXP2 and EXP3 has an underlying assumption that SDE and DRE are linearly additive. However, SDE and DRE could have interactive and nonlinear effects, which makes the calculations above inaccurate. For example, if we refer EXP4 to a new experiment with only SDE enabled, then how different would the result be if calculating DRE by taking the difference between EXP1 and EXP4, compared with "EXP2 minus EXP3". And how different would the result be if calculating SDE by taking the difference between EXP4 and EXP3, compared with "EXP1 minus EXP2". Do the authors have any suggestions on which way of calculation is more accurate in terms of quantifying dust SDE and DRE?

References:
Flanner, M. G., et al.: Enhanced solar energy absorption by internally-mixed black carbon in snow grains, Atmos. Chem. Phys., 12(10), 4699–4721, 2012.
He, C., et al.: Impact of grain shape and multiple black carbon internal mixing on snow albedo: Parameterization and radiative effect analysis, J. Geophys. Res.-Atmos., 123, 1253–1268, 2018.
Liou, K. N., et al.: Stochastic parameterization for light absorption by internally mixed BC/dust in snow grains for application to climate models, J. Geophys. Res.-Atmos., 119, 7616–7632, 2014.
Räisänen, P., et al.: Effects of snow grain shape on climate simulations: Sensitivity tests with the Norwegian Earth System Model, The Cryosphere, 2017.

2. Section 3.1 (Model validation): (1) For the AOD evaluation, since the model simulation did not include non-dust aerosols, it is not an apple-to-apple comparison for modeled and MISR AOD here. The AOD comparison did not give us very useful information. If the authors want to use

total AOD from observations, the model simulations need to include all aerosol types. If the authors only want dust AOD, maybe CALIPSO observations could help. Focusing on AOD over dust source regions can also be a way to evaluate modeled dust AOD, but in that case it is difficult to know how the model performs in terms of dust transport, particularly over remote regions such as the Tibetan Plateau. Besides, even over the dust source regions such as north of Tibetan Plateau, the modeled AOD is much smaller than MISR AOD. What would be the possible reasons? (2) Also it seems that MODIS AOD is better than MISR AOD at least over dust source regions, due to the MODIS deep blue retrieval algorithm. Why did the authors select MISR instead of MODIS? (3) The authors described in detail the consistency and inconsistency between model simulations and observations in terms of AOD, snow cover, and monsoon climatology, but it appears that not enough explanations have been provided for the model-observation differences. The readers may also want to know the reasons causing the model-observation discrepancies, which would be very useful for future model improvements. (4) Since the snow darkening effect (i.e., albedo reduction) is one focus in this work, it would be straightforward to consider evaluating modeled snow/surface albedo at least over the Tibetan Plateau, for example, by comparing with MODIS albedo product. Is there any specific reason for the authors to leave out this part?

Minor Comments:

1. Page 1, Line 16: I suggest replacing "clarified" with "quantified".

2. Page 2, Line 10: Please remove "reflect," since reflection is part of scattering.

3. Page 2, Lines 31-34: For the authors' information, some recent studies on BC/dust SDE are missing here, which improved the understanding of aerosol SDE particularly over the Tibetan Plateau. Some examples are listed as follows.

References:

He, C., et al: Black carbon radiative forcing over the Tibetan Plateau, Geophys. Res. Lett., 41, 7806–7813, 2014.

Zhao, C., et al.: Simulating black carbon and dust and their radiative forcing in seasonal snow: a case study over North China with field campaign measurements, Atmos. Chem. Phys., 14, 11475-11491, 2014.

Lee, W.-L., et al.: Impact of absorbing aerosol deposition on snow albedo reduction over the southern Tibetan plateau based on satellite observations, Theor. Appl. Climatol., 129(3-4), 1373-1382, 2017.

Niu, H.W., et al.: Distribution of light-absorbing impurities in snow of glacier on Mt. Yulong, southeastern Tibetan Plateau, Atmos. Res., 197, 474-484, 2017.

4. Section 1 (Introduction): It seems that the authors did not mention their motivation to focus on the Tibetan region particularly. Thus, I suggest adding a short paragraph to highlight the importance of Tibetan Plateau (such as its role in altering Asian water resources and hydrological cycle), although the authors already mentioned a little bit in the descriptions of dust effects.

5. Page 9, Line 6: please remove "is" before "occurs".