**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.

RE: Thanks for the positive 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?

RE: There are four size bins (0-1.0 μm; 1.0-2.5 μm; 2.5-5.0 μm; 5.0-10.0 μm) in diameters used in this study. In this version, the precentages of emission (0.02, 0.09, 0.27, 0.62) is modified to allow more large particles.

We noted this in the revision (Page 3 Lines 28-32): "The dust cycle including the emission, transport and deposition, is parameterized in CAM4 and its radiative feedbacks are also calculated on line. The dust sizes in CAM4 contain four bins of 0-1.0 μm; 1.0-2.5 μm; 2.5-5.0 μm; 5.0-10.0 μm in diameters, respectively (Mahowald et al., 2006). The CAM4-BAM has been improved by an optimized soil erodibility map and a new size distribution for dust emission (the percentages for four bins are 0.02,0.09,0.27,0.62, respectively), as well as updated optical properties for radiation budget, to present a better performance on simulating the global dust cycle (Albani et al., 2014)"

(2) For dust optical properties, what have been updated?

RE: Several optics in the CAM4-BAM were improved, as introduced in Page 7 of Albani et al., (2014). Due to limited space, we did not introduce these changes in our paper. We referred to Albani et al.' paper for detailed information (Page 3 Lines 31-32).

(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.

RE: The reason we chose the model CAM4 not CAM5 is that CAM4 does not include aerosol indirect effect, which is complicated with large uncertainty and not our focus. Certainly, the choice of CAM4 leads to other uncertainty, e.g., the in cloud removal of dust, as the reviewer pointed out. We admit this bias in simulations, however, the bias in wet deposition does not affect our discussion because wet deposition occupies a small part of total deposition over dust source regions.

We emphasized this possible bias in the revision (Page 3 Lines 32-33; Page 4 Lines 1-2): "In CAM4-BAM, the SDE of all aerosols are enabled but the indirect effect is not considered, which means that the aerosol changes in cloud process as condensation nuclei are prescribed. Wet removal through in-cloud process is not considered, which may induce bias of dust deposition on snow over Asia. "

(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.

RE: Thanks for the comment. SCINAR assumes external mixing between aerosols and spherical snow grains, which may induce uncertainty.

We referred the publications and discussed the uncertainty of simplification of spherical snow grains in this model (Page 4 Lines 7-9): "Of note is that SCINAR assumes external mixing between aerosols and spherical snow grains, however, aerosol-snow internal mixing and nonspherical snow shape could significantly affect aerosol-induced snow albedo effects, based on recent studies (Flanner et al., 2012; Liou et al., 2014; Räisänen et al., 2017; He et al., 2018)."

(5) How does the model deal with the aerosol removal in snowpack? Does it assume a fixed removal efficiency?

RE: Yes. In the model, it assume a fixed removal efficiency, that is, the removal by meltwater is proportional to its mass mixing ration (multiplied by a scavenging factor).

(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?

RE: Yes, the reviewer is right. The nonlinear term indeed exists because of the interaction between two effects. A good way to examine the nonlinear term is to conduct a fourth experiment with only SDE enabled and see whether EXP1-EXP4 and EXP2 and EXP3 is consistent (if yes, it means the nonlinear term can be neglected). In this study, we mainly focused the SDE of dust so we used EXP1-EXP2 (not EXP4-EXP3) because we considered the EXP1 as control experiment and in real world these two effects are indeed enabled. However, due to limited time for final response phase, it is difficult to finish the EXP4 to examine the nonlinear term (We have already added three more experiments on black carbon. Please see the response for RC2). If the reviewer insists, we wish to finish the EXP4 in next phase.

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?

RE: Thanks. We used the CALIPSO data instead in the revision. Compared to the dust

AOD in CALIPSO data, the simulated AOD is smaller, especially over the source areas (Figure R1). The possible reasons are as follows. Over the dust source, there are lots of dust with larger particle sizes but in the model, the considered dust particles are restricted to less than 10μm. Thus, the dust forcing is underestimated due to less coarser dusts in the current global climate models (Kok et al., 2017). Also, dust model may have quite large differences in simulating vertical distribution, emission, deposition, and surface concentration of dust (Pu and Ginoux, 2018), which affects the AOD as an integrated variable. Furthermore, the spatial resolution of model is not fine enough, which fails to well resolve the complex topography and dust sources over East Asia.

We mentioned these in the revision (Page 5 Lines 5-8): "The simulated absolute values of dust AOD over Arabian Peninsular, southwestern slope of the TP and Taklimakan desert are biased low because the considered dust particles are restricted to less than 10.0 μm and the dust forcing is underestimated due to less coarser dusts in the current global climate models (Kok et al., 2017). "

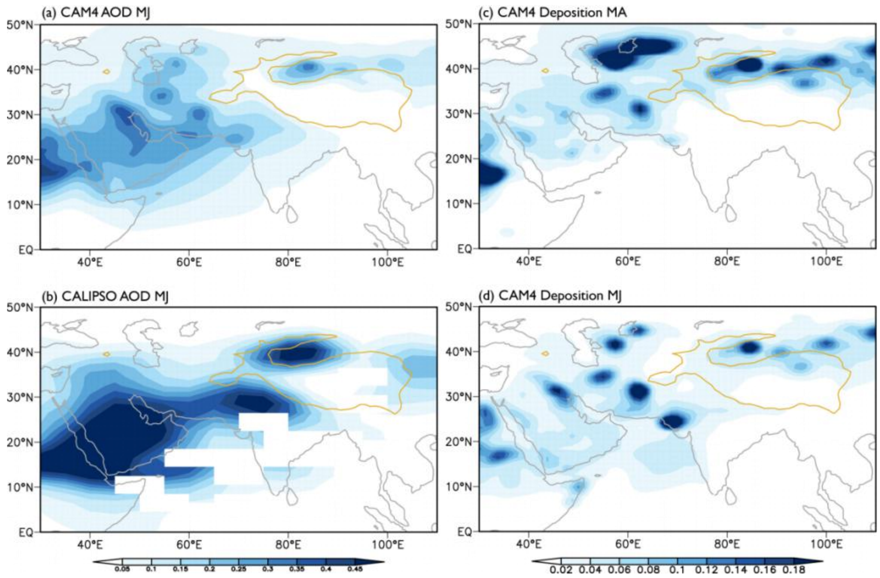


Figure R1: Averaged dust aerosol optical depth over Asia for May and June in CAM4 (a) and in Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIPSO)-retrieved data for 2007-2011 (b); and dust deposition flux including both dry and wet deposition for March and April (c) and for May and June (d).

(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?

RE: See the response above. We used CALIPSO data for dust because it can be directly compared to our simulated AOD.

(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.

RE: For the reasons for AOD differences, we discussed it in previous response 2-(1). The main cause for the underestimation on snow cover fraction and precipitation over monsoon regions is obviously the coarser resolution in our model. For example, our model can not resolve the high mountains over Tianshan mountains and western Tibetan Plateau, wihch the simulated snow cover fraction is relative smaller. Similarly, thin high topography over western edges of Indian subcontinent and Indo-China Peninsula is also partly missed so the observed maximal precipitation at these regions are also smaller in model.

In the revision, we added these explanations (Page 5 Lines 5-8, 17-20): "The simulated absolute values of dust AOD over Arabian Peninsular, southwestern slope of the TP and Taklimakan desert are biased low because the considered dust particles are restricted to less than 10.0 μm and the dust forcing is underestimated due to less coarser dusts in the current global climate models (Kok et al., 2017).", "Over the western TP, the MODIS observation presents a fraction larger than 80% but the simulated fraction is smaller. In particular, the model underestimates the elevations of finer-scale mountains and corresponding snow cover fractions due to the coarser resolution, e.g., over the Tianshan mountains. "

(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?

RE: Thanks. We did not show the albedo because the albedo is directly controlled by the snow cover fraction. Compared to the MODIS data over the TP (Meng et al., 2018), the model captures its spatial distribution but overestimates the surface albedo, which is similar with multi-model ensembles (Li et al., 2016), mainly due to the overestimated snow cover fractions.

Minor Comments:

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

RE: We kept unchanged because we can not quantify their links.

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

RE: Removed.

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.

RE: Thanks for the references and we added them (Page 3 Line 4).

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.

RE: Following the comments of another reviewer, we largely reorganized the results and discussion in the revision. In the original manucript, the emphasized role of Tibetan Plateau may be not approprite (the reasons can be detailed introduced in response RC2). Thus, we do not add a paragraph to introduce the role of TP.

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

RE: Removed.