Review Comments for "Modulation of surface sensible heating over the Tibetan Plateau on the interannual variability of East Asian dust cycle" by Xie et al.

Dust has important effects on TP and Asian regional climate, while the change in TP climate can feed back to dust generation and lifecycle. The authors conducted model simulations combined with analysis of observations and reanalysis datasets to quantify the effects of TPSH on the interannual variability of dust life cycle in East Asia. This would potentially enhance our understanding of the interactions between dust aerosols and Asian/TP climate, and hence regional climate change. The manuscript is generally well-structured, and the methodology is also sound. Before it can be considered for publication, I have a few comments and suggestions to potentially improve the quality of the manuscript. Particularly, more clarifications and discussions are needed in the modeling and analysis parts. Please see my specific comments below.

Comments
1. Title: The authors mainly focused on MAM TPSH, so I suggest including "Springtime" in the title to avoid any confusion.

2. Introduction: It would be good if the authors could explicitly highlight the difference and novelty of this study compared with their previous study (Xie et al., 2018b), since there are some overlaps between the two studies.

3. Section 2: (1) The website links for the observation and modeling data used in this study need to be provided if they are publicly accessible. (2) For the model, I suggest the authors use CAM-chem/MAM in the future study, which includes more realistic aerosol representations. Besides, the assumption of aerosol external mixing and/or simplified (i.e., spherical) particle structure/morphology in CAM4-BAM can lead to uncertainty/bias in DRF (e.g., He et al., 2015: https://doi.org/10.5194/acp-15-11967-2015; Scarnato et al., 2015: https://doi.org/10.5194/acp-15-6913-2015). This issue needs to be discussed to some extent in the manuscript. (3) Was the aerosol-cloud interaction included in the model? The authors did not mention this. (4) I am not quite convinced that it is a good idea to fix the emissions in year 1850. As the authors mentioned, the anthropogenic emissions could serve as a confounding factor for the perturbation of the MAM TPSH impact. If removing the anthropogenic emissions, then the resulting effect of TPSH on dust cycle would not be realistic. If so, the authors need to make some clarifications in the title, abstract, and introduction sections to state that this study only considers the scenario without anthropogenic emission effect. This issue is particularly important when considering that the observations used to evaluate model actually reflect both the natural dust emissions and anthropogenic emissions of other species throughout the study period.

4. Section 3.1: (1) Some more discussions on the reasons of the spatial patterns and biases of TPSH need to be provided. Currently, the authors mainly described the results without many explanations of the physics behind in this section. (2) A particularly interesting question is that dust also feeds back to the TP climate and affect TPSH. For example, column dust leads to surface dimming and reduce TPSH, while dust in snow enhance surface warming and increase TPSH. So how does this dust feedback contribute to the variation of TPSH under the effects of other influential factors? The authors mainly discussed the effect of TPSH on dust in the

following sections, but it will be interesting to see the effect of dust on TPSH too. (3) The authors seem to focus on evaluating modeled surface dust concentrations only. Why not also evaluate the column property such as AOD? This at least will be meaningful over dust source regions. This is also important for the discussions on dust loading in the following sections.

5. Section 5: (1) The authors made the argument that the dust-in-snow feedback plays an important role, but without any quantitative analysis and/or figures. The dust-in-snow feedback is actually an interesting point, so it would be good to see some quantitative results, figures, and discussions to back up the argument. Besides, a recent study (He et al., 2019: https://doi.org/10.1029/2019MS001737) showed that the dust-snow albedo effect/feedback can be significantly enhanced by dust-snow internal mixing compared with external mixing (presumably assumed in CAM4-BAM model). This could potentially enhance the importance of the dust-in-snow feedback in modulating TPSH. Some discussions on this aspect would be useful. (2) Since a number of potential uncertainty factors (some of them have been mentioned in my comments above) could be involved in the model simulations and analysis, I suggest including a paragraph or two to specifically discuss the uncertainties of this study in this section.