Review of "Quantifying snow-darkening and atmospheric radiative effects of black carbon and dust on the south-Asian monsoon ..." by Rahimi et al.

Using CESM with variable resolution, this study quantified the climatic impacts of absorbing aerosols through the snow-darkening effect and aerosol–radiation interaction. The scientific questions in this paper have been partially addressed in the literature. However, the authors used a relatively novel technique in the model, which is quite attractive to researchers who focus on regional climate using general circulation models. It is also unique that the authors compared the relative impacts induced by snow-darkening effects and aerosol–radiation interaction. I suggest a minor revision before being published.

Major comments:

1. Based on line 187–188, anthropogenic aerosols and precursor gas emissions are for the year of 2000. Why the authors compare the simulated AOD with observations from multiple years, such as 2001 to 2014 for MODIS and 2002–2014 for MISR. To make a fair comparison, only observations from the year of 2000 should be used, because both anthropogenic emissions in Asia and dust emission in the Middle East have experienced significant decadal increasing trends during the first decade of the 21st century (e.g., Hsu et al., 2012; Jin et al., 2018). I strongly disagree with the comparison method related to AOD in this paper. Moreover, the simulations were run for 11 years and only 10 years of data were used in the analyses, but observational data used for comparisons are more than 10 years.
2. AOD simulation results are too poor, as shown in Figure 2. Model significantly over estimate AOD over dust-source regions, which to me is highly caused by inconsistent spatial resolution of erodibility dataset used in dust emission scheme in CLM from model spatial resolution. The default erodibility dataset in CESM is for simulations of resolution of 1.9° by 2.5°. Dust simulations configured with different spatial resolutions other than 1.9° by 2.5° can be improved by tuning dust emission factor in the model, or else we would get largely over- or underestimation of dust aerosols, such as in Figure 2. I am not suggesting a re-run of the model, but just point this out and the authors should mention about this in their paper. On the other hand, the model underestimates AOD and misses the spatial gradients of AOD over heavily-polluted areas, such as in East China, Sichuan Basin, and IGG. What causes these inconsistencies should be at least discussed in the paper. Last but not least, model underestimates AOD over oceanic areas, probably meaning model underestimate sea-salt emissions, which should be also mentioned and discussed.
3. The analyses in the main text of this manuscript focused on May and June, which is the pre-monsoon season . Therefore, I suggest the authors change "monsoon" in the title to "pre-monsoon". Or the authors could move analyses during monsoon season from supplementary to the main text without modifying the title.

Minor comments:

1. Line 146, add this citation in your reference list.

2. Section 2.2, add the refractive index of dust and black carbon that are used in the simulations. Also indicate the spatial resolution of the erodibility dataset used in the dust emission scheme in CLM4.
3. Section 2.3, before describe all of the disturbed experiments, the control experiments should be first described in details, such as initial conditions, simulation period, and so on.
4. Line 187–188, revise this sentence so that it is clear that what variables are climatological and what variables are for the year of 2000. If they are climatological, indicate the period over which these variables are calculated.
5. Line 228, what interpolation method is used, e.g., linearly or non-linearly? Within how large areas were the spatial average calculated?
6. Section 3.1, again the comparisons of AOD between model and observations are not temporally consistent. For example, why MERRA AOD from 1980–2017 were used? Model simulation period does not cover the years before 2000. When using MACv2 and site observation in He et al. (2014) and Yang et al. (2018), please indicate the period when these observations were collected. Are these observations cover the same period as your model simulation?
7. Line 271–272, I do not quite understand this sentence, please revise or explain it.
8. In Figure 3, whenever correlations are discussed, please also indicate the associated p-values.
9. Figure 8, please indicate where the radiative effect was calculated, such as at the surface, top of the atmosphere, or in the atmosphere. It seems to me that they were the radiative effects at the surface. If so, I am surprised to see positive radiative effects. Please explain why.
10. Figure 11, please overlay the significant changes of circulation over Figure 11 even though you had Figure 13. And also compare and discuss spatial patterns and magnitudes of rainfall response to dust aerosols in this study with those in previous studies.

References

Hsu, N. C., Gautam, R., Sayer, A. M., Bettenhausen, C., Li, C., Jeong, M. J., Tsay, S. C., and Holben, B. N.: Global and regional trends of aerosol optical depth over land and ocean using SeaWiFS measurements from 1997 to 2010, Atmos Chem Phys, 12, 8037-8053, 10.5194/acp-12-8037-2012, 2012.

Jin, Q., Wei, J., Pu, B., Yang, Z. L., and Parajuli, S. P.: High summertime aerosol loadings over the Arabian Sea and their transport pathways, Journal of Geophysical Research: Atmospheres, 123, 10568-10590, 10.1029/2018jd028588, 2018.