

Interactive comment on “Quantifying snow-darkening and atmospheric radiative effects of black carbon and dust on the South-Aisan Monsoon and hydrological cycle: Experiments using variable resolution CESM” by Stefan Rahimi et al.

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Response to anonymous referee #2

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 at-

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tractive 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.

Reply: We thank the referee for their positive comments.

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.

Reply: Thank you for the insightful comment. Unfortunately, accurate satellite-derived AOD measurements are not generally available prior to the year 2000. Additionally, MODIS Terra and Aqua were launched in 1999 and 2002, respectively, while MISR was launched in 1999. Many of these satellite data resources do not have available level-3 data until several months or even a year after the launch date. We used all available AERONET measurements across the region from 1992–2016. However, the earliest data across southern Asia wasn't available until 1998, and there are many instances of missing AERONET data across south-Asia following 1998. The model simulations were conducted with climatological SST and sea ice, and present-day aerosol and precursor emissions (represented by the year 2000). This is always an issue when comparing climate model simulations with multiple observations (conducted also in

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different time periods).

To address this comment, we regenerated Fig. 3 but using MODIS, MISR, and AERONET data from their respective first 5 years of measurements. CONT-vr and CONT-un continued to underpredict AOD compared to observations. AOD underprediction actually worsened noticeably compared to AERONET measurements, while the undersimulation of AOD by CESM was similar to the case in which all reference data years were used.

This comment is important to the interpretation of the manuscripts key results. As such, the following paragraph has been added to the end of Section 3.2: “Non-simultaneity between simulations and observation data may also play a role in skewing the interpretation of simulated aerosol features. 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). These trends may partially explain why the CESM experiments conducted with the year 2000 emissions underpredict AOD compared to observations. It is important to keep in mind these considerations when interpreting the relatively poor model performance in simulating AOD.”

(2) AOD simulation results are too poor, as shown in Figure 2. Model significantly overestimate 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

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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.

Reply: Thanks for this helpful comment. Based on your suggestion, we have added the following paragraph to Section 3.2, “Across deserts, the overestimation of AOD may be due to the fact that CLM uses default erodibility dataset originally designed for use at a $1.9^\circ \times 2.5^\circ$ grid. The fact that many areas of our domain refined to 0.125° grid spacing may lead to an overestimation of dust emissions across the region, correctable by tuning the dust emission factor. Over heavily-polluted regions (e.g., East China), CONT-un and CONT-vr’s underprediction of AOD compared to observations may be due to the underestimation of anthropogenic aerosol emissions and the missing treatment of secondary aerosol production in the models (Fan et al., 2018). Across oceanic regions, the undersimulated AOD by models is most likely the result of inadequate sea salt emissions, which is not a focus of this study.”

(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.

Thank you for this advice. This comment is difficult to address. We found that, generally, the largest perturbations to the SAM occurred in the late spring or early summer. This time period is in a “gray zone” (i.e., it is difficult to define this time period as premonsoon or monsoon). Monsoon alterations in precipitation, specific humidity, and SWE were discussed and figures showing time series were included in the main manuscript as it is now (Figs. 7, 9, 12). In addition, equal attention was given to the July/August time period, but in the interest of manuscript length and in an attempt to reduce the number of figures, a considerable portion of the figures concerning the JA results were moved to the supplement and only mentioned in the main text. So, we will keep the title as is, but we will keep the possibility of adding figures in the main text for

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July/August, so that the nature of the manuscript is not misrepresented.

Minor comments

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

Reply: Added. The paper was just published in JGR, so the citation was changed to Rahimi et al. (2019).

(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.

Reply: The refractive indices for dust and BC have been included (first paragraph of Sec 2.2), as has the erodibility data resolution (first paragraph of Sec. 2.2).

(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.

Reply: This is a good idea. The paragraph describing the details of the control experiments has been moved before the description of the disturbed experiments. The paragraph in question: "Each individual experiment is run for 11 years, and the first year in each simulation is neglected in the analysis to allow for "spin up". Climatological sea surface temperature, sea ice, and anthropogenic aerosol and precursor gas emissions for the year 2000 are used. After comparing the simulations to both gridded and point source (locations shown in Figure 1b) reference data, the means of various climate variables from the last 10-year of simulations are computed to evaluate the impacts of BCD-induced SDE and ARI across southern Asia. Furthermore, all simulated data (both VR and UN) across the region are interpolated to an identical 0.125o rectilinear grid for direct comparison," is now the second paragraph in Sec. 2.3.

(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

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the period over which these variables are calculated

Reply: The sentence has been clarified to read, "The simulations are run with prescribed climatological sea surface temperature and sea ice cover averaged from 1982-2001 (Hurrell et al., 2008). The greenhouse gas concentrations and anthropogenic aerosol and precursor gas emissions are prescribed at the level for the year 2000."

(5) Line 228, what interpolation method is used, e.g., linearly or non-linearly? Within how large areas were the spatial average calculated?

Reply: All AERONET sites shown in Fig. 1b were used, so between 60o-110oE and between 5o-40oN. Data were interpolated linearly. A key sentence in Sec. 3.1 paragraph 3 has been modified to read, "Model results are linearly interpolated to AERONET site locations and averaged spatially (see Figure 1b) between 60oE-110oE and between 5oN-40oN."

(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?

Reply: Thank you for this comment. This does need clarification in the manuscript per your suggestion. MERRA-2 dates were chosen to encompass the largest period of record straddling the year 2000, which sits right in the middle of the date range of MERRA-2. Based on your comment, the MERRA-2 data time series was regenerated using the years 1998-2002. MERRA-2 AOD (1998-2002) was only slightly lower than MERRA-2 AOD (1980-2017); CESM experiments still showed a low bias comparably. Therefore, Fig. 4 remains unchanged.

Concerning the ground-based surface measurements from He et al. (2014) and Yang

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et al. (2018), the answer is technically no; we did not use temporally consistent surface measurements with our model time period (year 2000 emissions scenario). For He et al. (2014), all in-snow and in-atmosphere BC measurements were taken mostly between 1999 and 2006 (with the exception of one site). For Yang et al. (2018), all measurements were taken between 2008 and 2013. The temporal inconsistency between our simulations and observations has now been stressed and discussed in Secs. 3.2 and 3.3, and Table 2 has been added to highlight the site name, measurement time, location, elevation, and relevant citations (more than 20) for each surface measurement. Specifically, the following paragraphs have been added to the end of Secs. 3.2 and 3.3, respectively:

“Non-simultaneity between simulations and observation data may also play a role in skewing the interpretation of simulated aerosol features. 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). These trends may partially explain why the CESM experiments conducted with the year 2000 emissions underpredict AOD compared to observations.”

“Similar to what was noted in Section 3.2, the temporal inconsistency between point source BC measurements and the CESM experiments must be kept in mind. BC measurements were conducted between 1999 and 2013, while simulations are run with year 2000 anthropogenic emissions. Our results could therefore be biased depending on the trends in BC emissions after the year 2000.”

(7) Line 271–272, I do not quite understand this sentence, please revise or explain it

Reply: Thanks for the comment. We have removed the confusing statement, and the sentence now reads: “Furthermore, CONT-vr slope values for the total, fine-mode, and coarse-mode best-fit lines of 0.157, 0.135, and 0.402, respectively. The point of this statement is to reinforce the idea that CONT-vr and CONT-un underpredict AOD.

(8) In Figure 3, whenever correlations are discussed, please also indicate the associ-

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ated p-values.

Reply: This is a good idea. p-values have been added to panels (d) and (e). Additionally, p-values were computed for the data in panels (a) through (c), but they were very small (less than 10⁻¹⁰) indicating the rejection of the null hypothesis and highlighting the large differences between AERONET and simulations. Therefore, we did not explicitly add them to the plot. We did however mention their smallness in the Figure 3 caption.

In the interest of space, we did not discuss the relatively larger p-values between AERONET/MISR, AERONET/MACv2, and AERONET/MERRA-2. The means of MISR, MACv2, and MERRA-2 were closer to the AERONET mean. As time series become more similar, their p-values approach unity. This may explain the largeness of these datasets' 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.

Reply: Panels (a) and (b) are at the TOA, while panels (c) through (e) are the in-snow radiative effect of BC and dust across the 3 snow-covered subregions at the surface. To comply with another reviewer's request, we have moved this figure to Figure 5, and we have modified the figure and the caption to be clearer.

(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.

Reply: Thank you for this comment. Due to the very busy nature of overlaying circulation contours on the precipitation figure (Fig. 11) with statistical significance hatching,

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it was decided to leave this figure as it is. The point of Fig. 13 is to unify the results subject to thermal vorticity arguments. Fig. 11 stands alone for the precipitation section (Sec. 4.5) until the application of the theoretical dynamical framework.

Regarding the discussion of previous rainfall responses noted in other studies, the following sentence has been added in Sec. 4.5, paragraph 2: "These increases, primarily induced by dust ARI, are similar to those reported in Jin et al. (2016) and slightly larger than those found in Vinoj et al. (2014)." Previously, many studies have not considered dust effect on precipitation alone and have considered the effect of all aerosols together.

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