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Atmospheric Chemistry and Physics Discussions

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 #1

The paper provides a comprehensive analysis on absorbing aerosols effects on simulated South Asian monsoon system. The main novelty is a variable resolution modeling system which benefits climate simulation over mountain regions. I recommend some revision before it goes out for publication.

We thank the reviewer for their positive comments.

Major comments

(1) On the temperature response: For panel (e), why does BC induce local cooling? I thought BC is widely considered as a warming agent? This is even more puzzling when Figure 8a shows BC forcing as positive everywhere. Also, since the SST is constrained by observed value, it is highly doubtful that the 2m temperature over ocean will be 'significantly' changed due to BC and/or dust (seen from the hatch signs in Figure 5). Please clarify.

Reply: CAM results (i.e. the model output) reflect the combination of the forcing from BC plus the atmospheric response (i.e. warming across Tibet and cooling across India). This sum is certainly nonlinear. BC induces a positive radiative effect at the top of the atmosphere (TOA) while dimming the surface initially (dimming effects not explicitly examined here). BC warming of the air column (taken here to be the atmospheric geostrophic forcing) across south-Asia brings forth expansion of the air column across the region and rising vertical motion (the atmospheric ageostrophic response to BC-induced warming). This response is coupled to kinematic changes in the lower- and upper-tropospheric flow during May and June via the vertical vorticity equation (Eqs. 8-14), with cyclonic (anticyclonic) flow anomalies. Specifically, the cyclonic flow anomaly is oriented in such a way that its easterly component bifurcates the Arabian Sea, leading to enhanced premonsoon moisture transport into south-Asia. This results in increased cloud coverage and precipitation, which reduces the surface temperature. However, the column-averaged temperature (700-300 hPa) warms due to BC during this same time. The point here is to show that, even though BC warms the TOA initially, the atmosphere responds to BC's radiative effect with processes that can actually oppose the TOA warming associated with BC. This forcing/response logic applies to dust radiative effects as well. To summarize, BC warms the atmospheric column initially (the forcing). The atmospheric response leads to increased clouds and precipitation across south-Asia. The surface cooling across south-Asia is due to (i) aerosol dimming, (ii) increased cloud coverage, and (iii) increased low-level evaporation of precipitation.

Across the ocean, significant temperature changes were simulated owing to large changes in clouds and specific humidity owing to BC and dust effects. We note that these temperature changes were small (0.1∼0.2 K) and acknowledge that their statistical significance is subject to change if SSTs were not prescribed. We therefore add a sentence to the end of Sec. 4.2 paragraph 1: "We note that T2 changes of 0.1∼0.2oC are simulated across portions of the Arabian Sea. These values should not be interpreted as significant due to the fact that SSTs are prescribed."

The figure referred to in this reviewer's comment is now Fig. 6.

(2) On the radiative forcing: I would also recommend placing radiative effect (section 3.3) ahead of temperature and snow cover. How about the reflecting and surface dimming effects from BC and dust? Are these quantified? How about long-wave effect from dust? The authors need to say a bit more on whether this is considered in the forcing calculation (Line 61). How about Brown Caron? This is not mentioned at all – any reason for the simplification. How about snow grain size effect in affecting the albedo? Is this considered anyway in the simulation? So, you did not consider any cloud-aerosol-interaction in this study?

Reply: We address this comment in segments for clarity:

i. The radiative effect section has been moved ahead of the temperature and SWE sections as suggested, and the figures have been rearranged accordingly.

ii. Surface dimming and scattering by BCD were computed by the model, however these results are not presented in this study. Furthermore, dust longwave effects are computed by the model, but analyses for longwave or shortwave effects specifically are not conducted individually; our evaluation of radiative effects focused on the combined (SW+LW) effects. The radiative effects of BCD were calculated in the control experiment following Ghan et al. (2012) as diagnostics in control experiments (CONT-

vr and CONT-un). For the in-atmosphere BCD meteorological effect/response to be quantified (i.e. ARI), simulations were run with BCD excluded from the calculation of both the shortwave and longwave aerosol optical properties, effectively removing these aerosols' atmospheric radiative effects while preserving their capabilities to serve as CCN/IN (indirect effects). These "perturbation" experiments were then compared to CONT-vr (or CONT-un) to determine the meteorological response associated with their ARI.

iii. CESM 1.2.0 does not have the capability to explicitly treat Brown Carbon; hence its effect was not quantified here.

iv. Snow grain size change due to snowpack aging and its subsequent effect on snow albedo is treated in the SNICAR model (see line 151). The following clarification has been added to line 152: "Coupled into CLM4 is the SNICAR model, which treats albedo reductions associated with snow aging and snow grain size changes, as well as BC and dust deposition on snowpack absorption"

v. The CESM model considers aerosol-cloud interactions, specifically first and second indirect effects. However, the effects due to aerosol-cloud interactions were not quantified in this study and remain a focus of future work.

(3) Since the main novelty of this paper is the new modeling system, the authors should highlight more on what're the benefits of variable resolution? Overall, I do not see a clear comparison.

a. The section later related to VR and UN difference is a bit short and weak. For example, why the response is smaller in the case of VR? The authors only vaguely mentioned about meso- scale heating and it is very unclear what kind of evidence support that argument. The contrast between VR and UN is very important, because justifies whether people want to use VR for similar types of questions (precipitin and snow in terrain-complex regions). So please expand the discussion a bit more.

Reply: We thank the reviewer for the comment. As indicated on line 660, the differences in the total aerosol-induced response (ARI+SDE) between CONT-vr and CONT-un is attributed to a weaker SDE simulated in the VR experiment, which we believe to be more accurate. In the UN experiment, the snow coverage across Tibet is simulated to be much more homogeneous than in CONT-vr. Rahimi et al. (2019) showed that CONT-vr better simulates snow coverage than CONT-un. The deposition of BCD onto the snow is also simulated to be more uniform in CONT-un than in the VR experiment across this zone. Therefore, the warming induced by BCD SDE is simulated to be much more spatially consistent (and larger) in the UN experiment than in the VR experiment. The uniformity of the simulated SDE-induced temperature anomalies is the result of the more uniform terrain treatment in the 1-degree experiment. Since the SDE-induced warming is less spatially consistent (more heterogeneous) in the VR experiments, the overall response is smaller than in the UN experiment. Specifically, the SDE-induced warming in the VR experiment is insufficient to initiate a response comparable to that of ARI discussed in Sec. 5.1. In the UN experiment however, the SDE-induced warming is sufficient to initiate a response comparable to that of ARI.

The final paragraph to section 5.3 has been modified to accentuate the explanation for the differences between CONT-un and CONT-vr. In addition, 3 supplementary figures, S12, S13, and S14, have been added to showcase the differences in simulated water vapor, 850 hPa winds, and CF between CONT-un and CONT-vr. Additionally, the following paragraph has been added to the conclusions, “Finally, this study showed that the SDE and ARI influences on the premonsoon vary greatly as a function of model grid spacing. Specifically, SDE- and ARI-induced perturbations to premonsoonal climate were more comparable in the coarse resolution experiments. In the VR experiments, the SDE-induced effects were much smaller than those induced by ARI, with a better simulation of TP snow cover in the VR experiments (Rahimi et al., 2019).”

b. In terms of aerosol simulation, it is a long-standing problem when comparing model grid average variable with single-point measurement. Therefore, the underprediction

here is not very surprising. What’s surprising is that CONT_vr and CONT_un are the same? I thought the high-res will help in capturing some of those aerosol hotspots near city centers. Can you confirm and elaborate?

Reply: Default present-day anthropogenic emission maps from IPCC-AR5 were used in this study. These emissions reside on a 1.9o by 2.5o grid. Global monthly-varying high-resolution emission maps are not available, so we chose to use the default model emissions. Because the same emissions were used in CONT-un and CONT-vr, it makes sense that the VR simulation didn’t outperform the UN experiment in simulating BC. Dust emissions are calculated online at each grid cell. If dust surface observations were more prevalent, then we might expect the VR simulation to outperform CONT-un in simulating dust aerosols due to the stronger subgrid surface winds and the higher resolution erodibility map.

c. Another potential benefit is on reducing the cost of computational resources. The simulation time is bit short, which is subject to the background SST condition (2000s as used in this study). Ideally a multi-decadal background is preferred (50-100), such as those used in Xu et al., (2016). That of course is more computationally expensive but given the advancement in VR (only 3 times more expensive) than the global 1âŮęC model, it is doable. I understand this cannot be addressed in the revision process in this paper, but certainly should be mentioned in the “Conclusion and Future work” section of this paper.

Reply: This is a good suggestion. The addition has been made in the final paragraph, which now reads, “An opportunity exists for these simulations to be conducted without prescribed SSTs, as ocean–atmosphere feedbacks may affect the interseasonal and interannual variability of the monsoon. These feedbacks may depress or enhance the various BCD effects discussed here. To capture the multi-decadal variability of the monsoon, these experiments may also be conducted over longer time periods than considered in this study, as in Xu et al. (2016). Another opportunity exists for the quantification of aerosol-cloud interactions, which were not explicitly quantified in this study.

Additionally, it has been shown that monsoon intensity correlates with precipitation and wavetrain patterns far downstream of the Asian continent (Lau and Weng, 2002). Examining how this telconnection's sensitivity varies with the loading of light-absorbing aerosols may shed light on the importance of pollution in affecting far-field climate. "

(4) On the precipitation changes: Do the inclusion/exclusion of BCD make the precipitation closer to observational values? Since only SST-fixed simulation is used in this study, the BCD effect should not be considered as "climate change effect" but more on the importance of BCD in simulating S Asia monsoon? In that sense, the answer to the question above is critical. And if the answer is yes, it is worth highlighting. The precipitation change should also be expressed in % Can you also separate snow and rain in the precipitin?

Reply: We address this comment in segments for clarity:

i. CONT-vr was shown to significantly outperform CONT-un in simulating precipitation rate and frequency in Rahimi et al. (2019), but still overestimated precipitation compared to both satellite and surface-based measurements. These simulations were ran with prescribed climatology and emissions for the year 2000, and comparisons to satellite and surface-based measurements were generally not simultaneous (i.e. observations were mostly post-2000). Despite this overestimation of precipitation by CONT-vr, the model still can be used to estimate aerosol effects across the region, especially since even coarser resolution models have been used to evaluate these aerosol effects in previous studies (Lau et al., 2010; Qian et al., 2011). With this precedent in mind, we do acknowledge that BCD effects increase CONT-vr's as well as CONT-un's simulated wet bias compared to observations (see Rahimi et al. 2019).

ii. These experiments were not designed to highlight the "climate change effects" of BCD, but rather their in-snow and atmospheric radiative effects on S. Asian monsoon were examined. Specifically, the means by which BCD affects the monsoon are quantified. This is stressed throughout the manuscript and indeed in the title.

iii. We have added discussions of percent changes for precipitation rate in the revised manuscript.

iv. The precipitation examined in this study is the combined liquid+ice precipitation. However, it was found that the change in precipitation by BCD effects was mainly the result of changes in rain, not snow. We clarify this in the second sentence of Sec. 4.3, "Here, we define "precipitation" to be sum of liquid precipitation plus ice precipitation, and we find that changes in total precipitation are driven by changes in liquid precipitation; simulated changes in snow precipitation are minimal (not shown)."

(5) On the Physical mechanism. Why does the atmospheric heating co-locate with the subtropical jet? Why this leads to a stronger low-level jet that brings more moisture from Arabian sea?

Reply: Thank you for this question. The maximum atmospheric heating co-locates with the subtropical jet during MJ (Fig. S9c) because the subtropical jet is a transport pathway for middle-eastern dust into southern Asia. The dust heats the atmosphere within the pathway. Additional heating of this pathway is attributed to BC (see Fig. S10) that is advected northwards from India or originates far upstream of southern Asia.

The stronger WLLJ is the direct result of the aerosol-induced column warming, described in Sec. 5.1. Warming of the column by BCD SDE and ARI across southern Asia causes hypsometric thickening of the tropospheric column, forming an anomalous depression (ridge) in the geopotential field in the lower (upper) troposphere. The vertical vorticity is proportional to the concavity (the Laplacian) of the geopotential field by Eq. (9). By warming the tropospheric column, an anticyclonic (divergent) anomaly is formed beneath a cyclonic (convergent) anomaly, leading to enhance upward vertical motion across the region by Eqs. (13) and (14). It just so happens that the southern branch of the low-level cyclonic anomaly, characterized by easterly anomalies, lines up perfectly with the positioning of the WLLJ that develops from June through September and is a fundamental component to the monsoon. The juxtaposition of this easterly anomaly on top of the WLLJ leads to enhanced moisture transport into southern Asia.

Minor comments

(1) Line 99. Another benefit of using higher resolution is related to snow cover in complex terrain region.

Reply: Yes, this is correct. Lines 201-206 discuss the potential benefits of using the VR model based on performance evaluations discussed in Rahimi et al. (2019). The section of the manuscript reads, "A validation of the simulated meteorology was performed in Rahimi et al. (2019), in which CONT-vr and CONT-un were compared to surface- and satellite-based datasets. They found that there were marked improvements in the simulated temperature, precipitation, and snow coverage across the TP and its southern mountain ranges when using a VR grid. This is important when simulating the SDE, which is fundamentally dependent on the spatial distribution of snow coverage."

(2) Line 102. No need to introduce another acronym (LAM) since you have plenty of those already.

Reply: This is good advice. All instances of this acronym have been removed from the manuscript. Also removed the acronyms CAM-SE and RE.

(3) Fig 8. what's the unit of numbers here?

Reply: The units, W m-2, are listed in the figure title. We have added the units to the figure caption to avoid confusion.

(4) For AOD evaluation, can BC or dust AOD be evaluated separately? I assume most of the AOD is from SO4 which is not directly relevant to the paper.

Reply: BC AOD was not evaluated in favor of BC surface concentrations from surface observations. Dust AOD was evaluated against satellite measurements from CALIPSO and was found to be undersimulated by CONT-un and CONT-vr across the region (not shown). Since dust comprises a large component of the coarse-mode aerosol mass,

and other aerosols were underpredicted, too, we felt that the best way to represent this underprediction was by using coarse-mode AOD measurements from AERONET followed by the evaluation of simulated total AOD via satellite measurements and reanalysis data (Figure 3).

(5) Line 667. BCD burden increases during the spring and summer? I thought BC is highest in winter time?

Reply: BC burdens are largest in the wintertime (not shown), but dust burdens are maximized in March and April. Since dust aerosols contribute to such a large fraction of the total AOD across the region, Fig. 3d,e capture this springtime AOD maximum. This isn't surprising since the monthly AOD distribution is quite sensitive to the local aerosol emissions and prevailing wind patterns.

(6) The "noBCD" experiment is a bit misleading, because BCD can still affect cloud droplet number concentration. So, it should be called as noSDE&noACI.

Reply: Excellent suggestion. We have changed noBCD-vr to noBCDrad-vr and noBCD-un to noBCDrad-un to denote that there are no in-snow and in-atmosphere BCD radiative effects when both BCD SDE and ARI are removed.

(7) Line 196. The separation of those five sub-regions make sense. but can you provide more details on how the boundary are selected in Figure 1c? It seems to me some elevation threshold is used in picking up those regions; if so, please document those in the paper so others can repeat in future studies.

Reply: We have included the elevation threshold information in the text of Section 2.2. The specific paragraph now reads, "For our analysis, we break up southern Asia into 5 distinctive subregions (Figure 1c). We consider most of India separately from the Indo-Gangetic Plain (IGP) to gain a sense of the impacts exerted on the SAM by the TP regional aerosol effects. Across India and the IGP, we only consider land gridcells with elevations lower than 1,200 m and 600 m, respectively. We also divide the TP into the western TP (WTP) and eastern TP (ETP) since other studies (Lau et al., 2010; Qian et al., 2011) have found there to be noticeable differences in simulated aerosol effects between these two regions. For the TP analyses areas, we only consider gridcells with elevations greater than 3,700 m. A fifth region, the TP foothills (TPF), is also considered to explore how BCD effects may impact orographic precipitation. For the TPF subregion, we only consider gridcells with elevations between 400 and 3,700 m."

(8) Line 230. No black cross hatches.

Reply: Nice catch. This should read "blue stars" instead of "black cross hatches." This has been corrected.

(9) Line 235. I suggest giving some brief introduction of those products in the main text and removing supplement materials.

Reply: More information has been added regarding the MACv2 product. Specifically, we indicate that the MACv2 product's AOD values are for the year 2005 climate mean. We add the following sentence, too: "Surface observations from AERONET are used in conjunction with anthropogenic and fire emission plume strengths from CMIP6 simulations to estimate pre-industrial AOD based on present-day AOD."

(10) Line 241. The observational periods of those datasets need to be provided.

Reply: Great suggestion. Observational times are given in a new table, Table 2.

(11) Line 258. The authors should reduce uncommon acronyms (MJ, and later RE, JA) if they are only used for 1-2 times, which hinders the reader's ability to understand.

Reply: This is a good suggestion. We have removed the acronyms CAM-SE, LAM, and RE. The acronyms for MJ and JA are used quite frequently, so they have been left in.

(12) Line 312 - 321. The BC-in-snow discussion is very related to snow cover and precipitation evaluation. Can some model-obs comparison results be shown more quantitively?

Reply: An extensive review of simulated precipitation, snow cover, and snow water equivalent was performed in Rahimi et al. (2019). Specifically, it was found that CONT-vr significantly outperformed CONT-un in simulating these variables. In our current study, we found that CONT-vr reversed the sign and reduced the magnitude of the mean CONT-un in-snow BC bias (see Sec. 3.3). Since the atmospheric BC was undersimulated by both CONT-vr and CONT-un, the differing performance biases were probably attributed to differences in the simulated snow amount, for which CONT-vr demonstrated more skill. The simulated improvements in in-snow BC are also most certainly related to differences in simulated elevation, dry & wet deposition rates, and precipitation (see Fig. S2).

(13) Fig 5. (e), (f) sub title, I suggest removing of the '-' sign. Kind of misleading.

Reply: Excellent suggestion. Fixed this problem in Figures 5, 7, 10, 11, S3, and S8.

(14) Line 682-684. I do not see the logic in this argument.

Reply: The sentence previously read, "Across the TPF, precipitation enhancements due to BC SDE are comparable in magnitude to BCD ARI, indicating that BCD effects may be enhanced over regions where the TP can act as an elevated heat source." The TPF was the only subregion of the 5 chosen in which SDE brought forth rainfall anomalies comparable in magnitude to those brought forth by ARI. One possible explanation, pointed out in Sec. 5.2, paragraph 2, is that snow darkening warms the air across the TPF. A horizontal density gradient is introduced between this air and that of the free atmosphere further away from the TP, as only the air over the TPF/TP is heated directly by snow darkening. Baroclinic vorticity is generated due to this anomalous density gradient with anabatic rising motion occurring over the TPF. This anomalous rising vertical motion could explain why SDE precipitation enhancements over the TPF are comparable to those induced by ARI. Since this is only a proposed mechanism for the comparability of precipitation anomalies across the TPF, and it is elaborated more in Sec. 5.2, it is only briefly mentioned in the conclusions. The explicit study of this mechanism for precipitation enhancement in the TPF remains the focus of future work. The sentence has been reworded to read: "Across the TPF, precipitation enhancements due to BC SDE are comparable in magnitude to those of BCD ARI, as TPF surface heating adjacent to a cooler free atmosphere south of the TP initiates an anomalous anabatic circulation through anomalous density gradients whose rising branch is located over the TP." This solenoidal circulation would not be possible across the WTP, ETP, IGP, or India, as these regions are not characterized by strong terrain upsloping.

(15) Line 731. "These feedbacks may depress or enhance. . ." A supporting study is Xu and Xie (2015, ACP), in which the prescribed SST and coupled simulation were contrasted in detail.

Reply: Thank you for the reference. It, as well as Wang et al. (2017) have been added as references after this statement.

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