Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-195-RC1, 2019

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.

Anonymous Referee #1

Received and published: 6 May 2019

Review of "Quantifying snow-darkening and atmospheric radiative effects of black carbon . . ." by Rahimi et al. for ACP

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.

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.

(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?

(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.

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?

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 1°C 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.

(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?

(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?

Minor comments:

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

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

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

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.

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

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

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.

Line 230. No black cross hatches.

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

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

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.

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?

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

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-195, 2019.