

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2022-164-RC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2022-164

Anonymous Referee #2

Referee comment on "Effect of dust on rainfall over the Red Sea coast based on WRF-Chem model simulations" by Sagar P. Parajuli et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-164-RC2>, 2022

This paper investigated the effects of dust on precipitation over the Red Sea coast and explored the physical key mechanism underneath. The findings and scientific significance of this paper were clearly stated. In particular, the interactions between the dust, sea-breeze circulation, and the feedback on the rain enhancement/suppression in a region with high dust load were examined. This study will certainly contribute new aspects to the field of cloud-aerosol-precipitation interactions. However, the paper needs major revision before publication. Below are my comments in detail.

The model-observation comparison requires some further justification/explanation. In the obs-model comparison plots, we could see an evident and consistent overestimation of model results. For example, Fig. 4 the wind speed is consistently stronger during the daytime in WRF-Chem. This time (around 15:00 local time) was also when the rainfall peaked and CCN-sea-breeze circulation interactions were strong. In Fig 7-8, the model overestimates the CCN concentration in all cases. The statements of "were reasonably consistent" and "produced reasonably well" throughout the manuscript were not supported by the comparison in the plots. A more quantified description/analysis is needed to support the model is comparable to observation (e.g., the relative difference of the wind speed). One suggestion is to conduct sensitivity studies to quantify how this would affect the conclusion of this study (e.g., use other driving datasets to test the impact of wind field, change the CCN concentrations, etc.).

Line 382: "The baseline experiments is calibrated against MODIS/AERONET AOD data": this sentence is a little misleading, as the study mainly compared the model output (wind speed, CCN, precipitation) against the observation, and only the conditions related to dust (dust emission fractions and dust size fractions) were adjusted. Those details should be mentioned in this statement to avoid ambiguity.

Line 474: "identical" is a very strict description. The profiles of the model and observations were not identical.

Line 499: "The southern areas of the domain received more rainfall due to the presence of higher mountains". This was evident in the model results (Fig.5a) but not so in the IMERG observations (Fig. 5b). More evidence is needed to support this statement. We could see the precipitation was stronger in the North in the observation and weaker in the South. But the opposite was found in the model.

Line 527: "This order of difference, although large, is reasonable for microphysical parameters given the high uncertainty in their parameter". It is not "reasonable" to make this assumption and is not a justification from a scientific perspective either. Instead, this should be seen as a limitation, as it only adds the uncertainty on top of the inherent uncertainty from current microphysical schemes. I suggest stating that as a limitation instead of making justification for this difference.

Line 650: "at different grid points", "at different locations" will convey a more physical meaning.

Line 770: "It would be beneficial to evaluate the effectiveness of cloud seeding through regional modeling in the areas of interest as done in this study." I believe there were a few campaigns over the UAE area that have done such investigations. (e.g., Fonseca et al., 2022).

Comments on the figures:

Fig. 1 shows the domain setup of the simulation. I suggest adding one zoomed-in map of d03 with the topography map (terrain height). Orographic effect plays a role in the sea-breeze circulation and affects the precipitation as can be seen in the analysis. It is also helpful to add KAUST and Abha in the d03 terrain map as an overview of the two key locations and their topography, as they will be frequently mentioned later. The author only showed KAUST in some of the figures, not Abha.

Fig. 6: a histogram/step may be better to show the modeled ASD.

Fig. 9-10 should be describing the same time point, but one said "at the time of rainfall maxima", one said "15:00". Either mention both info or use consistent description for a better reference.

Fig 11: I suggest using the same color scale for all plots for a better comparison among the three simulations.

Fig. 13 (d,e,f) & Fig 14(c,d): the colorbar should not be there. The definition of the dots in the figure should be mentioned in the caption. (Author mentioned it in Line 613 but it's difficult for readers to find).