ACP-2021-597: Convection-Aerosol Interactions in the United Arab Emirates: A Sensitivity Study

By Fonseca et al. (2021)

Reply to reviewer #2's comments:

This manuscript attempts to use the WRF model to assess the convection-aerosol interactions over the UAE in summer. Unfortunately, there are some major flaws with the manuscript in its present form, and I have to recommend that the manuscript be rejected.

REPLY: The authors would like to thank the reviewer for his/her valuable comments/suggestions, which helped to improve the quality of the manuscript. Below we address the reviewer's queries one by one, both the major flaws and the specific comments, highlighting in the text where changes were made.

There are three critical issues with the paper.

1. The first is a lack of clear scientific question and focus. The title states that the topic of the paper is convection-aerosol interaction, but which part of that interaction is the key scientific question here? The introduction very broadly touched upon the impacts of ARI and ACI on the lifetimes and precipitations of MCS, but no key scientific question is brought forth. The abstract even mentions the impacts of nudging in the outer model domains, which further confuses the reader. Also, there are way too many sensitivity experiments. What is the purpose of testing (very) different aerosol composition assumptions in the model, if the point was to assess ACI in a particular case?

REPLY: We thank the reviewer for his/her comment. First and foremost, we have modified the title of the paper to reflect its main objective: investigate the sensitivity of the model-predicted convection to the aerosol loading and aerosol properties/composition. Following the reviewer's inputs, we have shortened it by (i) relegating the nudging discussion which we agree is not an essential part of the manuscript to an Appendix (lines 258-288) and therefore reduced the number of sensitivity experiments, and (ii) simplifying the discussion of the results focusing on the main findings (lines 790-853). We have also added a paragraph to the Discussion and Conclusions section on our recommendations for modelers targeting summertime convective events in this region, a very important take-home message (lines 837-853). We believe the manuscript's readability has improved and it has a clear focus now, with the discussion on the sensitivity to the aerosol composition fitting within the study's scope.

2. Secondly, the methodology used in this study is not appropriate for the question it appears to want to address. If the purpose is to investigate the 'interaction of convection and aerosol', then in the model, aerosols and cloud microphysics and dynamics should be allowed to 'interact' in a physically-realistic or reasonable way. Instead, the authors used a WRF model and implemented an 'aerosol-aware' cloud microphysics, which really does not allow aerosol and convection to 'interact' with each other. The use of assumed aerosol loading as initial condition and then allow them to be advected in no way physically represent the locations and strengths of the aerosols relative to the convective systems, as evidenced in Figs

2 and 5. Many of the assumptions (e.g., 30% dust in the radiation calculation; conversion of Ns to PM10; etc) are simply wrong.

REPLY: We thank the reviewer for raising this issue and apologize for not having explained in the text how the Thompson-Eidhammer aerosol-aware scheme works. The equations below which describe the temporal evolution of the number concentration of "water friendly" (N_{wfa}) and "ice friendly" (N_{ifa}) aerosols, and are taken from Thompson and Eidhammer (2014), their equations (3) and (4), respectively.

$$\frac{dN_{wfa}}{dt} = -\begin{pmatrix} rain, snow, graupel \\ collecting\ aerosols \end{pmatrix} - \begin{pmatrix} homogenous\ nucleated \\ deliquesced\ aerosols \end{pmatrix} - (CCN\ activation) + \begin{pmatrix} cloud\ and\ rain \\ evaporation \end{pmatrix} + \begin{pmatrix} surface \\ emissions \end{pmatrix} \quad (1)$$

$$\frac{dN_{ifa}}{dt} = -\begin{pmatrix} rain, snow, graupel \\ collecting\ aerosols \end{pmatrix} - (IN\ activation) + \begin{pmatrix} cloud\ ice \\ sublimation \end{pmatrix} + \begin{pmatrix} surface \\ emissions \end{pmatrix} \quad (2)$$

The most relevant aspects of the scheme discussed in Thompson and Eidhammer (2014) and, for "ice friendly" aerosols also in Su and Fung (2018), are summarized below:

- The nucleation of cloud droplets from N_{wfa} is done through a lookup table with the activation fraction a function of parameters such as the WRF-predicted temperature, updraft speed, number of available aerosols, and predefined values of the hygroscopicity parameter and the aerosol's mean radius;
- Once nucleated, the aerosols are removed from N_{wfa}, third term on the right-hand-side (RHS) of equation (1), but can be restored via hydrometeor evaporation, fourth term in equation (1). Aerosols can also be removed by precipitation scavenging, first term in equations (1) and (2);
- For "water friendly" aerosols, and when a climatological-based distribution is employed, a constant surface emission forcing is added in the lowest model layer based on the starting near-surface aerosol concentration. A similar contribution is not considered for the "ice friendly" aerosols in the present version of the scheme, i.e. the last term on the RHS of equation (2) is set to zero;
- The nucleation of dust particles into ice crystals occurs in the presence of supersaturation with respect to ice. Depending on the relative humidity with respect to water, condensation, immersion freezing (i.e. ice nucleation by particles immersed in supercooled water) and deposition nucleation (i.e. formation of ice from supersaturated water vapour on an insoluble particle without the prior formation of liquid) can occur. These processes are accounted for by the second term on the RHS of equation (2);
- The freezing of homogeneous nucleated deliquesced hygroscopic aerosols is accounted for, with the decrease in N_{wfa} represented by the second term on the RHS of equation (1), while the freezing of existing water droplets is more effective in the presence of higher amounts of dust aerosols. Cloud ice sublimation returns the aerosols to N_{ifa}, third term on the RHS of equation (2).

As the reviewer can see, aerosols and convection *do* interact with each other in the model, as besides transport we account for their emission and deposition. We have now made this clear in the text (lines 258-

288). While we agree that some approximations and simplifications have been made in the scheme, this "aerosol-aware" version of the Thompson cloud microphysics scheme has been widely used for convection-aerosol interaction studies such as in Alizadeh-Choobari and Gharaylou (2017). It has the advantage of being computationally cheaper than more sophisticated models like the WRF model with chemistry (WRF-Chem; Grell et al., 2005), as noted in the text (lines 221-224). This makes it possible to implement it in operational forecasts which is particularly pertinent for regions such as the UAE located next to major aerosol sources. We thank the reviewer again for his/her comment on this.

3. Thirdly, because the manuscript is lacking focus, it is also extremely long, without apparent need to be that way. For example, the verification diagnostics presented in section 2.4 are fairly standard; there is probably no need to elaborate. The discussion on the effects of nudging and the effects of assuming much of the aerosols to be carbonaceous is very confusing and not related to the topic at hand.

REPLY: We thank the reviewer for his/her comment, and fully agree that the manuscript is very long and needs to be shortened with the message sharpened. To that end, and following his/her suggestion, we have relegated the discussion of the nudging effects to an Appendix (a total of nine WRF simulations are now considered), and have added to the Discussion and Conclusions section a paragraph on the take-home messages to potential future readers of this study (lines 837-853). We believe the simulations on the sensitivity to the aerosol properties should remain in the text, as they are an integral part of this study (the focus is both on the aerosol loading and composition as now clearly mentioned in the text/title) as noted in the reply to the reviewer's critical comment #1. Likewise, some of the verification diagnostics employed are not standard (e.g. the normalized error variance, α, and the variance similarity, η), so they have to be defined in the text. We do so first mathematically and then with a sentence to explain what information the skill score conveys and the optimal value. In any case, we have relegated Table 3 as supplementary material. We have also simplified the discussion of the results to make the manuscript easier to follow. Examples of this can be seen in lines 504-518, 668-683 and 790-835. We would like to thank the reviewer again for raising this issue.

Specific comments:

1. Lines 12-13: "Both an idealised and ... are considered": This sentence is unclear. Please revise.

REPLY: We thank the reviewer for raising the issue and have rephrased the phrase accordingly (lines 13-14).

2. Lines 24-28: "In particular, ... 51 W/m2.": This sentence is extremely long and unclear. Please revise.

REPLY: We agree with the reviewer that this sentence is indeed very long. In the revised version of the manuscript we broke it into two and rephrased it for clarity (lines 20-24).

3. Line 28: Not sure what "the former" and "the latter" refer to.

REPLY: We are referring to the simulations where the aerosol composition and aerosol loading are changed, respectively. We have rephrased the sentence in the revised version of the paper (lines 24-25).

4. Lines 51-52: The increased number of smaller cloud droplets mostly lead to more scattering (hence higher albedo and optical depth). This is not the same as 'reducing the radiative window'. Also, this statement is missing reference.

REPLY: We have rephrased the sentence accordingly and added a reference (lines 49-51).

5. Line 54: Albrecht effect: missing reference.

REPLY: We have added a reference for the Albrecht effect (line 53).

6. Line 248: "Nwfa and Nifa ... evolve during the course of the model integration": How is this achieved? Do you simulate the emission/transport/deposition of the Ns? Or do you prescribe how N changes with time? If the latter, how do you ensure that this correctly represents the response of N to meteorology. More importantly, how do you know that you are not forcing the cloud microphysics to do things that you wish to see?

REPLY: Following the reply to the reviewer's critical comment #2, we now make it abundantly clear in the text how the "aerosol-aware" version of the Thompson scheme works (lines 258-288). Yes, the model simulates the emission and deposition of the aerosol number concentration besides its transport, all done in a physically-sound way.

7. Lines 249-250: "...dataset on a monthly time-scale ... downloaded from the model's website": What is this dataset based on? Is it based on a model calculation or some kind of satellite data inversion?

REPLY: We thank the reviewer for raising this issue and apologize for the lack of clarity in the text. This is the 7-year (2001-2007) climatological dataset we refer to in lines 230-232, which is obtained from a global model simulation in which aerosols are emitted by natural and anthropogenic sources are modelled using a bin scheme. This dataset is therefore a model product but the predicted aerosol optical depth and Angstrom exponent compare well against satellite estimates in particular in this region, as discussed in Colarco et al. (2010). We have now stated this in the text (lines 249-254).

8. Line 252: So Nwfa and Nifa are first set with initial conditions and then allowed to advect and diffuse? How do you ensure that the transported Ns are realistic? Also, the scavenging of Ns (both hygroscopic and non-hygroscopic) by precipitation are not considered?

REPLY: As stated in the reply to the reviewer's critical comment #2, and besides the transport, the emission and deposition of aerosols are accounted for in the model, including the scavenging due to precipitation. This is now clearly stated in the text (lines 258-288).

9. Lines 249-255: The description of the aerosol settings in the sensitivity experiments is unclear. I have a hard time following what assumptions are made. Please consider revising.

REPLY: We thank the reviewer for raising this issue. In the referred sentences we are mixing the idealized set up with the climatological initialization for aerosols which is indeed confusing. We have revised the text for clarity (lines 234-256).

10. Line 257: Does the number of non-hygroscopic aerosol affect the number of ice nuclei? This is not a default option in WRF. What parameterization is used to describe this sensitivity?

REPLY: Yes, it does, as noted in the reply to the reviewer's critical comment #2 and given by the second term on the RHS of equation (3). This option is only available in the Thompson-Eidhammer "aerosol-aware" cloud microphysics scheme which we employ in the simulations discussed in this study. We now describe the scheme in more detail in the text for clarity (lines 279-283).

11. Lines 264-265: "...assumes a mixture of 70% water soluble and 30% dust-like aerosols": Is this a reasonable assumption for this case, where most of the aerosols were dust? More importantly, is this assumption consistent with the prescribed hygroscopic/non-hygroscopic aerosol numbers?

REPLY: We thank the reviewer for raising this issue. We would like to stress that in this manuscript we aim at investigating the sensitivity of the model-predicted convection and atmospheric fields to the aerosol loading and composition, so as to issue recommendations to WRF modelers as to which set up may work best for a given simulation. Our goal is *not* to mimic the observed aerosol conditions, as we do not have relevant measurements that will allow us to do so.

In the comment above, the reviewer is referring to the assumptions made regarding the aerosol properties, in particular the single-scattering albedo, asymmetry factor and Angstrom exponent. Here we simply tested the three options available by default in WRF, denoted as aerosol models in the text, with one being that referred by the reviewer. A customization of the aerosol properties for our event is not possible for the aforementioned reasons. Regarding the reviewer's second question, we agree that the fraction of hygroscopic and non-hygroscopic aerosols is in closer agreement with the assumptions made in some of the aerosol models but not so much in others. In any case, we would like to note that, at a given grid-point, the percentage of each varies during the simulation, as seen in Figs. 5a-b, meaning that no one aerosol model works well all the time. Hence, we believe it is acceptable to test the different options for the event targeted in this study. We hope the reviewer agrees with us on this. Nevertheless, we have noted the caveats in the text (lines 307-311).

12. Lines 278-281: Is the effect of nudging in the outer domains one of the scientific questions you want to address in this study? If not, and if nuding is necessary to capture the large-scale atmospheric dynamics, then it should be included in all the key experiments. Otherwise there are simply too many experiments without a clear, key scientific question.

REPLY: We thank the reviewer for raising this issue. As stated in the reply to the reviewer's critical comment #3, we agree that the paper is too long and lacks a clear focus. Following his/her comment, we have relegated the nudging experiments to Appendix A (lines 1324-1379), and focused on the sensitivity to the aerosol loading and properties in the main text, the central goal of the study. We believe the paper is more targeted now and easier to follow.

13. Lines 312-314: Why use MERRA-2 aerosol product for this study? Has the AOD in MERRA-2 been evaluated over this part of the world, particular since the surface is bright?

REPLY: Yes, MERRA-2 has been found to perform well in the Middle East when compared to satellite-derived and ground-based measurements as concluded e.g. by Roshan et al. (2019) and Ukhov et al. (2020). We have also compared the 3-hourly MERRA-2 PM10 with that observed hourly at air quality stations and found a good agreement between the two, despite the still coarse spatial (0.625° × 0.5°) resolution of the reanalysis dataset and inherent model biases. The reviewer can see this in Fig. R1 below for two stations, one in the northeastern (Burairat) and another in western (Gayathi School) UAE, on 14 August 2013.

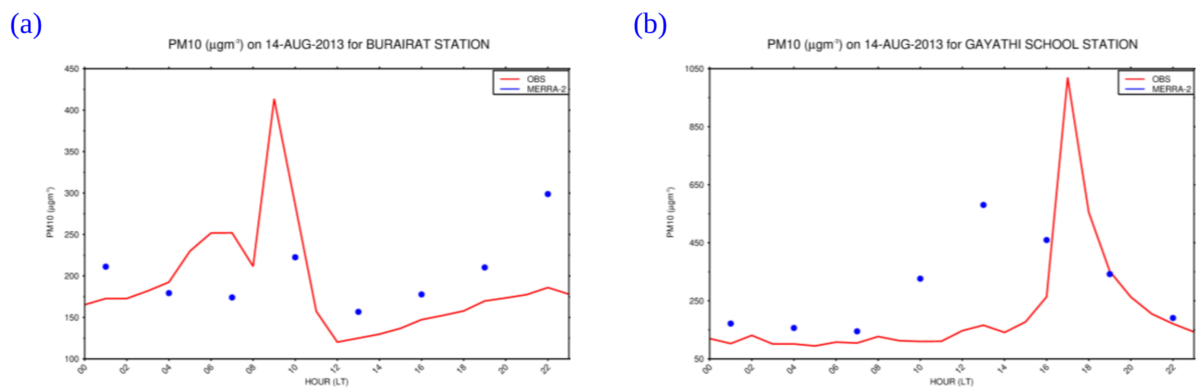

Figure R1: Observed (red line) and MERRA-2 (blue dots) PM10 concentration (μg m^{-3}) for the (a) Burairat station in northeastern UAE and (b) Gayathi School station in western UAE on 14 August 2013.

As AOD estimates from ground and satellite assets exhibit gaps and missing data on this day due to the extensive cloud cover (Figs. 2a-c), we decided to use the referred reanalysis dataset that explicitly accounts for aerosols and their interactions with the climate system and is found to perform well in the region. We have now stated this in the text (lines 347-349 and 504-511).

14. Lines 351-358: In fig2a-c, it appears that the dusts are mostly in the Northeastern part of the domain, but the MERRA-2 AODs are mostly in the central/southern part of the domain. Which one is more accurate and how do you reconcile the discrepancy? More importantly, which one is more consistent with the assumed Ns in the simulation?

REPLY: We would like to note that in the RGB images, Figs. 2a-c, and because of the extensive cloud cover, one cannot see the dust over the UAE, which can be emitted in association with cold pools as is common in desert regions (e.g. Bou Karam et al., 2014). Therefore, a direct comparison with MERRA-2 (and WRF) is not possible. While the AOD in MERRA-2 is lower over Iran when compared to the UAE, it is non-zero in the regions shaded in pink in the RGB images, so the two products are not necessarily inconsistent. A comparison of Figs. 5a-b with Figs. 2g-i shows that WRF also predicts lower amounts of dust over Iran as MERRA-2, while over the UAE it underestimates the dust loading compared to the reanalysis dataset, as seen in Fig. 6d. This is discussed in the text (lines 504-518).

15. Fig 2a-c: How are these figures colored? If this is indeed 'RGB' (i.e., real color) images, why would the dust be pink and the clouds orange/brown/black? Clearly, some other type of of processing has been applied.

REPLY: We thank the reviewer for raising this issue. Indeed, these are not raw RGB images but processed (false-colour) images to highlight specific features such as dust, sand and clouds in contrasting colours, following Banks et al. (2019). False-colour RGB images such as these are widely used in aerosol-related studies such as Francis et al. (2020). We have stated this in the text (lines 338-339).

16. Section 4.1: The aerosol loading simulated here are neither consistent with the RGB plots or the MERRA-2 AOD shown in Fig 2.

REPLY: As highlighted in the reply to reviewer's specific comment #14, we cannot directly compare the RGB images with the WRF and MERRA-2 plots. Besides, the dust over Iran in the RGB plots is not necessarily inconsistent with the MERRA-2 AODs in Fig. 2 and the WRF plots in Figs. 5a-b. We do accept that there are inconsistencies between MERRA-2 and WRF, and discussed them in the text in section 4.1 (lines 504-518).

17. Figure 5: The labeling is extremely confusing. Some subplots are not labeled, and which ones are (c) and (d)? The caption says: "The aerosol concentration in panels (c) and (d) is scaled as in panels (a) and (b)", which I do not understand.

REPLY: We apologize for the confusing caption of Figure 5 and have rephrased it in the revised version of the manuscript. Panels (a) and (b) have six subplots each and panels (c) and (d) have two subplots each which have a title but not a label so as to make the interpretation of the figure easier. Nevertheless, we have improved the readability of the figure in the updated version of the paper.

18. Section 4.1: The conversion of WRF 'simulated' aerosol loading to PM10 is inappropriate. The Ns here are not realistic aerosol loading. They are first prescribed as initial condition and then allowed to be transported. In no way were the Ns physically related to dust in the model.

REPLY: As noted in the reply to the reviewer's critical comment #2, we believe the aerosol concentration used here is realistic, as besides advection we also account for emission and deposition. In any case, we agree that the comparison between the WRF "ice friendly" aerosols and the observed PM10 at the location of the 11 weather stations is not correct as several assumptions have been made, some of which are questionable such as (i) considering that PM10 is just due to the dust in the model, (ii) comparing the WRF model-layer concentration with that observed at a single height/point for pollutants that exhibit a high temporal and spatial variability, and (iii) taking a fixed radius and density for the PM10 particles. We have taken out this comparison from the article, leaving just the evaluation of the WRF-predicted AOD with that given by MERRA-2 (lines 511-518), a more straightforward comparison.

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