Review of "Investigating the impact of subgrid-scale aerosol-cloud interaction on mesoscale meteorology prediction" by Zhang et al., submitted to Atmospheric Chemistry and Physics (ACP)

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This report contains general, major, and specific comments from this reviewer on the manuscript.

A summary of the manuscript and general assessment:

Recommendation: Major Revision

This study implemented an update of the Kain and Fritsch subgrid convection parameterization by incorporating cloud microphysics with aerosol-cloud interaction (ACI) into a mesoscale atmospheric chemistry model, CMA_Meso5.1/CUACE. The performance of the updated model was evaluated in two types of tests. The first test compared model simulations with and without the update of the subgrid convection parameterization, and investigated the differences by comparison with satellite and ground-based observations for June 2006. Overall, the update reduced various model biases, primarily through improved representation of the modeled cloud for the atmospheric radiation calculation. The second test configured simulations with and without anthropogenic emissions for several days in late June, when heavy surface precipitation was observed over southern China. The inclusion of anthropogenic emissions resulted in a reduction of surface precipitation due to a lower auto-conversion rate of cloud water to rain and less water vapor available for grid-scale condensation.

This study is within the aims and scope of Atmospheric Chemistry and Physics (ACP), specifically the subject for "Aerosols, Cloud and Precipitation", the research activity for "Atmospheric modelling", the altitude range for "Troposphere", and the science focus on "both Chemistry and Physics".

As mentioned in the text, the subgrid ACI effects are often overlooked in the modeling studies for ACI. It is scientifically significant to investigate this problem by implementing the effects on the subgrid convection parameterization and conducting simulations. The direction and approach of the research is reasonable and acceptable. However, I think that the current manuscript contains a number of misinterpretations of the results and major misleading descriptions, especially for the first set of simulations. These issues should be adequately addressed and corrected before acceptance for publication. Detailed comments are provided below.

Major comments:

1. Literature review of previous studies for modeling subgrid ACI effects

The introduction section needs to include more reviews of previous studies for aerosol-aware sub-grid convective parameterization. For example, Grell and Freitas (2014) is a widely used and, to my knowledge, the most cited work for sub-grid ACI parameterization, although the approaches for microphysical representation are very coarse compared to Song and Zhang (2011) and Glotfelty et al. (2019, 2020). It would be better to describe what is new and novel compared to these previous studies, in order to highlight the significance of this study.

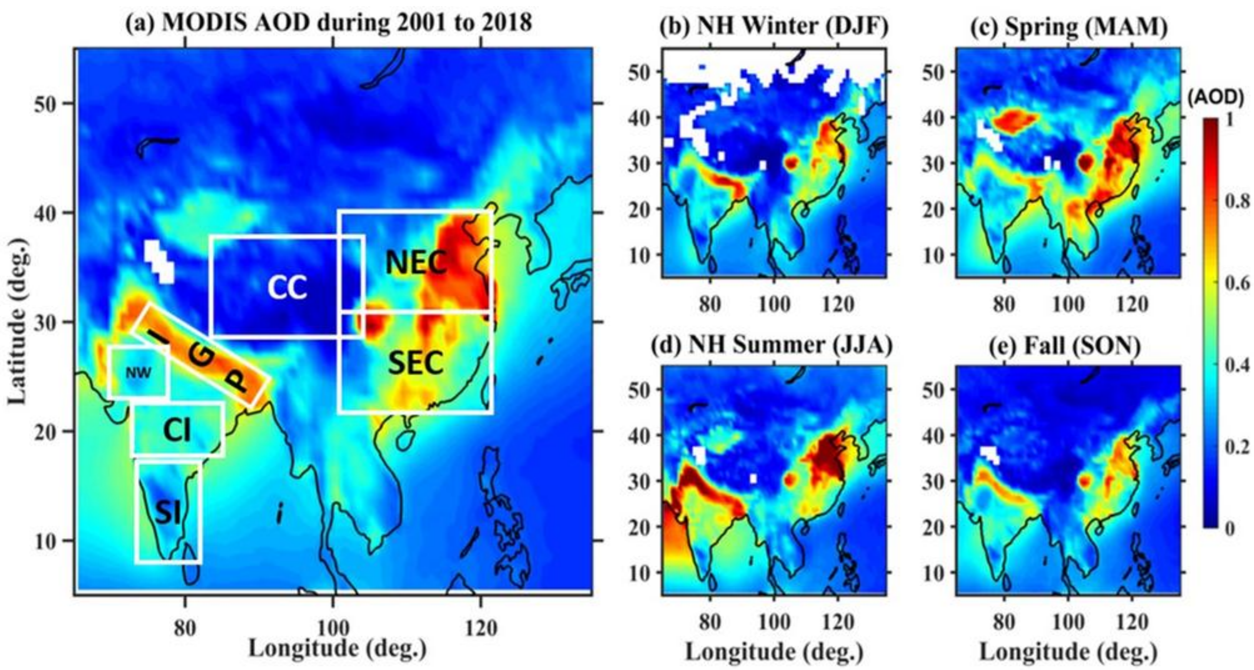
Grell, G. A., & Freitas, S. R. (2014). A scale and aerosol aware stochastic convective parameterization for weather and air quality modeling. Atmospheric Chemistry and Physics, 14(10), 5233-5250.

2. Parameterizing sub-grid updraft (or sub-grid supersaturation) for ARG2000

The current descriptions of subgrid ACI parameterization are missing important information, especially how to parameterize the subgrid updraft and its variability that needs to be entered into ζ and η in the ARG2000 scheme. I think this is the most difficult part of implementing cloud microphysics, considering the effects of aerosol nucleation on cloud droplets, such as the ARG2000 scheme, into subgrid convective parameterization. On line 183, "Meteorological factors include atmospheric vertical velocity, temperature, etc., which can be provided in real time by the CMA_Meso5.1 model". Since the grid-scale vertical velocity cannot be used here as the subgrid-scale vertical velocity for the ARG2000 scheme, the subgrid-scale vertical velocity needs to be prepared somehow. Song and Zhang (2011) and Glotfelty et al. (2019) use different approaches to parameterize the subgrid scale vertical velocity. Please clarify how to parameterize the subgrid-scale vertical velocity in this study, and add detailed descriptions in the text.

3. VIIRS AOD comparison (Section 5.1 and Figure 4)

I think there are possible misinterpretations of the VIIRS AOD data. First of all, the VIIRS AOD is "clear sky" AOD because COT is generally much higher than AOD, so retrieval algorithms for typical space-borne radiometers cannot calculate "cloudy sky" AOD. Thus, my first question is whether the simulation AOD in Fig. 4d is really clear-sky AOD or all-sky AOD. If the simulation AOD is all-sky AOD, then it causes an underprediction because cloudy sky AOD could be lower than clear sky AOD due to wet scavenging by precipitation. Second, I cannot believe that the real clear-sky AOD over South China is too low, such as 0 ~ 0.04, as shown in Fig. 4c. This strangely too low AOD is clearly inconsistent with the surface PM2.5 data in Fig. 4a as well as other observational data, such as the MODIS AOD climatology shown below. I think that clear sky AOD cannot be calculated from the satellite observations over the region for that month, because the region was covered by clouds on almost all days, as shown in Fig. 5a. Thus, I just wonder if the actual VIIRS AOD is "undefined" rather than 0 or really low values. Please check the downloaded data products and the process for plotting.



Adapted from, Ratnam, M.V., Prasad, P., Raj, S.T.A. et al. Changing patterns in aerosol vertical distribution over South and East Asia. Sci Rep 11, 308 (2021). https://doi.org/10.1038/s41598-020-79361-4

4. NO-ACIsub vs. ACIsub

This is the most important problem I ask the authors to address. As long as I read the whole section 5.2 for the first set of experiments, I think the drastic changes in the simulation results between NO-ACIsub vs. ACIsub (Table 3) come from the inclusion of the subgrid-scale cloud in the calculation of the atmospheric radiation processes, rather than from the inclusion of the aerosol effect for the subgrid-scale cloud microphysics. Therefore, I feel that the current descriptions of the difference between the two experiments, such as Table 3, may be misleading or exaggerated. If the authors want to show and discuss the result changes in cases with and without the subgrid-scale ACI effects, the results should be presented in a way that disentangles the two components, the inclusion of the subgrid-scale cloud in the calculation of the atmospheric radiation processes and the inclusion of the aerosol effect for the subgrid-scale cloud microphysics. I am aware that SZ2011 eventually uses ARG2000. However, this problem should be critically addressed because it is the core of the research topic and goal.

5. CERES data comparison in 5.2.2

I am really confused with what is going on in this subsection. Please redo the work. The CERES data products provide the top-of-atmosphere (TOA) upward radiation fluxes, not the surface downward radiation fluxes (how satellite sensors can directly measure the surface downward radiation fluxes...). Thus, Figs. 6a and 6d should show the upward TOA shortwave and longwave radiation fluxes. I have no idea which TOA upward or surface downward fluxes from the simulations are shown for the rest of the panels. The selections of the color table and contour ranges of Fig. 6 are quite messy, which further hinders my understanding.

6. Sampling timing of the simulation results for comparison with the daily products from polar orbiting satellites

The VIIRS and CERES sensors on the SNPP satellite measures a location only twice (daytime and nighttime) per day due to the polar-orbiting so that their daily products are based on the observed values at specific local time (daytime only or both) within a day. I wonder if the authors actually sampled the simulation results for the comparison at specific timing on the days as much as similar to the satellite flying timing. This is often important, especially for validation of cloud, because cloud and precipitation lifecycles have a strong diurnal cycle in summer as shown in Fig. 15d.

7. R difference between ACIsub-DC and CACIsub-DC in Section 5.3

In Section 5.3, the explanation of the mean bias of surface precipitation sounds reasonable. However, I am not convinced how the authors argue that R is also improved. The 0.03 between 0.7 and 0.73 of R is, in my opinion, almost the same or kind a level of random error noise. If the authors want to argue the improvement of R, please add some follow-up descriptions on the mechanism for improving R.

Specific comments:

Abstract: Please refine the abstract to help readers understand the conclusions of the study, rather than just listing the result changes in % values.

Line 158: The equation looks to be missing some components.

Line 228: "with a forecast time of 24 hours", does this mean a 24 hour forecast loop similar to Zhang et al. (2022)?

Figure 6: Please change color map and scale.

Line 315: Please clarify that the comparison with surface (ground-based) station data for SDSR starts from here.

Line 415: "The related statistical indicators also show that the simulation performance of precipitation is comparable to other models or studies (Table 5)." I do not understand what is meant here, especially "other models or studies". Please clarify.

Line 474: "Notably, the decreased cloud droplet number concentration within some YRD regions may be related to changes in environmental supersaturation due to thermodynamic perturbations (Fan et al., 2016; Glotfelty et al., 2020)." I do not understand what is meant here. Please specify which parts of the two publications I should read to understand.

Grammatical problems:

Abbreviations are sometimes not fully spelled out the first time they appear. Please check again.