

Dear Reviewer:

Thank you for careful comments. These comments are all valuable and very helpful for revising and improving our paper, as well as the important guiding significance to our researches. We have studied the comments carefully and have made corrections which we hope meet with approval. The Reviewer's comments are in blue and our responses are in black. Revised portions are marked in red in the marked-up manuscript. The main corrections in the paper and the point-to-point responses are as following:

Response to Reviewer #1:

Main 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.

Response: Accept. We have added more reviews of studies on subgrid-scale ACI in the introduction section, including studies of Grell and Freitas (2014), Lohmann (2008), Song and Zhang (2011), Lim et al. (2014), Glotfelty et al. (2019). Relevant contents have been added to the revised manuscript in lines 80-90.

Additionally, we emphasize the uniqueness and innovativeness of this study compared to these previous studies. In terms of model development, this study completes the full coupling of the double-moment cumulus scheme with the aerosol module and the short-wave radiation scheme in the atmospheric chemistry model, which makes the emissions-aerosol-subgrid-scale cloud-radiation/precipitation processes are closed, allowing the effects of subgrid-scale ACI on radiation and precipitation to be

investigated in more realistic aerosol levels; in terms of model application, this study more systematically evaluates the impacts of the treatment of subgrid-scale cloud microphysics and radiation feedback on multiple predicted meteorological factors, rather than limiting them to cloud and precipitation. Relevant contents have been added to the revised manuscript in lines 102-105.

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 realtime 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.

Response: Accept. We have added the description of the parameterization of subgrid-scale vertical velocity in lines 221-235 in the revised manuscript, the details of which are shown as follows:

The subgrid-scale vertical velocity (w_{sub}) is determined by the updraft kinetic energy (K_{sub}):

$$w_{\text{sub}} = \sqrt{2K_{\text{sub}}} \quad (5)$$

$$\frac{\partial K_{\text{sub}}}{\partial z} = -\frac{v_w}{M_w}(1 + \beta C_d)K_{\text{sub}} + \frac{1}{f(1+\lambda)}g \frac{T_{wu}-T_{we}}{T_{wu}} \quad (6)$$

$$T_{wu} = T_u(1 + 0.608Qu - Qr - Qi - Qc - Qs) \quad (7)$$

$$T_{we} = T_e(1 + 0.608Qe) \quad (8)$$

where v_w is the larger of entrainment or detrainment mass flux and M_w is the convective updraft mass flux in the Kfeta scheme. The β , C_d , λ , and f are constants, which are set to 1.875, 0.506, 0.5, and 2. The g is gravitational acceleration. T_{wu} and T_{we} are the density

temperature of updraft and environment, which can be solved by equations (7) and (8). In equation (7), T_u is the temperature of updraft, Q_u is the specific humidity of updraft, and Q_r (Q_i , Q_c , or Q_s) is the rain (ice, cloud, or snow) water mixing ratio. In equation (8), T_e is the temperature of environment and Q_e is the specific humidity of environment. The calculation of subgrid-scale vertical velocity refers to the method in Section 2.2 of the study by Song and Zhang (2011). The minimum value of the subgrid-scale vertical velocity is set to 0.5 m s^{-1} at the cloud base and the maximum value is less than 20 m s^{-1} .

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 $0 \sim 0.04$, as shown in Fig. 4c. This strangely too low AOD is clearly inconsistent with the surface PM_{2.5} 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.

Response: Revised. We remove the comparison between the simulated AOD and the VIIRS AOD data in the manuscript because the simulated AOD is the all-sky AOD as it includes contributions from both clear and cloudy conditions, whereas the VIIRS AOD mainly represents the clear sky AOD. These two data do not match exactly, especially in South China where there is a large amount of missing AOD data due to cloud cover, which also leads to AOD is too low.

Instead, we added a comparison of the simulated AOD with the MERRA2 AOD data (which provides all-sky AOD data) in order to evaluate the model aerosol simulation performance in lines 308-316 in the revised manuscript, the details of which are shown as follows:

The MERRA-2 data show that the regional average AOD is 0.42, 0.62, 0.35, 0.50, 0.52, and 0.27 in the NEC, JJJ, SB, CC, YRD, and PRD, respectively. The CMA_Meso5.1/CUACE model seems to capture some high-value and low-value areas of AOD well in the south of the domain (e.g., the regional average AOD is 0.31, 0.41, and 0.20 in the SB, YRD, and PRD with MB of -0.04, -0.11, and -0.07) but significantly underestimates AOD in the north of the domain (e.g., the regional average AOD is 0.14, 0.28, and 0.32 in the NEC, JJJ, and CC with MB of -0.28, -0.34, and -0.18). This substantially underestimated AOD in the NEC and JJJ region accompanied by underestimated $PM_{2.5}$ mass concentration is possibly related to underestimated anthropogenic emissions, inadequate representation of aerosol chemical reaction processes, etc. Compared with other studies or models, the CMA_Meso5.1/CUACE model has a similar performance in predicting AOD over China in summer (Werner et al., 2019; Wang et al., 2021; He et al., 2022).

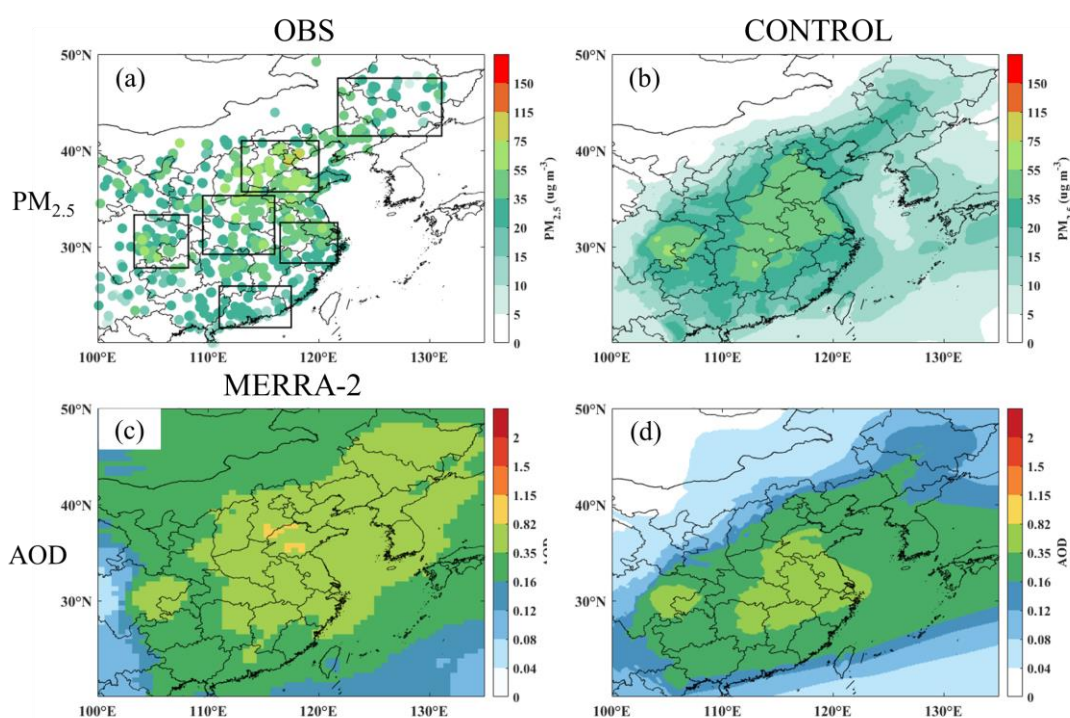


Figure 4: Spatial distribution of time average $PM_{2.5}$ (a and b) and AOD (c and d) in June 2016 from the NO-

ACI_{sub} experiment compared against the observations and MERRA-2 data.

4. NO-ACI_{sub} vs. ACI_{sub} 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-ACI_{sub} vs. ACI_{sub} (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.

Response: Revised. Thank you for your suggestions. Firstly, we have modified the description of the first set of experiments, in particular Table 3 and Section 5.2. The whole Section 5.2 for the first set of experiments represents the impact of model development (subgrid-scale cloud microphysics and radiation feedback in this model) on the prediction of meteorological factors. This is important for the investigation of subgrid-scale ACI, but our presentation may have led to misunderstandings, especially the name of the experiment. especially the name of the experiment. Therefore, we make a clearer distinction between the model development (subgrid-scale cloud microphysics and radiation feedback in this model) and the subgrid-scale ACI effects in the revised manuscript, which mainly includes that modifying the names and descriptions of the experiments throughout the manuscript, and removing descriptions about the impact of subgrid-scale ACI in Section 5.2. In addition, corresponding changes are made in the Abstract, Introduction, Model configurations and experimental design, and other analysis of results and conclusions.

Table 3: Descriptions of multiple sensitivity experiments.

Experiment	Description
CONTROL	Model runs without subgrid-scale cloud microphysics and cloud radiation feedback
CU-MP-RA	Same as CONTROL, but with subgrid-scale cloud microphysics and cloud radiation feedback
ACI _{sub} -DC	Same as CU-MP-RA, but for a deep convective process and fixing the cloud droplets number concentration in the Thompson cloud microphysics scheme as 300 cm^{-3}
CACI _{sub} -DC	Same as ACI _{sub} -DC, but turning off MEIC anthropogenic emissions

Secondly, we have redefined the objective and research contents of this study in lines 97-107 in the revised manuscript. The overall goal of this study is to achieve quantifiable subgrid-scale ACI in the atmospheric chemistry model CMA_Meso5.1/CUACE and to understand the impact of subgrid-scale ACI on numerical weather prediction (NWP). Based on this, we introduce the development of the model for implementing subgrid-scale ACI mechanism in the model in Section 3, evaluate the performance of the developed model with subgrid-scale cloud microphysics and radiation feedback in Section 5.2, and investigate the impact of anthropogenic aerosol on typical deep convective precipitation prediction via subgrid-scale ACI in Section 5.3.

As you mentioned, assessing the relative contributions of subgrid-scale cloud microphysics and radiation feedback is another critical research focus, despite the inherent complexity of reasons. For example, Lim et al. (2014) show that subgrid-scale cloud microphysics in the WRF model improves overestimated radiation and underestimated precipitation during the East Asian monsoon season by increasing detrained cloud water and cloud ice; Alapaty et al. (2012) find that introducing subgrid-scale cloud radiation feedback for regional meteorological and climate modeling makes the attenuation of SDSR more realistic and further suppressed convection. Future detailed discussions will continue in the next paper, including systematically distinguishing the differences between subgrid-scale cloud microphysics and radiation feedback effects on meteorological prediction, as well as quantifying the subgrid-scale ACI effects in detail through controlled emission experiments. We have added these contents to the Conclusion in lines 651-654 in the revised manuscript.

5. 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.

Response: Yes. We actually sample and calibrate the model simulations based on the transit times of the VIIRS and CERES satellites, and the relevant explanations have been added to the revised manuscript in lines 335-338 and 374-375, the details of which are shown as follows:

The daily cloud properties data from VIIRS used in this study consist solely of visible-band products, which are available only during local daytime. For comparative evaluation, the model simulations are sampled according to transit times of satellites over China. The transit time of VIIRS over China occurs approximately between 13:00 and 14:00 local time, and the corresponding simulations for comparison are averaged hourly data at 13:00 and 14:00 local time.

The daily radiation properties from CERES are computed with hourly data derived from MODIS and geostationary satellites (GEO), and the corresponding simulations for comparison are 24-hour averaged values.

6. 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.

Response: Revised. Thank you for highlighting the need to clarify the R value

improvement. We agree that R from 0.7 to 0.73 does not represent an improvement of R and remove the related contents in lines 557 in the revised manuscript.

Specific comments:

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

Response: Revised. We have refined the abstract in the revised manuscript.

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

Response: Revised. It is possible that the PDF is generated with a missing T (ambient temperature). We have regenerated the PDF and the equation can be displayed normally.

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

Response: Yes. The “with a forecast time of 24 hours” means “a 24-hour forecast loop” similar to Zhang et al. (2022)”

4. Figure 6: Please change color map and scale.

Response: Accept. We have redrawn the Figure 6 by using different color map and scale.

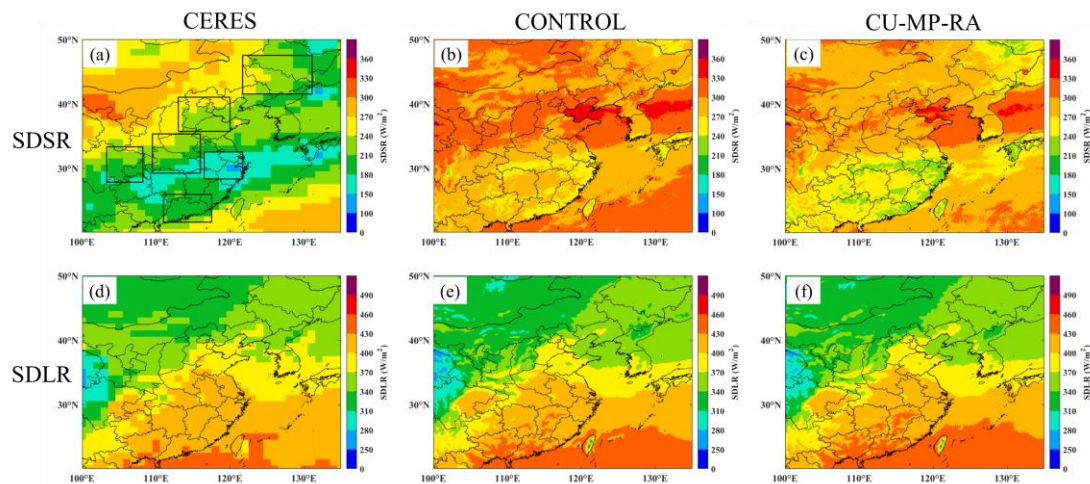


Figure 6: The spatial distribution of time average (a-c) SDSR, (d-f) SDLR in June 2016. The left, middle, and right column is the CERES, CONTROL, and CU-MP-RA experiment, respectively.

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

Response: Accept. We have added relevant explanations to clarify the comparison with surface (ground-based) station data for SDSR in lines 388-389 in the revised manuscript.

6. 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.

Response: Revised. The sentence "The related statistical indicators also show that the model's simulation performance of precipitation is comparable to other models or studies (Table 5)." is changed to "The values of related statistical indicators (Table 5) also show that the simulation performance of precipitation is similar to that of other NWP models (e.g., WRF-CMAQ, WRF, etc.) or results reported in previous studies (Glotfelty et al., 2019; Wang et al., 2021; Wong et al., 2012)." in lines 502-504 in the revised manuscript.

7. 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.

Response. Revised. The sentence means that we find that aerosol mass concentration in the YRD region increase overall, but cloud droplet number concentration does not increase accordingly, and decreases in some areas. According to the section 2 about the impact of CCN effects on deep convective clouds of the study by Fan et al. (2016) and the section 2 about the changed cloud liquid water induced by decreasing aerosol concentration of the study by Glotfelty et al. (2020), we think that the decreased cloud droplet number concentration may be related to lower environmental supersaturation due to thermodynamic/dynamic perturbations (e.g. weaker updrafts, evaporative cooling). These revisions are in lines 570-571 in the revised manuscript.

Grammatical problems:

8. Abbreviations are sometimes not fully spelled out the first time they appear.

Please check again.

Response: Revised. We have rechecked all the abbreviations throughout the manuscript and revised them accordingly.