

Reply to Anonymous Referee #1 Comments

We appreciate you for thoughtful and helpful comments. We tried to answer the referee's comments. Our 'Reply' is embedded below. We hope we provide the appropriate answers, and if there are more questions, please let us know.

The authors present a study about the influence of the representation of aerosol particles in a bin cloud microphysics scheme (HUCM) coupled to the Non-Hydrostatic Model of the Japan Meteorological Agency. While the original representation was rather simple, the study introduces a more sophisticated one basing on a global simulation of the spectral radiation transport model for aerosol species (SPRINTARS) and verified with ground based measurements.

Two realistic case studies over East Asia are performed; one maritime humid case and another polluted dry case. Both lead to reasonable results. To test the aerosol particles' influence, both cases are first repeated with exchanged aerosol conditions, and then with reduced/increased aerosol number concentrations. Micro- and macrophysical characteristics of the resulting clouds were investigated. The authors find that the cloud fraction, the liquid water path as well as the cloud optical depth rather depend on the meteorological conditions, while the effective cloud droplet radius and the cloud droplet number concentration are more sensitive to the aerosol number concentration. As expected, cloud droplets in polluted conditions tend to be smaller and their distribution is narrower. Also, the clouds developing in that condition were found to be geometrically thinner and higher. In the more humid condition, changes in aerosol particle concentration have a larger influence due to precipitation formation than in the drier condition.

The topic of this paper is within the scope of ACP. A new development of the employed bin cloud microphysics model with regard to the aerosol particles is introduced. Also, new ideas of creating a suitable aerosol data set for initializing the model are employed and new case studies are performed. However, though being an interesting study in itself, on the background of existing studies, the results offer only little additional scientific insight into the field of aerosol cloud interactions.

→ Thank you for your comment. The main objective of this study is the application of complicated aerosol size and chemical composition into the 3-dimensional realistic model, which made the model to distinguish from different aerosol conditions (e.g., maritime or polluted aerosol) and to better simulate CN/CCN and cloud microphysical properties. Especially, the consideration on the complexity of aerosol conditions is very important in the investigation of aerosol-cloud interaction over this region, because the enhancements of aerosol loading from various source regions (e.g., dust storms from deserts located on the

continent, anthropogenic pollutants transported from industrialized regions over the coastal area of China, and sea salt particles emitted over the oceanic area under strong wind conditions) have been predominant. With the distinction of aerosol characteristics, the investigations on low-level shallow clouds are performed under various environmental conditions by case studies over East Asia. In particular, the simulation of low-level shallow clouds over East Asia is the first attempt with focusing the aerosol effects on cloud with high-resolution modeling. In the revised manuscript, we rewrite several sentences to emphasize the originality and the findings in this work.

The employed methods are valid and sufficiently documented and discussed. The coarse model resolution of 3 km in combination with a bin microphysics scheme and shallow stratocumulus clouds might be regarded as insufficient, though.

→ Typical spatial resolution for simulating low-level shallow clouds with a bin microphysics scheme is less than 1 km, but this study uses 3 km grid size because target area and season tend to have a warm sea surface and relatively unstable atmospheric conditions to generate a relatively larger cloud cells than those in the East Pacific region where strong atmospheric stable conditions do not permit such a crude model resolution to simulate realistic aerosol-cloud interaction. For this reason, Iguchi et al. (2008, Fig. 21) confirmed that the simulated statistics of cloud microphysical parameters do not depend on the model resolution in the East China Sea region, which is also our target region, between simulations of 2 km and 7 km grid sizes. This is already described in the Introduction.

If the low-level shallow cloud will be simulated in finer resolution less than 3 km, it is expected that the simulations can resolve more small-scale cloud cells that are not represented in the present simulations. The attempt to the finer spatial resolution will be done in further work if computational resources are sufficiently given.

However, some issues remain to be corrected or clarified:

1. Some expressions are not used in a clear way.

1) For example, p.5 1.21 “bin-based mesoscale non-hydrostatic model of Iguchi et al” - bin-based is the cloud model, which is coupled to the mesoscale model.

→ It should be corrected to “a meso-scale non-hydrostatic model implemented by bin-based cloud microphysics scheme”. We have corrected it in the revised manuscript.

2) The definition of CCN is not given

→ The definition is supplemented in the Introduction of the revised manuscript.

3) p.21 1.2 “cloud droplet mixing ratio” probably means “cloud liquid water mass mixing

ratio”

→ “Cloud liquid water mixing ratio” is regarded to be more appropriate. We have corrected it.

4) Also, it is not always clear whether the authors look at spatial averages or not, for example p.16, 115 and following, probably average values for CN, CCN and so on are meant, but no details are given, only “spatial distributions” is mentioned.

→ Details on the “spatial distributions of CN and CCN” are presented with the explanation for Fig. 4. For CN and CCN values you mentioned, we included “averages” or “spatial average” to describe clearly that the spatial averages from model simulation are compared with observation dataset.

5) LWC is defined in the text as “liquid water content” (p.22) and in the picture caption as “cloud liquid water content.

→ We added “cloud” before “liquid water content” in the revised manuscript, same as the caption of Fig. 10.

2. The authors not always discuss the prerequisites for their simulations thoroughly. For example, on p.9, 114 they cite Pöschl et al for a certain range of B -values for continental and marine aerosols, but use different B -values from other authors which partly fall out of this range (1.22). Why do you use which values and why do they disagree with the first citation?

→ The criteria to determine B -values are different between Pöschl et al. (2009) (continental vs. maritime air mass) and this study (different aerosol composition, that is, sulfate, dust, sea salt, organic carbon and black carbon). In the present simulations, five different B -values were applied for each of five aerosol species in the calculation of CCN activation, as proposed by Ghan et al. (2001). Therefore, we decided to remove Pöschl et al. (2009) in the revised manuscript to avoid confusion, while we keep the references on B -values for different aerosol composition intactly.

3. Also, same p.9, the authors state Petters and Kreidenweis 2007 proposed the B -value and then cite Pruppacher and Klett for the definition (1.5 and 8). Please formulate clearer whose definition/original work you use, or maybe reformulate the sentence starting 1.5 so that it is clear what exactly Petters and Kreidenweis propose.

→ The definition that we adopted is one from Pruppacher and Klett (1997) as described in Eq. (1). To clarify the statement on it, we only cite Pruppacher and Klett (1997) who provided the definition of B -value in the revised manuscript with excluding the citation of Petters and Kreidenweis (2007).

4. The specific setup of the sensitivity tests isn't made too clear (p.23, section 5.4 beginning). It would be valuable to add a sentence what exactly you do here.

→ These sensitivity tests are performed to examine the variations of cloud microphysical parameters and precipitation amount by only change in CN number concentrations. Therefore, we only modified CN number concentration to be 0.25, 0.5, 2, and 4 times of that in four kinds of simulations (M_{humid} , M_{dry} , P_{humid} , and P_{dry}). We reworded to clarify the objective and setup of these sensitivity simulations in the revised manuscript.

5. Also, p.11, l. 9 following: Does the substitution of some data from the data set JMA-MANAL with NCEP reanalysis introduce inconsistencies in the model initial/boundary data?

→ In this study, SST and RH over 300 hPa from NCEP reanalysis data were used to complement JMA-MANAL datasets. We believe that the substitution of NCEP SST data is the current best way, as long as JMA-MANAL does not contain SST data. RH over 300 hPa has little influence on our cloud simulations because we are focusing on low-level clouds simulated only by liquid cloud microphysics. We included this in the revised manuscript.

6. p.15, 1st paragraph: The observed values for CN number concentrations and the estimated ones still differ much, but even the given error does not account for the difference. Please discuss this. The instrumental error is not given (and thus also missing in fig. 5), but would be very interesting – maybe it helps for the mentioned discussion!

→ It's a good point. In spite of the improvement, there is still a gap between calculated and observed CN and CCN concentrations, especially for case 2. Firstly, this discrepancy possibly resulted from the underestimation of aerosol mass concentration by SPRINTARS especially in polluted condition or from imperfect size distributions of pollutants, such as sulfate and organic carbon (too much shifted size distributions to Aitken-mode particle which is difficult to be activated to cloud droplet), rather than observational uncertainties. Secondly, inhomogeneous spatial distributions of aerosols can be attributable to this discrepancy, because direct comparison of simulated CN/CCN values with 3-km horizontal resolution with the observed data at a point (i.e., Gosan site) was made in this study. We added this discussion in the revised manuscript.

Also, it is correct that the error bar for observational data is not given because the data at a single point and specific observation time were used to compare with model simulation, while simulated results of CN and CCN were averaged near Gosan site. Following the suggestion of referee #3, in addition, we reset the averaged area near Gosan site to smaller size than that in previous manuscript, centered at Gosan site.

7. The challenges/downsides of bin-schemes are not mentioned, and the challenges of the simulation setup (boundary conditions from very coarse data, coarse resolution of 3km, hygroscopicity parameter) are only discussed/mentioned in the summary. You might also want to include this at a more appropriate place in the discussion.

→ Although bin-type cloud models have been used to investigate the detailed modification of the size distribution of aerosol and cloud droplet, it takes a large amount of computing time and is limited to be used for simulation of large-scale areas and for many runs for sensitivity studies (Sato et al., 2009). Various approaches have been developed to improve low computational efficiency (e.g., base function method by Suzuki (2004), and Monte-Carlo integration method by Sato et al. (2009)), but the computational costs of bin-type cloud schemes remain still high.

These downsides of bin-based cloud schemes are added in the summary, and the challenges of model setup are additionally included in Sec. 2.2.2 and Sec. 3 of the revised manuscript.

8. Some citations seem to be missing (p.10, 1.23 “MIROC-AGCM”; p.11, 1.6 “JMAMANAL”).

→ The citations for “MIROC-AGCM” were added in Sec. 3 of the revised manuscript. As for “JMA-MANAL”, we modified the part of the manuscript as “The JMA meso analysis dataset (JMA-MANAL) distributed by the Japan Meteorological Business Support Center (JMBSO, available at <http://www.jmbsc.or.jp>)”.) because we have no specific citation.

9. Sometimes the authors formulate very carefully, to give some examples: p.21 1.4 “The scale of simulated vertical velocity might be in the range of that prescribed for shallow stratocumulus by Feingold (2003)”. Is it or is it not in the range? Please quantify and give a clear statement. Also, 1.14. “...could bring the cloud bottom to a lower altitude...” - does it or not? There are a few of such statements more throughout the text.

→ Both of sentences mean the positive sense; the updraft scale is in the range from previous studies, and more precipitating particles bring the cloud bottom to a lower altitude. We tried to give clear statements through the revised manuscript.

10. Very large errors are given for LWP and COD in table 1 (larger than 100%), please discuss this! With such large errors a model could simulate almost any value and would lie within the interval given by the error.

→ Vertical integrations for each horizontal grid point (3 km x 3 km) over a large area (1500 km x 1500 km) result in extremely variable LWP and COT. If the cloud liquid water mixing ratio for even one level in one grid point exceeds 0.01 g kg^{-1} , it should be included in the vertical integration. Therefore, the estimated LWP and COT range from 0.1 to 800 and from 0.01 to 120, respectively. The estimation of such large errors for LWP and COT over large

target domain is regarded to be inevitable in the present simulation with high horizontal resolution. We added this statement in Sec. 4.2 of the revised manuscript.

11. The reviewer strongly suggests to have the text checked by a native speaker, since there several grammar errors in the text.

→ Thank you for your suggestion. We rechecked the grammar and wording in the whole manuscript.