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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulation of the aerosol effect on the microphysical properties of shallow stratocumulus clouds over East Asia using a bin-based meso-scale cloud model" *by* I.-J. Choi et al.

Anonymous Referee #1

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"Simulation of the aerosol effect on the microphysical properties of shallow stratocumulus clouds over East Asia using a bin-based meso-scale cloud model"

by I.-J. Choi, T. Iguchi, S.-W. Kim, S.-C. Yoon, and T. Nakajima

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 sim-

ple, 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.

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.

However, some issues remain to be corrected or clarified:

Some expressions are not used in a clear way. For example, p.5 l.21 "bin-based

mesoscale non-hydrostatic model of Iguchi et al" - bin-based is the cloud model, which is coupled to the mesoscale model; the definition of CCN is not given; p.21 l.2 "cloud droplet mixing ratio" probably means "cloud liquid water mass mixing ratio". Also, it is not always clear whether the authors look at spatial averages or not, for example p.16, l15 and following, probably average values for CN, CCN and so on are meant, but no details are given, only "spatial distributions" is mentioned. LWC is defined in the text as "liquid water content" (p.22) and in the picture caption as "cloud liquid water content.

The authors not always discuss the prerequisites for their simulations thoroughly. For example, on p.9, l14 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 (l.22). Why do you use which values and why do they disagree with the first citation?

Also, same p.9, the authors state Petters and Kreidenweis 2007 proposed the B-value and then cite Pruppacher and Klett for the definition (l.5 and 8). Please formulate clearer whose definition/original work you use, or maybe reformulate the sentence starting l.5 so that it is clear what exactly Petters and Kreidenweis propose. 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. 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?

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!

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.

Some citations seem to be missing (p.10, l.23 "MIROC-AGCM"; p.11, l.6 "JMA-MANAL").

Sometimes the authors formulate very carefully, to give some examples: p.21 l.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, l.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.

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.

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

Best wishes!

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23449, 2010.