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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 #2**

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The article presents simulations of marine stratocumulus clouds forming off the coast of East Asia using a bin microphysics scheme coupled to a mesoscale dynamical model from the Japan Meteorological Agency. The representation of aerosols in the model has been improved to account for more realistic multi-modal size distribution and multiple chemical components. The model simulations are driven by initial and boundary conditions from the meso-analysis dataset of the Japan Meteorological Agency for meteorological variables. Initial and boundary conditions for the aerosol are taken from the SPRINTARS model. The authors conduct two case studies with contrasting me-



teorological context and compare the improvements in the representation of aerosol and clouds with a previous version of the model and with observations from satellites and ground-based observations. Sensitivity experiments are conducted for both case studies by interchanging the initial conditions for aerosols or moisture. These sensitivity studies are used as a tool to investigate the effect of aerosols on marine stratocumulus in the East Asian region. It is found that the improvement of the aerosol parameterization and/or cloud microphysics in the model greatly improves the cloud microphysical and macrophysical properties when compared with the observations. In the sensitivity experiments, size distributions of cloud droplets showed a more narrow distribution if aerosol concentrations are increased, thereby also affecting precipitation development. Both findings are in agreement with findings from many other studies. Clouds growing in the polluted environment also showed the tendency of cloud thinning and higher cloud layers than those developing under maritime conditions. Sensitivity tests suggest that quantities such as effective radius and cloud droplet number concentrations are predominately affected by changes in aerosol number concentrations whereas changes in liquid water path and cloud optical depth are more influenced by changes in the meteorological conditions than changes in aerosols.

While the paper contains some interesting (rather technical) aspects such as the improvement of the microphysics in the model and comparison to observations, scientifically there is little in the manuscript that is really new and revealing. Although the general contents of aerosols, cloud microphysics and aerosol-cloud interactions would fit the scope of ACP, there is doubt that the manuscript meets the standards of ACP regarding novelty and scientific merit. Some of the details about the modeling framework are unclear, which leaves the reader with many open questions. Due to this lack of explanation it is also not clear if the modeling framework is really capable of realistically simulating the physics of aerosol effects on stratocumulus clouds. Although the manuscript generally has a good flow the grammar and wording would benefit from revisions. Some figures need improvements.

A detailed list of review points is listed below.

Section 2.2: It is unclear how the aerosols are treated in the model. Prognostic or diagnostic? Clearly, in reality, aerosol size distributions can change significantly, regionally as well as temporal. How is this taken into account? Are aerosol dynamical processes such as coagulation, condensation, aerosol nucleation, aerosol scavenging treated in the model. How is the activation of aerosols to cloud droplets treated. Please clarify.

Section 2.2.1 and Fig. 1: As can be seen from Fig. 1, the representation of the aerosol size distribution and comparison to observations is clearly improved in the new version of the model. However, for some species such as sulfate, black carbon and organic carbon there is no observational evidence that would support the bi-modality in the aerosol spectrum. Thus, the magnitude of the fine mode aerosol is speculative at this point and there is no reasonable argument that it should exist at all. Also, mass closure would not be sensitive to fine mode aerosols. However, the cloud parameterization is likely to be sensitive to the fine mode aerosols especially in cases with higher updrafts that create enough supersaturation to activate these particles. For dust, the size distributions is overestimating the number of accumulation mode aerosols by one order of magnitude while underestimating the size. I think a better representation of the aerosol size distribution could be obtained by fitting log-normal size distributions to the observations and using the estimated parameters of the fitted size distributions in the model.

Section 2.2.2: The assumption of insoluble BC is certainly a limitation of this study and unrealistic. Many studies show that BC can be coated with sulfate, which renders mixed BC-SU aerosols generally good CCN.

Section 3: While a horizontal resolution of 3 km seems to be a reasonable compromise between the LES scale and the synoptic scale, both of which are important for stratocumulus clouds, the vertical resolution of the model seems to be insufficient to realistically capture features of the boundary layer. It would be interesting to see com-



parisons of boundary layer structure with observations to be able to judge how well the model can handle the boundary layer dynamics at this coarse resolution. Otherwise, it is unclear to what extent the model is capable of simulating aerosol effects on boundary layer clouds through microphysical feedbacks that are ultimately linked to changes in the PBL dynamics.

P. 23460, L. 12ff: Even if the wind directions are similar in both cases there are clear dynamical features (e.g., cut-off low type feature, vorticity) that are completely different and clearly impact cloud development. However, these features are not discussed by the authors. I also recommend to show only the relevant parts of the weather charts in Fig. 2 (e.g., only part that overlaps with model domain).

P. 23461, L. 15ff: I think one fundamental question that has not been addressed here is to assess how the SPRINTARS model compares with observations for the particular case studies. E.g., one could compare the aerosol fields against observations from the ground based sites to get a first clue on how SPRINTARS compares. This is important because later in the text the authors compare the regional model with observations but it is unclear if the regional model does a better job than SPRINTARS.

P. 23463, L. 14ff: What does the uncertainty range mean for the model simulations? Why is there uncertainty and why is it so large? Why do the authors compare averaged values instead of comparing observations from the ground-based site against nearest grid point values? If the model has a horizontal resolution of 3km a point to point comparison should be reasonably fair.

Although the simulations with improved microphysics are closer to observations, which is encouraging, the overall comparison with observations could not be much worse. The aerosol concentrations are off by more than 30% in both cases and the CCN concentrations by more than 300% in case 2. This leaves the reader wondering if the “good” comparison in case 1 is just by chance or just pointing to deficiencies in the model’s microphysics to get the correct number of CCN from erroneous number of aerosols.

P. 23465, L. 13: Fig. 6 suggests that there are clear regional differences. I am not convinced how meaningful a comparison of averaged values is in this case. Also, comparing the model with MODIS observations shows large discrepancies in the spatial patterns. Yet the authors argue that the comparison is reasonable. It would be good to compare also cloud droplet number concentrations and cloud fraction with corresponding MODIS products. Fig. 6 would greatly benefit from a different color scale. It is difficult to discern differences if almost all values have very similar shading.

P. 23466. L. 9: So far, the manuscript exclusively deals with the evaluation/comparison of the newly implemented microphysics parameterizations and not at all with aerosol effects on Sc cloud as suggested by the title. Section 5 covers the aerosol effects by means of sensitivity studies but fails to provide any content that is scientifically new. At this point I wonder if it would be better for the manuscript to focus on the implementation/evaluation part only, change the title accordingly and resubmit the manuscript to a different journal.

P. 23468, L. 7: In my view a significant advantage at simulating aerosol-cloud interactions with a regional model rather than a LES is to capture mesoscale dynamical feedbacks that stem from the cloud microphysics and feed back to the cloud dynamics. However, it is unclear if a simulation time of 9 h would be sufficient to capture these effects. Can the authors comment on what motivated the simulation time of 9 h?

P. 23474, L. 15: The authors conclude that clouds in polluted aerosol conditions show tendencies towards cloud thinning and higher cloud layers and speculate that this may be related to updrafts. However, differences in updrafts and/or boundary layer dynamics have not been looked at so far. How can the authors arrive at this conclusion?

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