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Interactive comment on "Development of a cloud microphysical model and parameterizations to describe the effect of CCN on warm cloud" *by* N. Kuba and Y. Fujiyoshi

Anonymous Referee #3

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This work constitutes two parts. In the first part, authors integrate a parcel model (Lagrangian, simulates the activation of particles), a bin model (semi-Lagrangian, describes condensational growth and coalescence of cloud droplets) into a hybrid cloud model. The hybrid model is applied into a grid point model (Eulerian) to account for the sedimentation and advection of cloud droplets and raindrops. The whole hybrid model system ran for several scenarios to investigate impacts of aerosol particles with different size on cloud properties and precipitations.

In the second part of the work, authors develop a parameterization to predict the activated cloud droplet number concentration (CDNC) based on results from the first part. The “actual” activated CDNC is thought be less than the possibly activated CCN, while the latter can be calculated as a function of the supersaturation. The developed parameterization is implemented into the CCSR/NIES/FRCGC-AGCM for the CDNC calculation. Furthermore, assuming a Gamma distribution, the cloud droplet size distribution (CDSD) can be parameterized in terms of activated CDNC and condensable cloud water. Both parameterizations are verified with the parcel model in the hybrid model system.

This paper presented a very decent work on the simulation of aerosol-cloud interaction using models and parameterizations. It is important for a more accurate model estimation of aerosol effects on cloud properties. The model and parameterization developed in this work are adequately described and reasonably convincing. In addition to it some relevant questions may want to be clarified, to give a more explicit picture to the audience. Results of running the hybrid model are presented in section 3 and summarized in section 6. Potential influences of aerosols with different size on cloud properties and precipitation are exhibited. Based on these results, parameterizations of activated CDNC and CDSD are developed. Necessary verifications are conducted and CDNC parameterization is applied into a GCM for test. Investigation by authors on this topic is fairly sufficient and worthy a publication of ACP.

General scientific comments and questions:

The hybrid model system treats the activation process, condensation/coalescence process using different numerical method, which seems reasonable. My question is concerned more about the controlling factor on a larger scale, for example, available cloud water and updraft velocity at the grid point. If I understand correctly, these two quantities are not prognostic in the grid model (or at least not mentioned in this article). The grid model seems only predict relative humidity (RH) and wind field. Therefore I will wonder what updraft velocity is prescribed to run the parcel model. How is the cloud water transferred from the grid model to the parcel model and then to the bin model for

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simulating the different cloud processes? Are there any feedbacks of these processes in terms of the cloud water?

Authors may consider adding some sentences about these two quantities. Because they are probably more important than the aerosol number and size to determine cloud properties in some sense. The effect of updraft velocity may overwhelm the sensitivity of CDNC/CDSD to the aerosol number and size. Available cloud water can limit the growth of large droplets or inhibit the activation in the polluted case with more numerous aerosol particles. Thus, if the relative importance of updraft velocity/available cloud water and aerosol properties to the CDNC/CDSD can be addressed in this study more or less, it will be nice.

The parameterization of activated CDNC developed in this study is proposed to apply in large-scale climate models. For its application into CCSR/NIES/FRCGC-AGCM, as the reviewer understands, the GCM prognostic updraft velocity in each grid point determines which supersaturation is chosen. E.g., if the model predicted updraft velocity is 0.6 m/s. S=0.5% is chosen as the maximum environmental supersaturation in the GCM grid. Then critical radii at this supersaturation for different aerosol species are calculated following the Koehler theory. Based on the aerosol size distribution assumed in the GCM depending on the prognosed aerosol mass, number of aerosol particles with size larger than the critical radius can be obtained for each aerosol species. Sum of these numbers is the Nc(S) to be used to calculated the actual activated CDNC. Is it correct?

If so, it means that the competition of water vapor for more than one soluble aerosol species are not taken into account using this parameterization. Will it result in an overestimation of the possibly activated aerosol number Nc(S) potentially? And in turn overestimate the activated CDNC?

Specific comments and questions:

1) Abstract, line 10: "˜affects both the amount and the location of precipitation." Would

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it be better to say “the amount and the area/coverage of precipitation”?

2) Page 1418, line 8: “Considering the time constants at which˜” What is the “time constants” here referring to? Would it be better to say such as “Considering the time required for CCN to reach their equilibrium˜”

3) Page 1418, line 15-16: “The growth by condensation of each droplet during each step is limited so that the radius of each droplet does not exceed its equilibrium radius.” What is the “equilibrium radius” referring to? Is this treatment to restrict the parcel model only to produce the CDSD at the early stage of the cloud formation, to avoid the production of too large droplets that will influence the subsequent coalescence model simulation?

4) Page 1418, line 22-25: Has the reduction of saturation due to the activation been accounted for the subsequent condensation/coalescence calculation?

5) Page 1420, line 1: “˜to the peak updraft.” What is the value of this updraft? If a typical value of updraft velocity at which clouds generate in the hybrid model system can be indicated, it would be good.

6) Page 1420, line 25-28: It might be also because of the latent heat release. Those small droplets do not quickly precipitate but stay in clouds for longer time and go on to grow by condensation; latent heat released during the condensation induces turbulence, which further inhibit the fall down of these droplets.

7) Page 1421,line 8: The information about Szumowski et al., 1989 is not listed in the reference.

8) Page 1421, line 7-10, 15-20: Authors may want to explicitly indicate here, if alpha, beta and Qc0 are determined with the different CCN properties or they are empirically tuned.

9) General question for part 3.1: Does it make sense to use a bulk parameterization to validate the result of the bin-model? I would expect the opposite.

10) Page 1422, line 22-24 and Page 1423, general question for part 3.3: "Adding large-particle CCN does not affect rainfall amounts˜" I wonder if adding large-size/giant CCN results in faster precipitation when there are a large number of small particles? May the hybrid model give an evolution of drop size to see if the formation of rain is accelerated with increased large CCN? It will be interesting to check it.

11) Page 1424, part 4.1: Is this parameterization valid for all soluble constitutes of aerosol particles? Or an assumption of the chemical composition was made for the model to develop this parameterization? It will be good to mention it here.

12) Page 1425, line 22: "˜assumed size distribution of CCN." Is it assumed to be a certain type of distribution, e.g., log-normal? Has the sensitivity of parameterized CDNC to the CCN distribution type been tested?

13) Page 1427 part 5: Parameterization of CDSD is not applied into the GCM for test, but only applied into a bin model and verified in part 5. Authors may want to indicate it in the title of part 5 such as "verification of CDNC and CDSD parameterizations in bin model", and the title of part 4.1 such as "parameterization of CDNC and its application in AGCM".

14) Page 1431 line 7-13: The relative importance of dynamical factors and aerosol effect may want to be mentioned a bit here.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1413, 2006.

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