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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global simulations of aerosol processing in clouds" *by* C. Hoose et al.

Anonymous Referee #1

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This is a nicely written and well organized paper. The authors present clear descriptions for the new aerosol processing scheme in ECHAM5-HAM model. Efforts were made to compare simulated results to multiple observational datasets. The authors concluded that, with the new treatment, the model shows reasonable agreements in scavenged fraction and aerosol number concentrations in the marine boundary layer, while cloud droplet concentration, aerosol optical depth, and size of the accumulation mode are generally overestimated. Several sensitivity tests were carried out to identify possible improvement to the biases.

The paper provides detailed analyses to aerosol and cloud microphysics in the simulated results. The information is useful for future model intercomparison studies, but also makes the manuscript considerably lengthy and distracts from the main results. I suggest to mainly focus on results in the AP simulation (i.e. the extended model) in Section 4 (in-cloud aerosol budgets) and particularly in Section 5 (comparison with

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observation); results in CTL can be mentioned only to help explain trends or biases in AP. Below are some specific comments on the manuscript.

Section 2 Model description:

P13560, line 8 – The authors stated here (and also in Section 4.3, P13573) that inhomogeneous mixing is assumed for the release of aerosol mass from below-cloud evaporation of precipitation, which is different from the assumption for evaporation of cloud droplet and crystals. Although the description is clear for below-cloud evaporation, a more explicit description for droplet evaporation in the model is needed so that the readers can easily understand the difference between the underlying assumptions.

P13562, line 4 – I agree that tuning the autoconversion rate to maintain radiative balance is justifiable, but since the tuning basically changes the overall state of the hydrological cycle, some comparative evaluation for precipitation is needed. Briefly compare global mean values of precipitation rates in all simulations to some standard observational climatology (e.g. Global Precipitation Climatology Project) in Section 3.1 and Table 3 should be informative.

Sec. 3 Comparison to the standard model:

P13564, Table 3 and Figure 4 – LWP retrieved by SSM/I (and the other data in O'Dell et al., 2008) is only over ocean. Please confirm that the simulated results shown here are also over ocean. Is the global mean AOD observation from AERONET? Please clarify and provide more detailed descriptions (e.g. time span of measurement).

P13566, line 21 – It is stated that mineral dust emissions in AP are different from CTL, because the surface winds can be changed in the model. This argument can be verified by comparing figures of the geographic distribution of dust emissions and surface winds in CTL and AP.

Section 4.3 Life cycles of cloud condensate and in-cloud aerosol:

The comparison with Pruppacher and Jaenicke (1995) is interesting, but adds only lim-

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ited insights to model performance. The discussion related to Pruppacher and Jaenicke (1995) can be trimmed to make this section more concise.

Section 5 Comparison with observations:

As suggested above, results for CTL can mostly be removed to focus the whole section on AP.

Figures 16 and 17 – Part (a) in the figures is confusing. Just showing part (b) would be sufficient

Figures 18 and 19 – Duplicate figures for CTL. Please provide a figure for AP.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13555, 2008.