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Interactive Comment

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

C. Hoose et al.

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Reply to Referee #1

We would like to thank the referee for the constructive input and critical questions. We reply to the individual comments below.

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 observation); results in CTL can be mentioned only to help explain trends or biases in AP.

In section 4, simulation CTL is only included in section 4.1. We think this section, which reveals that the new treatment significantly reduces scavenging, is crucial for the interpretation of the following results. This could not be shown if simulation CTL was left out here. We were able to shorten section 4 by leaving out some parts of part 4.3 (see below). In section 5, we also would like to keep simulation CTL in both the plots and the discussion because these are sets of observational data to which ECHAM5-HAM has not been compared before, and it will be useful for future studies to have a reference simulation to compare to. Furthermore, the interpretation of many results from simulation AP is only meaningful when compared to the performance of the standard model, and this also gives insight into which physical processes influence the results. Section has been shortened by removing three of the figures.

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.

We have added a few sentences on the homogeneous mixing assumption right before this paragraph.

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.

We have added the global mean precipitation rate in Table 3 and a short discussion. The precipitation rates are very similar, because they are determined by the prescribed

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sea surface temperatures. The fraction of stratiform precipitation (not shown) is also nearly constant (ranging from 45.17% to 45.65%).

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).

The simulated LWP in Fig. 4 is over ocean only. We have changed the plot title and have included this in the figure caption. The simulated values in Table 3 are global averages over both land and ocean. We have added a footnote. The AOD observation is a multi-annual satellite composite from MISR, MODIS, POLDER and AVHRR-NOAA, enhanced by AERONET sun-photometer monthly statistics. We have added a reference for this (Kinne, 2008).

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.

These figures have been produced for the simulations as presented in the submitted paper and can been viewed on http://folk.uio.no/corinnah/plots/dust_emission_difference.pdf . The first row of figures shows the mineral dust emission flux in simulations CTL, AP and the difference between AP and CTL. It can be seen that although the patterns are very similar, emissions are slightly higher in simulation AP in all major emission areas. The second row shows the 10m windspeed, for which the difference AP minus CTL looks much more noisy. However, over continents, the 10m windspeed is on average higher in simulation AP. Although mineral dust emission is sensitive rather to peak wind speed than to the average wind, this is a strong indicator that this change in model weather

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is responsible for the higher dust emissions in simulation AP. Another parameter which can influence dust emissions is the soil moisture ws , which is depicted in the third row. ws is smaller in AP than in CTL in many source regions, which further contributes to higher emissions. The biggest source, the Sahara, does not exhibit significant changes in ws, because the values are very low (thus not limiting dust emissions) in both simulations. We have decided not to include these additional figures into the paper, as increased dust emissions are a not robust feature of simulations including a detailed representation of aerosol processing. When rerunning the model for the resubmitted version with a slightly different formulation of below-cloud re-evaporation, we find rather similar dust emissions in CTL and AP (722 vs 719 Tg/yr).

Section 4.3 Life cycles of cloud condensate and in-cloud aerosol: The comparison with Pruppacher and Jaenicke (1995) is interesting, but adds only limited insights to model performance. The discussion related to Pruppacher and Jaenicke (1995) can be trimmed to make this section more concise.

We have removed the "P&J, rescaled LWP" calculations and the related discussion from the text and from the table in order to shorten this section. Furthermore, we have taken out a paragraph about the homogeneous vs. inhomogeneous mixing assumption, because this is now described in section 2 (see previous comment).

Section 5 Comparison with observations: As suggested above, results for CTL can mostly be removed to focus the whole section on AP.

We think that including results from simulation CTL is important here, because they can not be found elsewhere and are necessary for a meaningful evaluation of simulation AP.

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

We have removed the upper parts.

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

Corrected.

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