

Interactive comment on “Cloud microphysics and aerosol indirect effects in the global climate model ECHAM5-HAM” by U. Lohmann et al.

U. Lohmann et al.

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Response to reviewer 3:

1) Beyond minor technical issues there are a few general points which could be clarified or discussed in the manuscript. It would be nice to separate for the reader what is new in this manuscript beyond the documentation of the microphysics scheme by Stier et al 2005, to highlight the key features of this work.

Actually the microphysics scheme discussed by Stier et al., (2005) only refers to the aerosol microphysics. The aerosol module in Stier et al. (2005) is not coupled to the cloud scheme. The coupling of the aerosol scheme to the cloud scheme is the achievement of this study. We clarified that point.

2) It would be nice to see some discussion of the stability of the model physics to horizontal

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resolution in particular. Does the microphysics change substantially at resolutions higher than T42? Do the aerosol effects change? Since more of the vertical velocity might be resolved, it might change things.

We did one experiment at T63 horizontal resolution. The results were comparable. Thus in order to save computer time, we focussed on the T42 simulations.

3) It would also be nice to estimate what the effect of including the more detailed cirrus scheme might be on total aerosol indirect effects. Would the glaciation effect be enhanced and reduce the total indirect effect?

We were able to implement the cirrus scheme and redid the ECHAM5 simulations with the cirrus scheme. It actually changes the effect very little. The indirect aerosol effect increased from -1.8 to -1.9 W m^{-2} . We rewrote the manuscript accordingly.

Specific comments:

1) P4, L104: What pieces of the discussion in section 2.1 are different from Stier et al 2005. Please indicate what is new, and eliminate that which is covered elsewhere.

Only the first paragraph is from Stier et al. We made that clearer.

2) P4, L108: Where does alpha come from? What is it, and how is it chosen?

It's based on observations as discussed by Lin and Leaitch (1997). We added that.

3) P6, L160-169: This section was very confusing. Is 'this threshold' the 0.5 mg kg^{-1} threshold? I think you are saying that you follow water saturation until you reach 0.5 mg kg^{-1} ice, then drop to ice saturation, both with a hard limit. I do not understand exactly how this differs from ECHAM5-REF and ECHAM4. The word 'intended' is awkward.

Yes, the threshold is the 0.5 mg/kg threshold. The paragraph has been rewritten for clarification.

4) P11, L323: I do not see a secondary liquid water path maximum in the tropics in figure 1. At

what latitude?

We referred to the peak in LWP in the ITCZ. This statement has however been deleted in response to reviewer 1 who suggested shortening of sections 3.1 and 3.2.

5) P14, L409: Does the model also reproduce aerosol values in clean regions? Can you show a plot with data from a pristine region in Figure 3 too? I am quite worried about large values of black carbon in the stratosphere which are a factor of 2-5 too large. This might substantially alter the radiation balance either (a) directly or (b) indirectly by changing tropopause temperatures. Can you show that this does not impact the stratospheric radiation balance?

The used ECHAM5-HAM aerosol-climate model has been extensively evaluated for polluted and clean environments in Stier et al., ACP, (2005) and Stier et al, ACPD, (2007) and shows generally a good agreement with the available measurements. The evaluation of the aerosol profile in Fig. 5 supports the general quality of the simulations - for the scales a global aerosol model (c.f. Schwarz et al. 2006).

It is very unlikely that the overestimation of the BC mass mixing ratios in the stratosphere has any significant impact. First of all, it is important to point out that due to the fact that the air density decreases exponentially with altitude, the absolute BC mass at this altitude, more directly related to the strength of the aerosol radiative effects, is very small. In addition, the BC mass mixing ratio in the UTLS region is 1-2 orders of magnitude lower than in the BL. Further, it is clear from Fig. 5 that the BC mass constitutes only a small fraction of the total aerosol mass at this altitude so that it can be assumed that the total aerosol absorption at this altitude would be dominated by the long-wave absorption of the total aerosol components (not considered here, see Stier et al., ACPD, 2007 for more details).

6) Figure 5 would be easier to interpret as a series of joint PDFs: with IWC/TWC v. Temperature.

We prefer it in this way in order to have the direct comparison to the figure in the paper

by Korolev et al.

7) *P19,L574: Cloud drop number concentrations are not shown in Figure 8.*

The reference to Figure 8 has been deleted.

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