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Interactive Comment

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 1:

GENERAL COMMENTS: This manuscript contains a lot of material, some of which is quite interesting (e.g., the comparison with field data in section 3.3). To some extent, the paper may be even overloaded with details, which leaves the reader exhausted and wondering what is really important. Fortunately, the concluding section is concise and clear. Scientifically, this seems like a sound paper, which should be acceptable for ACP subject to the minor revisions listed before.

One general scientific comment concerns the use of the "total anthropogenic aerosol effect". It would be worthwhile to point out in the paper the differences to the IPCC's definition of aerosol radiative forcing. Firstly, "the total anthropogenic aerosol effect" is not strictly a ra-

diative forcing because the tropospheric temperature field is allowed to change, although this might be a minor problem in practice when sea-surface temperatures are prescribed. Second, it includes (apparently major) contributions from the cloud lifetime effect (i.e., second indirect effect) which is excluded from the IPCC aerosol radiative forcing estimates as too uncertain (see. e.g. the Summary for Policymakers of the IPCC's Fourth Assessment Report). Finally, the values of the total aerosol effect simulated by ECHAM5 are quite large (-1.8 ... -2.9 W m-2), which approaches the positive radiative forcing due to anthropogenic increases in greenhouse gases (about 3 W m-2, including CO2, CH4, N2O, CFCs and O3). It would be interesting to know whether or not the authors consider this result realistic.

We added that "The total anthropogenic effect is not a forcing in the IPCC's definition of aerosol radiative forcing because it includes contributions from the cloud lifetime effect and allows adjustments of the temperatures."

Yes, we regard the result of -1.8 (now -1.9 with the inclusion of the cirrus scheme) as realistic. We don't regard the higher values of -2.6 and -2.8 in the revised version as realistic for exactly the reasons that you are quoting above. We added that.

SPECIFIC COMMENTS

1) p. 3720: It would be helpful to explain already in the abstract what the "doublemoment" cloud microphysics scheme means (i.e., it predicts both the mass-mixing ratio and number concentration of cloud particles).

Done

2) p. 3726, last line: the effective ice crystal size is based on Boudala et al. (2002) here, but on Eq. (3) for the computation of cloud optical properties. If, as it seems, the definition is different for the ice cloud microphysics scheme and for ice cloud optical properties, this point should be clarified.

This sentence has become redundant as we were able to implement the cirrus scheme and redid all the simulations. Thus the numbers in the tables and the figures changed

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slightly.

3) p. 3729-3733: It is my impression that the comparison of global and zonal mean values is reported in more detail than necessary to convey the main points of this paper. For example, the comparison to observations is rather inconclusive in many respects.

Yes, that is true. While we feel that this discussion is necessary in order to understand the differences between the different simulations, we shortened it where ever possible.

4) p. 3733, lines 23-26: This sentence implies that the cloud droplet number concentration is larger for ECHAM5 than for ECHAM4, but Fig. 1 tells a different story!

This contradiction has been removed.

5) p. 3735, lines 1-4 (also p. 3737, lines 16-20) and p. 3744 (line 18)): While this may sound like hairsplitting, you should be clear about the physical reason for the larger ice water path. In my understanding, it is not "caused" by the larger ice crystal sizes per se, but by the use of smaller ice crystal aggregation rate. The link to effective ice cystal size comes through tuning of the aggregation rate.

Yes, that is what we meant, we reworded that statement.

6) p. 3738, lines 22-24: I cannot make sense of this sentence. What follows from what?

The sentence has been rewritten.

7) p. 3739, lines 17-19: I think the cause-effect relationship is slightly different here. Increased aerosol concentration reduces autoconversion rate and hence enhances the condensate amount, which leads to increased cloud fraction when using the Tompkins scheme. (A primary difference between the Tompkins scheme and the RH-based scheme is that in the latter, cloud fraction only depends on RH, while in the former, it also increases with increasing condensate amount).

Yes, that is what we implied. We now mention that explicitly.

8) p. 3742, lines 10-12: Is the increase in liquid water path in ECHAM5-RH and ECHAM5-

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COV limited by, or caused by, the increase in convective precipitation in ECHAM5-RH and ECHAM5-COV? I suppose the former, but the sentence is not clear.

The former, we added that.

9) p. 3742, lines 13-27: Is it possible to comment on the relative importance of first and second indirect effects? In particular, the results give the impression that the second indirect effect is much larger for the Tompkins scheme (which is an important and possibly worrisome result) but it should rather be stated explicitly.

Yes, it is (see also response to 10). The cloud lifetime effect is indeed more important in the Tompkins scheme than in the Sundqvist scheme. We added that.

10) A follow-up comment on the previous one: would it be feasible to separate the first and second indirect effects through off-line radiation calculations with (preferably instantaneous) data saved from the GCM simulations? (E.g., it might be possible to get a reasonable estimate for the first indirect effect by performing two sets of calculations: one with preindustrial effective radius, another with effective radius perturbed as shown in Fig. 8).

Yes, we did that. Unfortunately it is not possible to do that with instantaneous data but we did that with monthly mean effective cloud droplet radii. The data are added in Table 3 and in Figure 8.

11) p. 3743, line 1: judging by the numerical values in Fig. 8, the midlatitude reduction in OLR for ECHAM4 might be more related to an increase in LWP rather than IWP.

I don't think so because the OLR still decreases polewards of 70N, where the LWP increase is zero, but where total cloud cover and IWP increase as well.

12) p. 3744, line 5: Based on the values in Table 1 and Table 3, the large difference in aerosol optical depth between ECHAM4 and ECHAM5 results from a gross underestimate of the optical depth of natural aerosols in ECHAM4 (0.02 in ECHAM4 vs. about 0.13 in ECHAM5). It would be good to point out this, either here or somewhere earlier.

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Yes, that's a good point. We added that.

13) p. 3745, lines 11-20: This is not a conclusion of this study! The reasons for not implementing the cirrus scheme should be explained earlier (section 2.2.).

We actually managed to implement the cirrus scheme and redid all the simulations with the cirrus scheme included.

All technical comments have been addressed.

Figures 1,2,7 and 8 are now divided into 2 each but the original Figure 6 is kept as is because it better conveys the message.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3719, 2007.