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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global anthropogenic aerosol effects on convective clouds in ECHAM5-HAM" *by* U. Lohmann

U. Lohmann

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Dear Hans,

Please find my replies in italic below:

The new model set-up ECHAM5-conv is superior to others in some aspects but not in all, reminding us of the need for further tuning or the inclusion of cloud dynamics beyond the bulk mass flux approach used here, plus retuning.

That is a good point; the need for inclusion of cloud dynamics is mentioned in the final paragraph of the revised manuscript.

A number of the parameterisations look quite arbitrary or ad-hoc, so $\sqrt{CAPE}$ in Eq. 3 (although taken from a former publication of the author), the cut-off for precipitation in convective clouds (25 nm), or the use of w_u=0.5 m/s to obtain cloud cover. More

information/discussion would be appreciated here. The Hoose et al (2007) paper is submitted but not published; so a bit more information should be given on the cooling by turbulent motions. Also: what were the inconsistencies in the BF process (this might help interpreting former publications!).

I agree that these numbers appear quite artificially. I added more explanations in the text and below:

1. $\sqrt{CAPE}$*: Elementary parcel theory yields that* U^2=U_0^2 + 2 CAPE *(see Rogers and Yau, 1989). This however is an overestimation because the effects of mixing with environmental air, compensation downdrafts, aerodynamic drag and the weight of the condensate have been neglected. Therefore, Lohmann (2002) used 0.5* $\sqrt{CAPE}$ *as suggested by Leo Donner (personal comm.). This however, is an underestimate because the parameterization used for cloud droplet activation needs a maximum vertical velocity. Thus, I used* $\sqrt{CAPE}$ *as a compromise.*

2. *Cut-off radius of 25 nm in convective clouds: Because the vertical velocity is higher in convective clouds, more and smaller aerosols are activated according to Köhler theory. To take that into account, we potentially allowed all aerosols larger 25 nm to be activated in convective clouds.*

3. w_u*=0.5 m/s: That is a good question. In the standard ECHAM5, the convective cloud cover was set to 5% for the calculation of the evaporation of precipitation and 2 m/s was used to obtain cloud cover for wet scavenging. I do not recall why I used* w_u*=0.5 m/s originally, but I changed* w_u *to 2 m/s for consistency and repeated the simulations.*

4. *Turbulent motions: The turbulent motions are obtained from the TKE as described in the paper for the cloud droplet nucleation process. The Hoose et al. paper is in press now.*

5. *Bergeron-Findeisen process: Inconsistencies meant that there was a bug that we removed.*

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As mentioned above, retuning is probably not yet completed. How can one be sure that autoconversion and aggregation are the right knobs to turn? The sensitivity of the ECHAM5-conv model version seems to be appropriate, even though the means are not always satisfying (see Fig.5).

I agree that the discrepancies between the observed and computed cloud altitudes are quite large. That indeed suggests that something else, like cloud dynamics as you suggested above, needs improvement. I mentioned it here as well. Also, there was an inconsistency in the computation of these cloud amounts, which is now corrected. It does not change the results significantly though.

In the discussion of the relative contribution of large scale and convective precipitation in ECHAM5-conv vs. strat I find a total of c+ls of 85% and 80%, respectively. What about the rest? Or did I get the numbers wrong?

You did get the numbers wrong. The comparison with observations (the second pair of numbers) only refers to the tropics. I have clarified that.

Chapters 3.1 and 3.2 are evaluation, not validation!

The title of the chapters has been changed.

A mass-mixing ratio cannot detrain.

The respective sentence has been rewritten.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14639, 2007.