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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of cloud-borne aerosol representation on aerosol direct and indirect effects" *by* S. J. Ghan and R. C. Easter

Anonymous Referee #1

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1 General comments

The authors present sensitivity studies examining the influence of the representation of cloud-borne aerosol particles on GCM results. As reference case, the MIRAGE model (Easter et al., 2004) is used, which contains a relatively detailed state-of-the-art treatment of cloud-borne particles. Four experiments with simplified treatments of cloud-borne particles are carried out, which are supposed to represent the way other current aerosol-climate models represent cloud-borne particles. The authors reach the conclusion that even though global mean errors for aerosol fields can be significant, the estimates of the aerosol indirect and direct forcing are rather unaffected.

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Interactive Discussion

Discussion Paper

These investigations are original and of great importance to evaluate uncertainties of current state-of-the-art aerosol-climate models. I recommend the article for publication after some revisions.

2 Specific comments

2.1 Abstract:

The abstract is short and overall clear (except page 4342, line 10: “many variables of interest”: this is rather vague). The conclusions concerning radiative effects should be included here as well.

2.2 Section 1:

The compilation of how other models treat cloud-borne particles is very interesting and the authors should go into more detail here, as the sensitivity experiments are meant to span the range of these currently used treatments. In particular, those models which prescribe the fraction of cloud-borne particles do so in different ways and for different processes. Some of the cited models prescribe the fraction of cloud-borne particles only for wet deposition. This fraction either depends on aerosol size, LWC, and cloud type (warm/cold) (ECHAM/MADE), cloud type (stratiform liquid/ice/mixed or convective) (ECHAM5-HAM), or is set to 100% whenever clouds are present (Barth et al., 2000), and so on. In some of these models (e. g. CAM-Oslo and ECHAM/MADE) changes to the aerosol size distribution during cloud evaporation are taken also into account in a simplified way. It should also be mentioned how scattering of sunlight by cloud-borne particles and by interstitial particles in the cloudy fraction of a grid-box is treated (or whether it is treated separately from the rest of the aerosol at all) in the

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cited models. Going into more detail here would increase the relevance of this paper, because it would allow the reader to gain a better understanding of other currently used models in addition to MIRAGE.

2.3 Section 2:

- The treatment of cloud-borne particles in the experiment DIAG does not become very clear. In at least some of the cited models (e.g. Barth et al. (2000), Stier et al. (2005), Lauer et al. (2005)), the cloud-borne fraction is only prescribed when clouds precipitate. For non-precipitating clouds all particles (interstitial and cloud-borne are not differentiated) take part in aerosol microphysical processes like coagulation. Is this the same in DIAG? Please specify. What about radiative properties of interstitial particles in the cloudy fraction of a grid-box? Do they add to the optical depth? This should have an impact on figures 11, 12 and 13. I suspect that treatment of cloud-borne particles in some of the models is even cruder than in the experiment DIAG, and this should be underlined. I suggest to add another sensitivity experiment similar to DIAG but with a more rudimentary treatment of cloud-borne particles, e.g. including cloud-borne particles into the normal aerosol microphysical processes and not including (if DIAG does do so) interstitial particles in the cloudy fraction into calculation of the direct radiative forcing.
- In the simulation P-NOADV, cloud-borne particles do not undergo large-scale transport. What about cloud droplet number? Is this quantity advected, and if yes, doesn't this lead to inconsistencies?
- Ice-phase processes are totally neglected in the P-FULL and all other experiments. Although discussion of the exact treatment is not subject of this paper, this should be mentioned.

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Section 4:

- All results are discussed for annual mean values. I expect that this reduces the errors considerably. How would results look like for monthly mean values?
- Is there a reason why no correlation coefficients are given for the scatter plots?
- page 4349, line 7: ". . . , where aerosols are resuspended when clouds glaciate”: This is not clear to me. Do you mean that aerosols are resuspended in the real atmosphere because of cloud droplet evaporation during the Bergeron-Findeisen process? Is this treated in P-FULL? But then the bias between P-FULL and P-RESUSPEND should be small for these situations. Or do you mean that P-RESUSPEND does actually better represent the truth for these situation than the reference simulation? Please clarify.
- page 4349, line 17: “stratiform cloud wet removal and adjustment are both significantly greater in the DIAG and P-RESUSP simulations.” This is confusing. Shouldn’t these processes be smaller in DIAG as less particles are transferred to the cloud phase (no particle-droplet collisions)?
- page 4350, line 15 ff: This sounds like cloud-borne particle concentrations, summed over all modes, do not equal cloud-droplet concentrations not even in P-FULL. How can this be the case? How good is the agreement when all modes are included? I understand that biases can arise when cloud-borne particles are not advected, but this is a serious model inconsistency as well. Please clarify and comment.
- page 4350, line 23: “... and the P-TOTM treatment also agrees quite well”: From figure 7, it seems like cloud droplet number is systematically underestimated in P-TOTM although this is less the case for CCN@2% in figure 8. Could you explain this?

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- The number of figures could be reduced, as some of the figures 2, 5, 6, 7, 8, 9, 10, 11, 12, 13 give a similar message. I would suggest to omit figure 5 (accumulation mode mass), as it does not give substantially new information. Figure 8 is presented in order to provide the link between cloud droplet number concentration and accumulation mode number concentration. However, the scatter in cloud droplet number concentration is still larger than the scatter in CCN@2%. Therefore, as figure 8 does not provide a satisfactory explanation for the bias in P-RESUSP and DIAG, I would suggest to omit this figure as well.

- page 4362, figure 2: There seems to be a threshold value at 1.5E12 m-2 in P-RESUSP and DIAG. Can you explain this?

3 Technical corrections

- page 4345, line 18: Ovtchinnikov Ghan (2005) is missing in the reference list

- page 4346, line 5: Koch et al. (2006) instead of (2005)

- page 4347, line 6/7: “the same representation of aerosol activation”: For the reader’s convenience, please add which activation parametrization is used.

- page 4351, line 26: To help the reader, please add “the negative sign of the bias is consistent with the positive sign of the bias for aerosol optical depth”

- page 4356, line 21: Correct “Kristjansson1”

- page 4358, table 1: Barth et al. (2000): This paper describes the NCAR CCM3, not the NCAR CAM. This is confusing. Is there a reference for treatment of cloud-borne particles in the NCAR CAM, or is it equal to the treatment in the CCM3?

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EGU

If so, please add. You also should add CAM (Community Atmosphere Model) in order to differentiate it from the Canadian Aerosol Module.

- page 4358, table 1: ECHAM5-HAM instead of ECHAM5 for Stier et al. (2005)
- page 4359, table 2: According to my understanding from the text, these are errors of accumulation mode (and not total) aerosol number, mass, and cloud-borne aerosol number. Please indicate this in this table as well.
- page 4362 ff, figures 2, 5, 6, 7, 8, 9, 10, 11, 12, 13: The x-axis title is drawn quite close to the tick labels.
- page 4364, figure 4: The fonts are not rendered very well.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4341, 2006.

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