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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Explicit representation of subgrid variability in cloud microphysics yields weaker aerosol indirect effect in the ECHAM5-HAM2 climate model" *by* J. Tonttila et al.

Anonymous Referee #3

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In this study, the authors explored how explicit representation of subgrid variability in cloud microphysics using a stochastic subcolumn framework affect estimates of aerosol indirect effect in a climate model, ECHAM5-HAM2. They found that global annual mean aerosol indirect effect decreases by 18%, from -1.59 to -1.30 W m-2. Their founding is interesting, and the paper is also generally well written. However, the differences in the basic states of three configurations examined in the paper need to be better accounted for. The paper can also benefit from more discussions on physical mechanisms involved in differences in simulated LWP change from anthropogenic aerosols in different configurations.

Major comments:

1. The differences in the basic states in three configurations. The differences in the basic states of three configurations are large. For example, LWP is 67.4 g/m2 in ACT and reduces to 50.2 g/m2 in ACACT, about 25% decrease. The same large differences are also true for column-integrated droplet number concentrations. Therefore when the authors discuss the differences in aerosol indirect effects, the large differences in the basic states need to be accounted for. The relative difference therefore may be more meaningful. For example, although increase in in-cloud CDNC at the 890 hPa is the largest in REF (36.42/cm3), with smaller increase in ACT (31.83 /cm3) and ACACT (30.7/cm3), the relative increase (compared to the PI CDNC) is the largest in ACACT (44.5%), with smaller relative increase in REF (41.7%), and Act (40.5%). The same can be applied to LWP and aerosol indirect forcing as well. When the relative differences are used, the picture can be quite different. Accordingly, many discussions in Section 4 and 5 will need to be revised.

2. The physical mechanisms behind simulated changes in anthropogenic aerosol effects on LWP needs to be better understood, especially between ACACT and ACT. The large difference in LWP change from PI to PD between ACACT and ACT is probably one of the most important results of this manuscript, but the reason behind this is not clear at all. On the other hand, if the relative change is used, the difference is much more moderate, and not sure whether the difference is still statistically significant.

Specific comments:

1. line 17, page 15524: Results from climate model simulations that account for cloud-scale motion (Wang et al., 2011, doi:10.5194/acp-11-5431-2011; Wang et al., 2012, doi: 10.1029/2012GL052204) also contribute to the weaker aerosol indirect forcing estimate, as discussed in IPCC AR5. Wang et al. (2012) is particularly relevant to this paper, as that paper is also about understanding aerosol indirect effect differences in different models, and how differences in cloud microphysics might help to explain

model differences.

2. Lines 22-24, page 15525: Not sure I would agree with this statement ("a significant part of model-based overestimation of aerosol indirect effect can be explained by omitting subgrid variability in cloud microphysical processes") and a similar statement in the abstract. I think the paper needs to provide more evidence to support this statement. One thing is that the differences in the basic states are needed to be accounted for. Another thing is that the physical mechanisms that lead to the large reduction of aerosol indirect forcing from ACT to ACACT needs to be further explored.

3. Lines 21-23, page 15526: It is not clear to me how the subgrid distribution of CDNC is purely determined from the subgrid distribution of vertical velocity. To my knowledge, grid-mean CDNC is a prognostic variable in ECHAM5-HAM2, which accounts for both source and sink terms such as droplet activation, advection and precipitation. So how can the PDF of CDNC is directly determined by the PDF of sugbrid vertical velocity?

4. Lines 25-26, page 1526: Please also elaborate how the model account for the correlations between LWC and CDNC in their subcolumn generator (or, more precisely, the correlation between LWC and the subgrid vertical velocity). This can be elaborated either here or in the description of the case of ACACT on page 15527.

5. Page 15527, the case of ACT: Please clarify whether the subgrid CDNC in ACT is used in radiation calculation.

6. Figure 1c and 1d, page 15529: I understand the decrease in CDNC from REF to ACT, as activated droplet number concentration increases non-linearly with increasing vertical velocity and this non-linearity is mainly caused by the competition of water vapor from more activated droplets. However, there is also a significant decrease in CDNC around 60S from REF to ACT. This is not clear to me.

7. Line 20, page 15529: "even slightly increased CDNC". The increase at around 60S is quite significant. What causes this increase?

8. Lines 1-7, page 15530: I am a little bit surprised by the difference in LWP between REF and ACT. The almost identical LWP in the NH is particularly puzzling. Does this mean that LWP in the NH is dominated by those over oceans in your model? Is this result consistent with Tonttila et al. (2013)?

9. Lines 10-17, page 15531: see the major comment #1. As the basic states are quite different in three configurations, the absolute difference in CDNC can be misleading sometimes, and a relative difference can be more meaningful. See the approach used in Wang et al. (2012). If the relative difference is used here, the picture can be quite different (see the major comment #1). Accordingly, many statements and discussions in this section will need to be revised. For example, I do not think you can attribute 80% of this difference to subgrid cloud droplet activation alone. I also do not think you can conclude that "the type of autoconverion is not important for the anthropogenic perturbation in CDNC in our model".

10. Lines 5-15, page 15532: see my last comment and the major comment #1. The same argument can be applied to LWP as well. The relative change in LWP is 11.0% in ACTACT, and 12.8% in ACT. The difference between ACACT and ACT is therefor much moderate than 35% cited in the paper based on the absolute change.

11. Lines 5-15, page 15532: the differences in LWP between ACTAC and ACT clearly needs more explanation. It is not immediately clear to me why accounting for the subgrid variation of LWC and CDNC leads to smaller LWP change in ACACT. Given that the relative difference is now quite moderate, I am not sure whether the relative difference is still statistically significant.

12. Lines 16-23, page 15532: the smaller aerosol indirect effects in ACTAC can be partly explained by the smaller SWCRE in this case (-52.75 W m-2) than in REF (-55.92 W m-2).

Technical corrects:

1. line 12, page 15528: “sortwave” → “shortwave”

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