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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative forcing by aerosols as derived from the AeroCom present-day and pre-industrial simulations" *by* M. Schulz et al.

M. Schulz et al.

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Final author comment to reviewer 2

Paper ACPD acpd-2006-0107 Schulz et al. "Radiative forcing by aerosols as derived from the AeroCom present-day and pre-industrial simulations"

We would like to thank the reviewers for careful reading and the time spent to help clarifying the paper!

General comment: We apologise for sometimes abbreviated responses. This is especially the case, when we have changed the manuscript in the revised version. Note an acronym used in the remainder: RF = radiative forcing.

Answers to specific comments: 1. abstract: forcing values are now given with one digit more.

2. P5099 L3: "in addition to non-linear aerosol dynamics the changes in circulation between present day and 1750 add to the aerosol effect": The sentence is clarified. In case of doubt: This paper, however, is about the direct aerosol radiative forcing, by definition computed as difference between the disturbed and undisturbed case, where the cloud fields and thus circulation are held constant. No indirect effect has been computed thus in experiments B and PRE. Circulation changes were a priori not present in the model experiments.

3. P5100 L6: "what does 'usual' emissions in AeroCom A mean?": In AeroCom experiment A all modellers used their standard best guess of emissions for a simulation of the year 2000 or a climatological mean.

4. P5100 L26: "why regrid to a higher resolution? this suggests a higher degree of spatial resolution.": We disagree that this is a problem to our study. The reason to interpolate to 1x1 degree is a choice intended to facilitate comparison with the satellite data sets available to AeroCom, which are on that resolution. Interpolation in this study is mainly done to create an average model. Note, that the maps of forcing in figures 4-6 use the original resolution of the models!

5. P5102 L9: "Why does the RF calculation methodology differ for individual components?": Thank you for pointing to the section. It is indeed not clear and we have tried to clarify the section. The methodology does not differ between aerosol components, but between models.

6. P5102 L21: Thanks for the proposition to cite papers on non-linear effects of aerosol mixing on RF. We will do.

7. P5104 L12: "what do you mean by 'from individual model results'": We refer to the nine models which have participated.

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8. P5104 L6: "implication of higher uncertainty of forcing efficiency per AOT? Does it mean that optical properties differ more between models?" Reviewer 1 has suggested discussing the reasons for the diversity in forcing efficiency in more detail. This is what we assume this reviewer suggests as well. Other optical properties such as asymmetry parameter and absorption are indeed important and independent of the optical depth.

9. P5106 L22: "is it realistic that BC lifetime is shorter than POM lifetime": Not by first guess. But, yes, in global mean BC can exhibit a shorter lifetime than POM if the BC and POM emissions are differently distributed in space, which they are. This spatial distribution difference would be responsible for BC and POM to experience different removal scenarios. The lifetime of an aerosol particle depends on the place on earth it is emitted. A comment is added.

10. P5106 L28: agreed and thanks for sentence clarification.

11. P5108 L9: "Why does a high positive BCPOM RF imply a relatively large negative POM RF and positive BC RF, couldn't both be small as well?": We agree, the split of the BCPOM RF into carbonaceous aerosol components is problematic. Based on the limited information for MPI_HAM, split information from other models and assuming that RF components add up linearly, there is only the solution we have put in table 4. The text is clarified.

12. P5109 L18: reviewer suggestion followed and text changed.

13. P5110 L10: "why single out LOA?" Inspection of the scatter plots of the RF components and atmospheric forcing suggest that the correlation between the BC RF and atmospheric forcing would be higher if omitting one model (LOA). Given the small number of cases we agree that this is somewhat arbitrary. Reformulated.

14. P5112 first para: "Explain what causes the differences between LSCE and LOA instead of describing it": Unfortunately, we can not explain all differences, because of limited diagnostics available. However, our analysis of LOA and LSCE takes advantage

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of the fact that transport and meteorology in addition to emissions are almost identical in both models. This should allow for additional analysis as we have attempted to do. The differences in cloud sky forcing can be linked to the continental pollution aerosol effects (figure 5). There are considerable differences in sulphate optical thickness (LOA 0.35; LSCE 0.23 Table 2). Negative cloud forcing of scattering aerosols then can be either due to scattering in the presence of thin clouds or be a result of a co-variation of clear sky sulphate optical thickness and cloud cover. More detailed reasoning would require considerable additional analysis. This is now made clear in the text by adding discussion based on the forcing efficiency values documented in tables 2-5.

15. P1113 L5: "Why does the GISS model has a negative all sky forcing, based on a small sulphate RF and positive cloud forcing?": The computations are correct. The sentence is eventually misleading. Note that the GISS model has considerable negative POM RF in addition to sulphate RF. Rephrased and explained.

16. P5115: "Are you sure that the compensation between small sulphate production and high aerosol extinction coefficients results from differences in transport and not from tuning?": It is of course difficult to detect tuning, but we have not found evidence of it. Both in this study and in the analysis of larger set of all 16 AeroCom A models in Kinne et al; ACP 2006, this compensation appears. For clarification we have added a diagnostic from Textor et al. 2006, which indicates the fraction of sulphate mass above 5km, to the sulphate forcing table. This shows more clearly, where the sulphate mass resides in models with a high mass extinction coefficient.

17. P5115: Thanks for the suggestion to restructure the paragraph.

18. P5115: "questions marks behind MPI-HAM and GISS intended or to be answered": The sentence is now made more clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 5095, 2006.

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Interactive Comment