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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 for reviewer 4

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 reviewer 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.

Author comment to Reviewer #4

Answers to general comments:

1. "few references in main text": The text has been looked through again for unjustified statements and references have been added.

2. "AeroCom work on radiative forcing should be put in context of previous work, e.g. IPCC 2001, COSAM. Where do the results differ from previous work.": We believe we have done quite some effort in the direction the reviewer asks for. We have cited most of the major RF papers recently published since IPCC 2001 and have added the available data in the different tables. However, we agree that we have not used all the information available in the IPCC 2001 and COSAM publications for comparison. Where possible, we have thus added crucial numbers for comparison from these two studies.

Answers to specific comments:

1. Abstract L8: "Is the use of the standard deviation rather than a range appropriate? Do the results follow a lognormal distribution?": We have certainly not enough data to claim, that the results follow a lognormal distribution. We suggest, that our manuscript is complete in that it gives all results in the tables and allows thus the reader to derive a range. For crucial parameters we have added the range in the text. A standard deviation has been used also in the first AeroCom paper from Textor et al. 2006 to describe the diversity of the model results. We have added a cautious note on how the standard deviation should be interpreted.

2. Abstract L23: We have now added the exact range of the atmospheric forcing as for the other results.

3. Page 5098 L14: "Are indeed technical difficulties the main reason for misrepresentation of aerosol physics in the models, or is it rather due to unknown physics...": Thanks for pointing to this flawed reasoning.

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4. Section 2: "It would be useful to provide a clearer overview of all AeroCom experiments,.. on species considered, ..on emissions used,...to explain model setups in table?": We now give more reference to the documentation found in the three accompanying papers Textor et al, Kinne et al and Dentener et al., which all appeared in ACP 2006. Emissions for instance are in detail documented in Dentener et al. 2006. Aerosol models and species considered are to be found in Textor et al. 2006. Note that all models used in this study are covered by the other studies. A fairly outstanding documentation is thus achieved, to our opinion. With respect to the specific forcing calculation method details on pages 5102 and 5103 we believe that they are too inhomogeneous to be put into a table. Please note, that detailed documentation is also available on the public web page of AeroCom, which provides access to vast image catalogues from all experiments.

5. Page 5103, L7: "What is the reason for extended lifetime of BC in the SPRINTARS model": The solubility and uptake in precipitation is parameterised differently in the case of externally mixed BC and POM particles. Clarification has been added. The AeroCom emission data set did not provide information on the degree of internal mixing of the carbonaceous particles, but rather emission info on the separate categories BC and POM, leaving the actual mixing parameterisation up to the participants.

6. Results: "what is your definition of model diversity?": We have chosen to define it as standard deviation, as has been done also in the Textor et al 2006 paper.

7. Page 5103 L24: "first model group should be called previous model predictions" The introductory sentences are clarified.

8. Page 5104 L4: "be more precise in referring to 'both model groups'": Thanks for pointing to this, we will do.

9. Page 5104 L9: "How can shifted SO2 emission patterns explain differences in RF? Are total emissions comparable? Are sulphate production rates higher in Asia than in Europe?...": The reviewer rightly asks for clarification. The discussion is enhanced.

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However without dedicated experiments in a single model we will certainly not be able to explain the diagnosed differences between the AeroCom models and the previous model predictions. This is an example of where the intercomparison diagnostics are insufficient. The statement is reformulated to express uncertainties as indicated by the reviewer.

10. P5104 L25: "To support the statement that the difference between LOA and LSCE is due to dry deposition of SO2, provide lifetimes of SO2": The lifetimes of SO2 would not solve the problem, since chemical production and dry deposition determine the lifetime. We only know from AeroCom diagnostics emissions and chemical production of sulphate from SO2. In conclusion: (same answer as for reviewer 1): The underlying AeroCom diagnostics are indeed not sufficient to make the statement as found in the paper that the differences between LOA and LSCE "must be due to different dry deposition of SO2". Dry deposition and chemical loss of anthropogenic SO2 through formation of aerosol sulphate compete in different ways in both models. All parameterisations are slightly different and it is not known to which degree this impacts net sulphate production. From the AeroCom diagnostics we know SO2 emission and chemical production of sulphate and thus implicitly SO2 dry deposition, assuming wet deposition of SO2 is small. However, both reasons for diversity between LOA and LSCE, different dry deposition schemes and different chemical loss schemes, could result in more chemical production in LOA, as we had diagnosed. To resolve such questions one would need to do a more clean experiment within one model or eventually retrieve much more detailed diagnostics.

11. Page 5105 Section 3.2: "Are SOC included in POM": Yes they are, as detailed in Dentener et al 2006.

12. Page 5106 L8: "which ratios, be more specific": Thanks to pointing to this, we will be.

13. Page 5106 L24: "Why are the mass absorption coefficients for BC so different?

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Where are the highest and lowest values from?": The values provided are global mean values, based on global column integrated values of absorption and BC mass loading. Models have derived absorption based on internal and external mixing assumptions, different effective refractive index and have used quite different aerosol size to translate BC mass into absorption. We have so far not collected enough information to explain the differences.

14. Page 5106 L26: "both model groups, which are these?": We refer to the AeroCom models and the previous model predictions.

15. Page 5106 L29: "Is the BCPOM RF difference between 0.14 and 0.26 'slightly less important'?": The difference is indeed almost a factor of two. Revised formulation now used.

16. Page 5107 L2: "How much smaller are the biomass burning emissions in AeroCom? Biomass burning aerosol optical thickness would be a better diagnostic.": We agree that the optical thickness from the biomass burning aerosol would be an excellent diagnostic. Such source related diagnostic was not available in AeroCom unfortunately. An attempt is made now by looking into regions dominated by biomass burning. Discussion of actual emission amount differences is added.

17. Page 5108 section 3.3: "A boxplot showing the range of forcing results for land/ocean would be useful": As has been responded to reviewer 1, such separation of land and ocean values is now added in form of a table.

18. Page 5108 L28: "Constant emissions create a low limit of diversity": We have added further remarks on the uncertainty due to unknown emissions. However, this was noted already in the manuscript. Additional analysis from AeroCom A and B experiments shows that the emissions assumption is not the dominant component explaining the overall diversity between models.

19. Page 5109 L11: thanks, sentence clarified.

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20. Page 5109 L12: thanks, 'northern high latitudes' describes the region much better.

21. Page 5109 L14: "Does external mixing of BC/other aerosols really results in more positive forcing compared to internal mixing assumptions. Please give evidence or reference for it.": Thanks, statement can not be made this way. SPRINTARS assumptions are clarified.

22. Page 5109 L20: "What is the diversity of absorption optical thickness?" => 41% as documented in table 3, last column. Discussion added.

23. Page 5110 L10: "this implies that the correlation between absorption and BC-RF is particularly low for the LOA model. Correlation for POM-RF of 0.36 is not much lower than that for BC-RF. BC and POM are not treated separately in the models anyway." Agreed, also reviewer 2 has pointed to this. We have removed the mentioning of LOA. We also agree that BC-RF and POM-RF correlations to atmospheric forcing are not that different. BC and POM are indeed treated pretty similar in the models. Even their lifetimes are correlated.

24. Page 5110 L12: "Any idea on how to carry out measurements of atmospheric forcing? The knowledge of the total radiative forcing would reduce the need for determining the forcing components." Measuring in more detail the radiative fluxes at different levels in the troposphere has been used to derive the atmospheric forcing due to absorbing aerosols (e.g. in the INDOEX campaign). We do not understand fully the remark of the reviewer, that knowledge of total RF would reduce the need to understand the forcing components. A better understanding of forcing components would allow for a bottom-up modelling from emission sources to anthropogenic forcing. This has been clarified now in the text.

25. Page 5111 L7: "Positive clear sky forcing by ULAQ over the ocean cannot be explained by strong absorption, given the low ocean albedo." Thanks for pointing to this. The discussion of the ULAQ results has been enhanced by adding information on the surface albedo assumptions. It is indeed an outlier result with respect to the clear

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sky forcing.

26. Page 5111 L18: "warming versus positive forcing...": thanks, this should not be used as we did.

27. Page 5111 L28: "don't understand sentence": What was meant: Cloud skies cover 70% of the earth surface and weigh more when combined with clear sky forcing to obtain the all-sky forcing. Clarified.

28. Page 5112 L2: "Is there a physical explanation of negative cloud sky forcing?": The most important explanation would be scattering of aerosols below and above thin clouds. Discussion is reformulated.

29. Figures 4-5-6: "Why are not all results shown for UIO and ULAQ": UIO-GCM indeed reported only all-sky results. ULAQ reported only clear-sky results. As explained now better, cloud-sky forcing for ULAQ has been assumed to be zero, (see also discussion in response to reviewer 1). Maps for ULAQ with respect to all sky and especially cloud sky forcing would not be supported by detailed computations and are omitted for clarity of the available data.

30. Page 5113 L8: "UIO GCM and ULAQ authors are co-authors and are asked for explanation for low all sky forcing results": Additional documentation has been added.

31. Page 5114 L21: "search better expression for confusion..": thanks, reformulated.

32. Page 5115 L5ff: "What are the hygroscopic growth rates for the different models? Are the models calibrated to match observed oceanic or Aeronet AOT's?": We can not answer to this in all detail. What would have been useful is a diagnostic of the anthropogenic aerosol water load. However, that is not available. Aerosol water has been documented for AeroCom A in Textor et al. 2006 and is indeed largely different. As has been said in response to reviewer 2: 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.

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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.

33. Page 5115 L19: "Reasons for diversity in forcing efficiencies are not clear... co-authors should provide clarification". As has been responded to reviewer 1, additional discussion has been added with respect to 'forcing efficiency differences'.

34. Page 5115 L29: "Is aerosol water is diagnosed in AeroCom A? This may be an indicator for its importance in the other AeroCom experiments.": In addition to response above: Since aerosol water is largely linked to natural sea salt loads, such diagnostic is insufficient. As said above, we would need to diagnose the anthropogenic aerosol water, which is unfortunately missing. The discussion, though, has been clarified.

35. Page 5116 L15: "global averages hide regional affects": Agreed, the regional impact of the aerosol can be large.

36. Page 5117 Figure 9: "Additional maps of MEC and NRF as well as their standard deviations would be very useful for the interpretation of results": Maps have been added.

37. Page 5118 L7: yes - meant was "one order of magnitude".

38. Page 5118 L15: "'Furthermore, the harmonised AeroCom emissions did not reduce model diversity'. Can this be supported by numbers. What is the difference to Experiment A. The assumption of fixed emission is an idealization": The statement is indeed too strong, since RF from the same set of model versions in experiment A is not available. The statement is founded on a comparison of other parameters such as optical properties and mass loads and a comparison to diversity documented for AeroCom A experiments in Textor et al and Kinne et al 2006. Also the comparison to RF in previous model predictions does not indicate a smaller diversity in AeroCom B-PRE

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RF estimates. Statement is however revised.

39. Page 5118: "TOA radiative forcing may be smaller than greenhouse forcing, but surface forcing might change hydrological cycle": Agreed.

40. table 2,3,4: "For the previous results missing values for lifetimes and MEC could be given. At least for those models, where authors are among co-authors of this manuscript": We will try our best to complete the table in more detail.

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