

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

Anonymous Referee #4

Received and published: 6 August 2006

The paper is a summary of the direct radiative forcing modelling results computed with state-of-the-art global aerosol models, which is an outcome from the AEROCOM global aerosol model intercomparison exercise. Only model results for the case of fixed aerosol or precursor emissions are considered here, resulting in a minimum estimate for the variability in the model results for direct aerosol radiative forcing. The paper is acceptable for publication in ACP after some revisions.

General comments:

1. The main text contains only few references, please ensure that statements are supported by the appropriate references.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

2. The AEROCOM work on radiative forcing estimates should be put into context of previous global aerosol model intercomparison results, in particular the results on aerosol forcing presented in the IPCC 2001 report, and the COSAM results for the case of sulphate aerosols. Throughout the paper it would be useful to learn where the new work differs from the earlier intercomparisons (rather than just citing results of individual papers).

Most of the more specific comments are requirements to be more precise in the descriptions of the various methods and results, and to either support statements for explanation of different model results by references, or to be more careful in hypothesizing reasons for model discrepancies.

Specific comments:

1. Abstract, lines 8/9: Is the use of standard deviation (rather than giving a range of results) appropriate, i.e. do the results follow a normal distribution?
2. Abstract, line 23: Why not give exact range for atmospheric forcing as done for the other forcing estimates?
3. Page 5098/lines 14/15: Are indeed 'technical difficulties' as stated in the text the main reason for misrepresentation of aerosol physics in the model, or are these rather problems of either unknown physics, missing model input data, inappropriate parameterization, or insufficient model resolution? Please be precise.
4. Section 2 (Model simulations): It would be useful to provide a clearer overview of the AEROCOM experiments, i.e. what is AEROCOM A, what are all species considered in the experiments; and to give the emissions used for experiment B in form of a table for easier comparison to results from other publications. Also, the different model setups detailed on pages 5102/5103 could be added to Table 1 for a better overview.
5. Page 5103, line 7: What is the reason for the extended BC lifetime in SPRINTARS following separate treatment of BC and POM?

6. Results: What is your definition of 'Model diversity'? Range or standard deviation or something else?
7. Page 5103/line 24- should be 'previous model predictions'
8. Page 5104/line 4: 'Both groups of model predictions' - please be more precise in the formulation as to which 'both groups'
9. Page 5104/line 9: How can the shifted emission patterns explain the different optical thicknesses - and thus forcings - compared to earlier results? Are total emissions comparable? Are the production rates higher in Asia than Europe? Different removal rates? Cloud cover? Please give evidence to support this statement.
10. Page 5104/ line25: To support the statement that the difference between LOA and LSCE models is caused by different dry deposition schemes for SO₂, please provide the atmospheric lifetimes for SO₂ in the two models.
11. Page 5105, Section 3.2 - Are SOC included in POM, or are these only primary emissions?
12. Page 5106/line 8: Ratios between what? Please be specific.
13. Page 5106, line 24: Where are the mass absorption coefficients for BC so different? Where are the lowest/highest values from?
14. Page 5106/line 26: 'both model groups' - which are these?
15. Page 5106/line 29: I would not call a difference between 0.14 and 0.26 'slightly less important', as this is almost a factor of two difference!
16. Page 5107, line 2: How much smaller are the biomass burning emissions compared to the average, and is this sufficient to explain the difference? As TOA forcing for BCPOM will show some cancellation of positive and negative forcing over bright/dark surfaces, the differences in optical thicknesses would be a better quantitative measure of the difference to previous results.

17. Page 2108, section 3.3: A box plot showing the range of forcing results for individual continents as well as land/ocean values would be useful for interpretation of the intercomparison results.
18. Page 5108/line 28: Keep in mind that these results were obtained with constant emissions, thus the low standard deviation is a lower limit. In particular the emissions of BC are highly uncertain, and it is doubtful that even for Experiment A the range of emissions reflect the 'real' uncertainty range for this species.
19. Page 5109, line 11: This sentence would make more sense without the comma.
20. Page 5109, line 12: Northern regions=northern high latitudes.
21. Page 5109, line 14: Does external mixing of BC/other aerosols really result in more positive forcing compared to internal mixing assumption? Please give a reference or evidence for this.
22. Page 5109, line 20: What is the diversity for absorption optical thickness?
23. Page 5110, line 10: This implies that the correlation for absorption and BC-RF is particularly low for the LOA model. What is the reason? Line 12: Correlation with POM of 0.36 is not that much lower than for BC - and it seems that BC and POM are not really treated separately in the models anyway? Please comment.
24. Page 5110, line 12: Any ideas how to carry out such suggested measurements of radiative forcing? In fact the goal of the model computations of global aerosol distributions is to understand its radiative forcing and climate effect, rather than the other way; the knowledge of total radiative forcing would reduce the need for determining the individual forcing components.
25. Page 5111, line 7: The positive forcing over oceans shown in the ULAQ results cannot really be explained even by very strong absorption, given the very low ocean albedo. The all-sky forcing (Figure 6) in the areas of clear-sky positive forcing for the ULAQ experiment is less positive than the clear-sky forcing itself. This has to be

explained. In particular as in Figure 2 it does not seem that the positive BC in the ULAQ model is larger than other models, it is actually relatively low compared to the other results. The reason for this positive clear-sky forcing over the ocean must be explained. E.g., is there anything different with the surface albedo in ULAQ?

26. Page 5111, line 18 warming=positive forcing by absorbing aerosol (please don't confuse)

27. Page 5111, line 28: I don't understand this sentence.

28. Page 5112, line 2/line 15: It is not made clear how cloud-sky forcing can be negative (is there a physical explanation?), other than this being just an artefact of the calculation.

29. Figures 4/5/6: Why are not all results shown for the UIO and ULAQ results? Presumably UIO only reported all-sky results, but again, what is the reason for not showing the cloud-sky results for ULAQ? Maybe this would provide a hint towards the strange clear-sky results for this model.

30. Page 5113, line 8 - The diagnostics may not be provided, but as far as I can see the authors of those models are co-authors on this manuscript, and are asked to provide an explanation.

31. Page 5114, line 21 Please find better expression for 'confusion'

32. Page 5115, line 5ff: What are the growth rates for the different models? Are the models perhaps calibrated to match observed oceanic or AERONET AOTs?

33. Page 5115, line 19: 'The reason is not clear' - If you list the assumptions for dry size, and assumptions for hygroscopic growth, the differences should be better understood. Again, the model authors are co-authors on this manuscript and are asked to provide the necessary information.

34. Page 5115, line 29: Is aerosol water diagnosed in AEROCOM Experiment A? This

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

may offer an indicator for its importance in the other experiments.

35. Page 5116, line 15: When giving global mean average RF results please keep in mind that these contain regional cancellations.

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

37. Page 5118, line 7: 'one order' = 'one order of magnitude'

38. Page 5118, line 15: 'Did not reduce model diversity': Can this statement be supported by numbers? I.e., what is the difference to the experiment A? Please make clear that the assumption of fixed emissions is an idealization.

39. Page 5118: Note that the TOA direct radiative forcing may indeed be much smaller than the greenhouse forcing, but surface forcing by aerosols is considerable, and may e.g. change the hydrological cycle.

40. Tables 2,3,4: For the previous results (non-AEROCOM) the missing values e.g. for atmospheric lifetimes and MEC could be given at least for some of the models, as several of the authors of those models are also authors of this manuscripts and should be able to provide the information, even if the numbers are not given in the publications.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper