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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 1

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.

Answers to general comments: The reviewer suggests to further "pin down the underlying factors concerning the differences in forcing efficiency". We agree with this suggestion and have reinforced the discussion section in all subsections of chapter 3. There are indeed data reported in the tables and in the two accompanying papers which were not adequately discussed.

We have now computed also the land and ocean values of the RF components in an additional table.

We also have now added a section to compare with observation based estimates of the radiative forcing.

Answers to specific comments:

P5097 L6 Yes - the RF is for solar only.

P5097 L15 yes - factors which are specifically acting on the clear-sky forcing efficiency are also important. The sentence is changed.

P5097 L20 yes - the statement "by opposite mass extinction coefficients" Was not clear and was changed.

P5099 L2 "...how good are the models capturing interactions between natural and anthropogenic aerosols and the non-linear aerosol dynamics?" We think that the reviewer asks for some validation of the aerosol dynamics described in the different models. This is unfortunately not possible to do at this point. The coupling of the different meteorological factors and dynamic processes of the aerosol life cycle is complex and we have not found a way to pin down the quality and role of non-linear aerosol dynamics modelling. A word of caution has been added.

P5099 L5 "is it possible to discuss and estimate uncertainties associated with emissions?" While emissions are admittedly a very important problem, we think that discussing them in detail is beyond the scope of this paper. In view of the extensive literature on the topic we prefer to refer to recent publications. Some relevant references

ACPD
6, S3216–S3220, 2006

EGU

have been added.

P5101 L17 "aerosol load and optical depth are not adequate for examining model diversity of RF" This sentence was indeed too simple! Thanks for pointing to it.

P5101 L14 Data and references for dust and nitrate forcing have been added.

P5103 L14 "the positive forcing of ULAQ may be amplified, if realistic cloud fields would have been applied. The all-sky forcing of ULAQ should be removed": We agree that there is indeed a good chance that the ULAQ aerosol fields, if introduced in a model with realistic clouds, would produce a positive cloud-sky-forcing. However, we have not done this and must resort to another solution. Note, that we have replaced also other values in the tables by "reconstructed" estimates to provide a more complete picture. All-sky forcing is certainly a key target number to be compared and we thus wanted to include ULAQ in this. We think we have chosen a conservative approach in attributing zero cloud-sky forcing to ULAQ. Looking at the other models with complete information reveals that out of 7 models, 3 suggest negative and 4 slightly positive cloud-sky-forcing. There is no consensus on the sign of this forcing. As discussed in length in this paper the contribution to direct aerosol RF from cloudy-skies is an uncertain contribution, with little observational constraints. Omitting the ULAQ all-sky results would have changed the average AeroCom RF estimate from -0.184 to -0.197 W m-2. This is certainly insignificant as compared to other uncertainties. The discussion section is revised in order to emphasize the limited value of the ULAQ all-sky forcing.

P5103 L22 AOD is always at 550 nm

P5104 L16 "prescribed emissions do not produce a significantly larger agreement among models': such conclusion can only be drawn from two sets of AeroCom models, running with different sets of emissions. Comparing AeroCom models with recent publications is influenced by other factors". We agree. Here we show just that a reduction in diversity is not found right away when harmonising emissions across models. A proof for the limited importance of current emission diversity should come from analysing

ACPD

6, S3216–S3220, 2006

AeroCom A and B simulations. We compare now also some key quantities of the 9 models which had been documented in the two accompanying papers which correspond to AeroCom A. A more careful phrasing has been adopted here. Note also, that the 2nd reviewer found this finding interesting.

P5104 L25 "how different is the dry deposition scheme of SO2 for LOA and LSCE? " 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.

P5104 L25 "How is the fine mode SO4 in the GISS model defined?" This notation refers to the part of the sulphate which is not on the dust particles.

P5104 L25 "cite a paper showing that GISS simulates significant SO2 loss on mineral dust": Bauer S. E., D. Koch (2005), Impact of heterogeneous sulphate formation at mineral dust surfaces on aerosol loads and radiative forcing in the Goddard Institute for Space Studies general circulation model, J. Geophys. Res., 110, D17202, doi:10.1029/2005JD005870. They find that "the global annual mean burden for SO2 is reduced from 0.66 Tg S to 0.46 Tg S".

P5111 L4 "evidence for large differences in desert albedo among models?": versus "can differences in single scattering albedo also contribute?" The statement was in-

ACPD
6, S3216–S3220, 2006

deed over-precise. The gathered information in the AeroCom database on the surface albedo is insufficient. Ideally we should have gathered the effective surface albedo over the course of a year and diurnal cycles. The statement is corrected.

P5111 L6 “reference Kinne 2006 and Textor 2006 to discuss the aerosol absorption in the ULAQ B and PRE runs”: There is no referencing possible, since both papers do not discuss the AeroCom B and PRE simulations. However, table 3 of this paper shows the respective values. More discussion is added. Otherwise, as mentioned above, additional discussion on the reasons for diversity in RF efficiency is added.

p5111 L26 "should all-sky forcing equal the sum of clear sky forcing + cloud sky forcing" ?: Thank you for pointing to it. This is indeed an error due to not correctly doing the global average. A correct area weighting procedure is employed now.

p5114 L7 "could differences in surface albedo contribute to diversity?" yes, see our comment above to P5111 L4

p5116 L9 The suggestions to add more discussion with respect to over ocean values of clear-sky RF are welcomed. These values are now added in a new table and a paragraph has been added to discuss differences.

p5116 L15 “Does aerosol has "a limited impact on climate"”: We agree, the statement should be altered.

p5118 L4 We emphasize now more on the surface and the atmospheric forcing.

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