

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 #1

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General comments:

The paper by Schulz et al. describes an intercomparison of model-based assessments of the aerosol direct radiative (solar) forcing (RF). Nine global models with detailed but different aerosol modules were initiated with the same emissions. The RF at the top of the atmosphere (TOA) is estimated to be $-0.2 \pm 0.2 \text{ Wm}^{-2}$. But aerosol impacts on the surface energy budget and atmospheric heating are much greater, with a forcing of -1.03 Wm^{-2} and $+0.85 \text{ Wm}^{-2}$, respectively. Factors contributing to model RF diversity are discussed through examining several diagnostics.

The topic is well suited for publication in Atmospheric Chemistry and Physics. This is

the third paper resulting from AeroCom initiative that is documenting and intercomparing a large number of models and observations to identify and reduce the uncertainty in current global assessments of aerosols and their radiative impacts. The subject matter is of great scientific significance because the aerosol forcing is one of the most uncertain components of climate forcing. Model simulation is an indispensable tool for estimating past aerosol forcing and projecting future climate change. Thus assessing capability of global aerosol models is an important step toward better understanding of past climate and projection of future climate change.

I would suggest that the paper should be published after minor revisions. The paper is well structured and clear. There are a few weaknesses to the paper, none of which should prevent the paper being published in ACP. But addressing them should improve the paper. Although the forcing efficiency (with respect to optical depth) is a useful diagnostic, it is necessary to go further and pin down the underlying factors determining the forcing efficiency (e.g., aerosol single-scattering albedo, phase function or asymmetry factor, vertical distribution, surface albedos, and cloud profiles). Discussion on model diversity of these underlying factors should be enhanced in the paper. For this, findings from two previous AeroCom papers (Textor et al., 2006; Kinne et al., 2006) can be and should be used. The current version of the paper does not link the three papers well. It is surprising to find no single reference to these two papers when discussing the model diversity of RF in section 3. For the aerosol radiative forcing, giving only annual global average is not enough. At least the paper should present the forcing values land and ocean separately. The separation would also help comparisons with satellite measurements, because “the satellite observations are more reliable over the ocean” (p.5116). Although the focus of the paper is models, it is helpful to build some connections with measurements. For example, in “Summary and conclusions” section they may discuss how the model results compare with a few measurement based estimates of anthropogenic aerosol optical depth and forcing (e.g., Kaufman et al., 2005; Bellouin et al., 2005).

Specific comments:

Abstract, p.5097, L6-8: please specify that the RF is for solar radiation only.

Abstract, p.5097, L15-18: According to Figure 8 and Table 5, the clear-sky forcing efficiency (forcing per unit optical depth) has diversity comparable to that for the all-sky/clear-sky forcing ratio. So factors determining the clear-sky forcing efficiency, such as aerosol absorption, size, and surface albedo, should also contribute significantly to the model diversity.

Abstract, p.5097, L20: “by opposite mass extinction coefficients” should be “by opposite differences in mass extinction coefficients”?

p.5099, L2-3: The statement is correct. But how are the models used in this paper doing their job in capturing interactions between anthropogenic aerosol and natural aerosol and the non-linear aerosol dynamics?

p.5099, L5-6: Is it possible to discuss and estimate uncertainties associate with emissions in the summary section?

p.5101, L17-19: aerosol load and optical depth are not adequate for examining model diversity of RF. Aerosol absorption and size determining the forcing efficiency are also very useful parameters but not discussed adequately in the paper.

p.5101, L24-26: It will be helpful to give a range of anthropogenic contribution of nitrate and dust based on some published studies

p.5103, L14-16: It may not be a good idea to convert ULAQ clear-sky forcing to all-sky forcing by multiplying 0.3. While 0.3 is a reasonable estimate for the clear-sky fraction on global average, the clear-sky fraction has large spatial and temporal variability. Another concern is the underlying assumption of zero RF in cloudy conditions would introduce very large uncertainties if the clear-sky RF is positive (Figure 4, right-bottom). The positive forcing may be magnified, not reduced in cloudy conditions. I strongly suggest that the ULAQ all-sky forcing be removed from the paper.

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p.5103, L22, please specify that AOD is derived at a wavelength of 550 nm throughout the paper.

p.5104, L16-17: “From this we can conclude that the prescribed emissions in AeroCom do not produce a significantly larger agreement among models”. The conclusion is a little bit shaky. Ideally such conclusion can only be drawn from a comparison of two runs of the same AeroCom models, one with the prescribed emissions and the other using emissions as usual. When comparing AeroCom models with recent publications, factors other than emissions may have introduced complications.

p.5104, L25-28: How different is the dry deposition scheme of SO₂ between LOA and LSCE? Do Textor et al. (2006) discuss such differences? If so, the paper should be cited here. How is fine-mode sulfate defined? Please cite a paper showing that GISS model simulates a significant loss of SO₂ on mineral dust.

p.5111, L4-6: Can they provide some evidence of large differences in desert solar albedo assumptions among models? Can differences in aerosol single-scattering albedo contribute?

p.5111, L6-7: Do Kinne et al. (2006) and Textor et al. (2006) show an overestimation of aerosol absorption by ULAQ model? I believe that findings from these two AEROCOM papers should provide some guideline and evidence for explaining model differences in RF. However, this paper doesn't refer to the two papers when discussing model RF diversity.

p.5111, L26: It seems that Fig. 6 should be Fig.7. According to Fig. 7 caption (p.5134), the total forcing is a sum of clear-sky forcing and cloudy-sky forcing weighted by clear-sky fraction and cloudy-sky fraction, respectively. I assume that RED=BLUE+GRAY. But it appears that the relationship does not hold for some models.

p.5114, L7-11: Do differences in surface albedo contribute to the diversity?

p.5116, L9-14: While it is challenging, they can still make some efforts to compare mod-

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els with satellites. For example, Bellouin et al. (2005) use the anthropogenic fraction of AOD from five AEROCOM models to calculate clear-sky RF over land. So it is possible and useful to discuss differences between the two studies. It is true that the satellite observations (clear-sky conditions only) are more reliable over ocean than over land. But if averages of AOD and RF are derived separately for land and ocean (I strongly suggest the authors do this way), they can at least do some over-ocean satellite-model inter-comparisons. I would suggest they put values of land, ocean, and global average in figure titles of Fig.9 and discuss comparisons with satellite observations in the text (Kaufman et al., 2005; Bellouin et al., 2005).

p.5116, L15-16: Aerosol impact on climate depends not only on the TOA RF, but also on surface forcing and atmospheric forcing (much greater than the TOA RF). So a small TOA RF (-0.2Wm^{-2}) does not necessarily suggest “a limited impact on climate”.

p.5118, L4-6: need to emphasize that the surface forcing and atmospheric forcing are much greater than the TOA RF.

References:

Bellouin N, Boucher O, Haywood J, et al., Global estimate of aerosol direct radiative forcing from satellite measurements, *Nature*, 438 (7071): 1138-1141, doi:10.1038/nature04348, 2005.

Kaufman, Y.J., O. Boucher, D. Tanré, M. Chin, L.A. Remer, and T. Takemura, Aerosol anthropogenic component estimated from satellite data. *Geophys. Res. Lett.* 32, L17804, doi:10/1029/2005GL023125, 2005.

Kinne, S., Schulz, M., Textor, C., et al.: An AeroCom initial assessment – optical properties in aerosol component modules of global models, *Atmos. Chem. Phys.*, 6, 1815-1834, 2006.

Textor, O., M. Schulz, S. Guibert, et al., Analysis and quantification of the diversities of aerosol life cycles within AeroCom, *Atmos. Chem. Phys.*, 6, 1777-1813, 2006.

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