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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative forcing of the direct aerosol effect using a multi-observation approach" *by* G. Myhre et al.

G. Myhre et al.

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We thank the reviewer for the positive evaluation of our manuscript.

Review of "Radiative forcing of the direct aerosol effect using a multi-observation approach" by Myhre et al., submitted to Atmos. Chem. Phys. The paper presents a thorough comparison of the Oslo chemistry transport model against ground-based and satellite measurements, and gives the aerosol direct forcing estimated by that model. The model performance, demonstrated by what is certainly the most extensive comparison against observations ever achieved, is very impressive. Comparing model and satellites on an equal basis against AEROCOM is really interesting. The study also reveals the importance of anthropogenic secondary-organic carbon aerosols, which are not included in most general-circulation models. In fact, the Oslo CTM2 could very well be used as a reference for assessing the aerosol modeling in climate models. The

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paper is well written and organised, and I've only minor comments. The only disappointing point is that the deficiencies identified by the comparison rarely translate into suggestions for improving specific aspects of the aerosol modelling.

Response: We thank the reviewer for the positive evaluation of our manuscript

Specific comments - The title of the paper is slightly misleading, as the forcing estimate is derived from a numerical model, not observations.

Response: We have changed the title to: 'Modelled Rradiative forcing of the direct aerosol effect using awith multi-observation approachvalidation evaluation'

- Page 12828, line 26: What is the reason for decreasing the single-scattering albedo of fresh biomass-burning aerosols? What is the new value?

Response: We have included the following sentence: 'The single scattering albedo of biomass burning aerosols is now more in line with recent measurements from DABEX (Haywood et al., 2008; Johnson et al., 2008) with values slightly above 0.8.'

- Page 12830, line 12: "EC and OC [...] challenging." I do not understand this statement.

Response: Using Thermal- Optical analysis (TOA) it is assumed that OC does not absorb light at the specific wavelength of the laser constantly monitoring the blackness of the filter, and that BC/EC is the only light absorbing carbon. During analysis, however, transformation of OC to EC/BC can occur due to charring of OC, and this is observed as a further blackening of the filter by the monitoring laser during the part of the analysis when OC is quantified. This charred OC needs to be separated from the initial EC/BC content and this leads to a great uncertainty in the split of OC and true EC/BC. The following sentences: 'To separate EC and OC is analytically challenging, hence introducing substantial uncertainty to the observed concentrations of EC and OC. Differences between the most commonly used analytical approaches can be extensive, i.e. $>$2 for EC (Schmid et al., 2001).' is suggested to replace the two original ones;

ACPD

8, S10017–S10020, 2008

Interactive Comment

i.e. EC and OC are both operationally defined and separating them is analytically challenging. Thus, differences between the most commonly used analytical approaches can be substantial, i.e. $>$2 for EC (Schmid et al., 2001).

- Page 12831, line 11: Why didn't the authors use the meteorology for each campaign? Doing so might improve the comparison, and in any case make the interpretation of results easier.

Response: Unfortunately we do not have all the meteorological data available. Furthermore the model is quite time consuming to run on a 1x1 degree resolution with so many aerosol components and chemistry components.

- Page 12833, section 3.2: The methodology should be given at the start of the section, rather than scattered throughout. If I understand well, the comparison is done on daily means, without trying to match satellite overpass times? Are the AERONET data level 2.0 for algorithm version 2?

Response: A sentence is included stating that 'AERONET Level 2 and algorithm version 2 is adopted throughout this study.' Some of the text for the satellite comparison is moved to the start of the section

- Page 12834, lines 20 and 24: A change of 10% is not really a minor difference.

Response: The sentence is slightly reworded to 'resulted in only minor differences with only a few exceptions'

- Page 12835, line 12: Figure 6 is very original. Could the authors define a threshold for correlation coefficients, below which the model/satellite performance might be deemed unsatisfactory?

Response: This is useful comment, but we are unable to quantify such a threshold

- Page 12836, line 10: Forth should be written FORTH, as it is not the name of a location but stands for "Foundation for Research and Technology - Hellas".

ACPD

8, S10017–S10020, 2008

Response: Changed as suggested

- Page 12837, line 23: "In terms of RF, the underestimation [...] in East Asia [...] is the most important". Why is it more important than errors in the biomass-burning regions?

Response: The following is added to the manuscript: 'since most of the aerosols in this region are of anthropogenic origin'

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 12823, 2008.