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Comment

## ***Interactive comment on “Aerosol direct radiative forcing based on GEOS-Chem-APM and uncertainties” by X. Ma et al.***

**Anonymous Referee #1**

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### General Comments

The manuscript provides predictions of aerosol direct radiative forcing (DRF) using the GEOS-Chem-APM global chemical transport model extended with aerosol microphysics, coupled with a radiative transfer model. These modelled DRF values are shown to fall within the range of values obtained during the AEROCOM model inter-comparison project. The authors also show the sensitivity of the anthropogenic DRF to assumptions of aerosol mixing state, black carbon and primary organic carbon particle size, and black carbon density.

The effect of anthropogenic aerosols on climate, in particular carbonaceous aerosols, continues to be difficult to precisely quantify, and this work provides a welcome contribution. The manuscript is recommended to be published provided the following con-

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cerns are addressed:

### Specific comments

p. 196, line 28: The authors state here that the aerosol mixing state is critical for the aerosol burden. However, it is not clear to me that the mixing state will significantly impact the aerosol burden, except perhaps through changes in aerosol removal rates through wet deposition. If the aerosol burden was significantly different between the base run and the NoCoat experiments (or either of the other experiments), the change in aerosol burden should be reported, so that the effects on radiative forcing due to changes in aerosol mixing state (or size or density) are not confounded with effects due to changes in aerosol burden. As the manuscript is currently written, it is implied that aerosol burden did not differ significantly between the sensitivity studies and the base run.

p. 205, lines 2-3: Here it is implied that the only possible reason for the disagreement between the modelled BC concentrations and the observations is that there are deficiencies in the BC emission inventory. In the summary, it is correctly stated that parameterizations of physical processes could also contribute to the discrepancies. That information should also appear here.

p. 209, lines 15-19: The authors state that “the DRF at TOA is higher in MAM and JJA than in SON and DJF, due to low oxidation concentrations and oxidation rates in the latter two seasons”. I have two minor comments and one major comment about this sentence: First, because the global mean anthropogenic DRF is negative, I find the use of the word “higher” ambiguous in this context. I assume that by “higher” the authors mean more strongly cooling. Secondly, the oxidation concentrations and oxidation rates would be lower during SON and DJF in the northern hemisphere only; In the southern hemisphere, oxidation concentrations and oxidation rates would be lower during MAM and JJA. Global DRF is impacted to a greater extent by oxidation rates in the northern hemisphere than the southern hemisphere due to the larger concen-

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trations of aerosol precursors in the northern hemisphere. This should be made clear. Most importantly, this statement does not agree with what is shown in Fig. 10, where a minimum in TOA cooling during JJA is visible. I do not feel that this minimum has been adequately explained here or elsewhere in the text.

Sect. 3.2: The AOD predicted by GEOS-Chem-APM shown in Fig. 3 has a significant maximum over Europe and Northern Asia that the authors attribute to industrial fossil fuel emissions. However, this feature is evident for only one measurement site in the AERONET data shown, and is not visible in either the MODIS or MISR data. This apparent disagreement between the model predictions and the observations seems significant and should be discussed.

Sect. 4.2.1: Despite describing in detail a sensitivity study of the effects of BC and POC particle size on anthropogenic DRF, I do not believe that the initial assumed sizes used in this study of BC or POC particles were ever explicitly stated in the manuscript. These should be stated early in the manuscript, before results are given; See my second technical correction.

Table 3: I assume that the values given for POC aerosol burden were derived from the values listed for particulate organic matter in Table 3 of Schulz et al. (2006). Please detail how the conversion from POM to POC was made.

### Technical Corrections

There remain numerous spelling errors in the current manuscript. Most concerning are the errors in the names of authors referenced in the manuscript, for example “Lesis” instead of “Lesins”, “Seifeld” and “Seindeld” instead of “Seinfeld”, and “Schultz” instead of “Schulz”. In addition, the reference to the work of Murphy et al. (1998) seems to have an incorrect title and volume number. A reference to Koch et al, (2010) appears in the caption of Fig. 2, yet no corresponding reference from 2010 appears in the list of references. There are likely other errors that I have missed. Please check carefully throughout the manuscript.

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The number of bins and modes used to represent the particle size distribution in this study are not described until Sect 4.2.1. I feel that would be helpful to the reader to have this information presented earlier, either in Sect. 2 when the APM model is described, or near the beginning of Sect. 3, before model results are presented.

p. 195, lines 20-23: The study of Koch et al. (2009) did not directly study the radiative forcings of black carbon aerosol. The numbers reported here are for AAOD x 100 at 550 nm, not the DRF.

p. 196, line 15: This sentence as it is written implies that the real part of the refractive index was held constant while the imaginary part was held fixed. However, the real part of the refractive index was also changed between the two simulations in Stier et al. (2007).

p. 196, line 25: The value for clear-sky TOA total atmospheric forcing for AERCOM minimum surface albedo from Stier et al. (2007) was -4.29 W/m<sup>2</sup>, not -4.19 W/m<sup>2</sup>.

p. 200, line 1: There is no indication of where the list of given values ends and where the list of values obtained using the look-up table begins.

p. 212, line 16: Jacobson (2000) is the correct reference here, rather than Jacobson (2001).

Table 6: The average value and standard deviations for clear-sky NRF from AEROCOM models are missing. In addition, the standard deviations for atmospheric all-sky forcing and surface all-sky forcing should be interchanged.

Fig. 9: Please improve quality of this figure.

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