Thanks to the referee for his/her very helpful suggestions, which have allowed us to clarify and improve the manuscript. Below we addressed the reviewer comments, with the reviewer comments in italic and black, and our response in bold and blue. We have revised the manuscript accordingly.

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.

Yes, you are right. The mixing state does not significantly impact the simulated aerosol burden. In the revised manuscript, we have changed the text to "… critical for aerosol optical depth and DRF." The difference of radiative forcing between the base run and the NoCoat run are mainly attributed to the changes of optical properties due to different aerosol mixing state.

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.

The text has been modified as suggested.

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 concentrations 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.

Thanks for the suggestions. We agree that the use of the word "higher" is indeed ambiguous. We modify the text as suggested. Also, we included a few sentences to explain why a minimum cooling at TOA is seen in Fig. 10.

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.

The AOD predicted by GEOS-Chem-APM shown in Fig. 3 is the result for all sky. In our companion paper (Yu et al., ACPD, 2012) we discussed in detail the comparison of AOD between the model and observations. As shown in Fig. 6 of the paper by Yu et al. (2012), the higher AOD values over Europe and Northern Asia found in Fig. 6a (all sky) do not exist anymore in Fig. 6b (clear sky), therefore the higher values is associated with the hygroscopic growth of atmospheric particles.

We have included a short discussion to explain this disagreement between the model predictions and the observations in the text.

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.

Thanks for the suggestions. We have added a short description in Section 2.1.1 to give the information including size bins (for SP, sea salt and dust) and mode diameter (BC and POC) used in the model.

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.

Yes, the emission values of AeroCom simulations in Table 3 (Table 4 in the revised manuscript) were taken from the paper of Schulz et al., 2006. Since the values are for POM in Schulz et al. (2006), so we convert them to POC for comparison by simply multiplying a parameter (POM=1.40*POC). A note has been added in Table.

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.

Thanks. We double checked the manuscript, and made the necessary change accordingly.

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.

We have added a short description in Section 2.1.1.

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.

Thanks for the correction. We have modified the text accordingly.

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).

We have modified the text accordingly.

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/m2,
not -4.19 W/m2.

We have made this change to the text.

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.

We have modified the text slightly to make it clear.

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

Thanks. We corrected it in the text.

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.

Thanks for the correction. We have made this change to the text.

Fig. 9: Please improve quality of this figure.

Yes, we improve the resolution of the figure.