Reply to Referee Comment by Anonymous referee #1.

We thank Anonymous referee #1 for comments that made an improvement of the manuscript possible. In response to the comment, we have added some comparison of our simulation results with measurements and facilitated a visual comparison of the results. We have also responded to the specific comments and questions and provided additional explanations, where such were requested by Anonymous referee #1.

General Comments
The paper provides an overview of the seasonal cycle and spatial distribution of aerosols in India and China for a current year (2006) and concentration estimates for a future year (2020). The author's rationale for the study is to shed light into aerosol characteristics in two regions of Asia where high aerosol loading is prevalent. The two country India and China have different emissions source strength and they differ in their climate characteristics.
The authors use a global aerosol climate model, ECHAM5-HAM. The anthropogenic emission used is the REAS emissions inventory both for the current year analysis as well as the future scenarios. The aerosols simulated are sulfate, black carbon, organic carbon, natural dust and sea salt.
The model performance is evaluated with few published data from scientific literature which includes another modeling study, in situ experimental data set and aerosol optical depth from MODIS Terra satellite.
The ECHAM5 model performance evaluation is very qualitative and quite hard to follow with all the numbers and descriptions. At times numbers are thrown in from literature and statements about model performance are made that do not give a strong indication of the strength or weakness of the model. For example, the paragraph starting with "Ramachandran and Cherian in their study on MODIS Terra AOD (line 9 page 4028).
Visual comparisons in the forms of 2D spatial distribution graphs and temporal evaluation based on time series data would make the paper much more valuable. Of course the argument can be made that the data are not readily available, however, MODIS data are available and could be plotted for the whole four years or year by year comparison can be made. There are several papers on experimental data for the year 2006 from India such as ICARB campaign. The authors can compare few of the AERONET sites for time series analysis. As far as mass concentration data are concerned, if not for country averages, point source location can be compared with specific model grid points. The authors cite an example of aerosol composition from the INDOEX days at KCO. How does the model grid point aerosol composition wherein KCO site falls in, compare with the observation data (albeit from a different time frame)? Without rigorous model performance evaluation strong statements like "India had a higher concentration of black and organic carbon and China had higher concentrations of sulfate" seems to disregard the model and emissions uncertainties as well as not give due credit to the experimental observations.

The simulated AOD distributions in Figures 3 and 5 can be and are compared to MODIS coll. 5 data in Figures 6 and 7. Figure 13 and related discussion have been added to facilitate comparison of seasonal and interannual variability in simulation and MODIS results. Figures 14 and 15 have been added with comparison to AERONET results and the figures are now discussed in the text. The aerosol composition at KCO (from the simulation time frame) is now mentioned in the text. Additionally, we have compared the black carbon contribution to PM2.5 in Trivandrum on the western coast of southern India to that at a nearby gridpoint in the Arabian Sea. Black carbon measurements from the ICARB campaign in 2006 are already discussed in the text in the original

manuscript based on the reference (Beegum et al., 2009) in Section 4.1. Also other experimental data are discussed. The resulting average concentrations are a combination of aerosol emissions, processes, transport and removal, which all have uncertainty. Our results indicate that, although Chinese black carbon emissions are slightly larger in total than Indian black carbon emissions, less efficient wet removal during the winter in India and a smaller averaging area lead to larger average concentrations in India. India has larger organic carbon emissions, and less efficient wet removal in the winter applies also for organic carbon. China has higher concentrations of sulfate simply because emissions are much larger than for India.

It is important to be able to visually compare the results. It would be useful to see the color bars on the same scale if we are comparing India and China. The authors also conducted several sensitivity studies with emissions, two for the current year and two for the future years. Other than the differences in numerical concentration values it was not really clear what the implications to policy /impacts were.

The total AOD is plotted using the same color bar for China and India and for simulations and satellite measurement results (except for very small differences between the two plotting softwares used), and similarly for total PM2.5. Plotting values for the different species for India and China was a conscious choice, as for instance sulfate concentration and AOD distributions are so different for the two countries. Direct visual comparison is easier in the figures showing the seasonal cycles. Detailed implications to policy are outside the scope of this article, but other impacts can be seen for instance in what is Figure 16 in the revised manuscript and the related discussion in the text. It can be seen that cuts in anthropogenic emissions affect surface concentrations the strongest in winter for India and in the latter half of the year for China, and similarly for the AOD. More impacts could be studied for instance through simulating/investigating the climate effects of the changes in spatial distributions and seasonal cycles.

The paper provides insight into seasonal cycles and spatial distribution of aerosols using different model (different physics and chemistry) and emissions inventory, so this reviewer would recommend for publication only after the concerns listed above and in the specific comments are addressed.

Specific Comments
Model and Simulations:
Lines 1-7, page 4022: Please explain how the model deals with the sulfur chemistry since all other aerosols are primary aerosols. Also mention the growth rate of sulfate aerosols, or how is it distributed to different size bins. Line 16 page 4022: A brief discussion on the resolution of the model used to study regional scale phenomena, i.e., uncertainties, limitations.

Explanation and discussion added to the mentioned places in the text.

Emissions inventory and scenarios:
Line 1 page 4023: Emissions are available from 1980-2003 and for future 2020. The model is run from 2005-2009 and 2019 2023. A bit more explanation is needed. Does the model emission grow in each of these years? If so, does REAS provide growth factors? What is the base year for emissions inventory for the REAS emissions? The authors claim to have used a recent emissions inventory. Line 6, page 4023: Does the emissions take into account diurnal variation of emissions? Line 6, page 4023: How do the authors take into account the emissions resulting from biomass burning, especially for BC and OC? Line 13-15 page 4023: BC and OC emissions are large, compared to what? Other countries, or compared to other emissions inventory of China and

India. Line 10 page 4023: How is natural emissions treated in the model? Line 1 page

4025: The first year is used a spin up and the rest of the years are used for presenting the results. The atmospheric part of the model was referred to another paper. If the ECHAM model is a climate model, is 1 year spin up enough? If it is a synoptic scale model please discuss how the observations are taken into account? If emissions are used from 1 year (2006) and meteorological fields are used for 4 years, please discuss the rationale.

The REAS inventory is made for the years 1980-2003 and for future scenarios for the years 2010 and 2020. We are using the REAS prediction inventory for 2006 available at the ACCENT/GEIA website. The prediction inventory for 3 years past the last year in the inventory was created using linear interpolation of emissions in 2003 and 2010 (policy failure case (PFC) scenario, information from the FRCGC website: http://www.jamstec.go.jp/frcgc/research/p3/emission.htm). This is now mentioned in the first paragraph of Section 3. When we started our study, REAS was to our knowledge the newest emission inventory for Asia. Since that, the INTEX-B (Zhang et al., 2009) and GAINS-Asia (Klimont et al., 2009) inventories have come out with their own strengths and weaknesses as compared to the REAS inventory. Diurnal variations are not taken into account in the model used, but emissions are spread out evenly between the model time steps. BC and OC emissions are large compared to other countries and also compared for instance to the earlier TRACE-P inventory of Asian emissions. Natural emissions of mineral dust, sea salt and ocean dimethyl sulfide (DMS) are calculated online such as described in (Stier et al., 2005). Terrestial biogenic DMS emissions are prescribed. Other natural emissions are based on AEROCOM emissions (see (Stier et al. 2005) and http://nansen.ipsl.jussieu.fr/AEROCOM/ for a description). A paragraph describing the natural emissions is added to Section 2. A one-year spin-up period is more than enough, because only the atmosphere needs the spin-up. The model creates its own meteorology using sea surface temperatures as boundary conditions. Sea surface temperatures in our simulations are prescribed and taken from a simulation with the coupled ECHAM5-MPI-OM atmosphere-ocean GCM, as mentioned in the sentence starting on line 18 of page 6 in the original manuscript. The rationale behind using emissions for the year 2006 and meteorological fields from 4 years is that we want to study distributions characteristic for the Asian climate without having too large impacts of meteorological conditions and for instance thereby following monsoon rains and dust emissions of an individual year.

Spatial Distributions:
Repeating earlier statement, it is useful to see the color bars on the same scale for figure 2-7 if we are comparing India and China and observations. Line 8 page 4027: Dust concentrations peak . . .. in the north on the Tibetian plateau. The figures including the Taklimakan and Gobi deserts are not shown but one would expect to discuss emissions from these two desert regions. Line 27, page 403: Again as mentioned in the general comments, one needs to be careful in interpreting the results. The impression I get is that China has less BC, OC than over India. Please discuss the emissions inventory over China and India used in this study. How did you average the concentration in the grids? What was done to the coastal areas, are they also averaged? If carbonaceous aerosols are the dominant species over India, then one needs more experimental data to back the claim.

As said in the reply to one of the comments above, the choice to use same color bars for total AOD and PM2.5 and flexible scales for the different aerosol species was a conscious choice, balancing easy visual comparison and resolution of the scales. As to the Taklimakan and Gobi deserts, a large part of Gobi desert is included in Figures 4 and 5. The dust over Taklimakan desert contributes to the average concentrations and AODs of China in Figures 8, 9 and 10. Averaging domains for India

and China consist of the longitude-latitude boxes mentioned in the text and they include the coasts as well as the grid points over the sea falling inside the longitude-latitude boxes. For the black and organic carbon emissions and concentrations large uncertainties remain, which is largely due to the large uncertainty in emission factors. However, as mentioned before, our results indicate that despite slightly larger total BC emissions in China in the REAS inventory, less efficient wet removal in the winter in India and the smaller averaging domain make Indian average concentrations clearly higher.

If the four year runs are simulated what do the inter annual variations show?

Figure 13 and related discussion is added to investigate this.

Section 5 future scenario: Is the meteorological field for the years circa 2020 substantially different from the current years? The paper say the meteorology in the model also takes into account the role of aerosols (line 12, page 4021)

The meteorological fields for the years circa 2020 are generated by the model, using sea surface temperatures (SSTs) for those years from a coupled simulation with the coupled ECHAM5-MPI-OM atmosphere-ocean GCM as boundary conditions, as mentioned above in the text and in a reply to another comment above. The sea is slightly warmer in 2020 than in 2006, but the Asian climate is not fundamentally different. Effects of aerosols on the meteorology come in through their effects on shortwave radiation.

Line 16 page 4033, If no anthropogenic emissions, then where are these concentration values coming from? Are they being brought in from the boundary conditions? Are you only presenting surface layers or averaging column over the region? When averaging concentration for all the other reported values, are just the surface values being considered or do you include higher levels? What is your lowest level height in the modeling domain?

Aerosol concentrations seen in the simulation with no anthropogenic emissions origin from natural emissions. Natural emissions are now discussed in more detail in Section 2. Concentrations presented are for the surface layer in the model, which in the sigma-hybrid coordinate system used reaches from the surface to about 60-70 meters. AOD values are column values.

Is the spatial distribution within India and China changing in the emissions inventory for the year 2020? If not, then shouldn't one expect to see similar distribution except change in the magnitudes as observed from your results?

In the REAS reference scenario, there are changes in the spatial distribution within India and China, resulting from independent scenarios for power plant, industry, transport and domestic emissions. In the -2%/year scenario, all emissions are merely scaled and spatial distributions are not changed. But even here, aerosols interacting with each other and with the meteorology have potential to change the spatial distributions. The final result is a result of natural and anthropogenic emissions and of the interactions of aerosols with each other and with the meteorology, whereby the level, not just the distribution, of the anthropogenic emissions matter.

Conclusion: Line 24 page 4035: Again there isn't a single figure or table that shows some experimental data with model to evaluate model performance to back the statement. Model data are shown for 4 year averages while MODIS is shown for 1 year. Results like figures 10, 11, 12 could be shown for at least some grid cells if not for the whole country.

Figure 13 is added to facilitate visual comparison of simulation and MODIS results. Also Figures 14 and 15 are added to provide a comparison of simulation and AERONET results. The new figures are discussed in the text.

Technical corrections Line 1 page 4034, incomplete sentence.

Sentence completed.

References:

Klimont, Z., et al.: Projections of SO2, NOx and carbonaceous aerosols emissions in Asia. Tellus 61B, 602-617, 2009.

Stier, P., J. Feichter, S. Kinne, S. Kloster, E. Vignati, J. Wilson, L. Ganzeveld, I. Tegen, M. Werner, Y. Balkanski, M. Schulz, O. Boucher, A. Minikin, and A. Petzold: The aerosol-climate model ECHAM5-HAM. Atmos. Chem. Phys. 5:1125-1156, 2005.

Zhang, Q., et al.: Asian emissions in 2006 for the NASA INTEX-B mission. Atmos. Chem. Phys., 9, 5131-5153., 2009.