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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Particulate absorption of solar radiation: anthropogenic aerosols vs. dust" *by* C. Wang et al.

C. Wang

wangc@mit.edu

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We thank Dr. Lorraine Remer for her constructive comments on our paper. We have revised the manuscript based the comments and suggestions from both reviewers. The following are our point-to-point responses to Dr. Remer's comments (listed using bold and Italic font).

1) We need more information about the data sets used to create Fig. 1. Exactly what AERONET data? Climatology? Averaging from scratch and how? There should be discussion of the uncertainties in the AERONET AAOD. These are retrievals, not observations. AAOD numbers have changed with updates to the AERONET retrieval. Which version of AERONET retrieval? Exactly what MODIS data? Level 3 daily and averaged? Level 3 monthly and averaged? Which col-

lection of MODIS data? How were the gridded data sets matched to AERONET? The exact grid square? There are lots of dots on these plots, especially for the monthly plot. Is there a better way to display this information? Contours for example? It is hard to see whether the model used in this study is sufficiently valid. Definitely the symbol for MODIS needs to be changed so that it can be seen. Perhaps it is not smart to put all these different things on the same plot. Perhaps the goal is to compare the results from this study against AERONET and then against MODIS and against GOCART. The RMSE numbers are rather higher than advertised. Would showing regression lines and correlation coefficients be helpful? I just wonder if it is at all useful to have RMSE of 0.10 and above when most of the world's oceans have AOD in the 0.10 to 0.11 range? The plots suggest more skill than the RMSE values suggest, yet the only hard numbers given are these RMSEs.

We have clarified the descriptions of data and analysis methods that are used to produce Figure 1. Sentences such as "AOD measurements and AAOD retrievals" along with data versions (e.g., AERONET inversion product, level 2.0 and version 2.0; MODIS level 3 monthly means) have also been added to the text.

We have also revised Figure 1 by taking out the GOCART model points (GOCART is one the AeroCOM models, see below) from the scattering plots. This makes the points of our models as well as MODIS AOD more visible. We noticed that the RMSE value is comparable to the typical AOD value over oceans. A few large model-observational discrepancy points might also significantly influence the value of this quantity. However, since the comparison is made for AERONET stations that are all land-based (some are even urban stations), therefore, the RMSEs are rather low compared to the base values. We have also indicated in the revised manuscript that "Many detailed model-observation comparison results can be found in Kim et al. (2008)".

In addition to the above-described revisions, a plot showing the comparison of our modeled AAOD with the median of AeroCOM models (Kinne et al., 2006), both as

monthly and global means, is added along with discussion in the text.

2) What is meant by anthropogenic? I was taken aback by this statement on Page 6578. "Anthropogenic fraction is also high due to DMS oxidation". Huh? DMS is natural. This statement implies that the partitioning between anthropogenic and dust needs to be better defined from the beginning. What is included as 'anthropogenic'? Does it include boreal wildfires? Does it include DMS? Does it include fine mode sea salt? Terrestrial biogenics? This starts to become important. Is the reason for the dominance of anthropogenic aerosol over the oceans because of the biogenic component there?

We have added two sentences in the text to clarify this: In P6575, L16, "There are, however, particularly in the AOD calculation, rather small contributions from natural sources such as DMS in sulfate production and secondary particulate organic carbon in OC that have not been separated from anthropogenic fraction owing to the complexity of such a procedure."

P6578, L17, "Note an artifact over the remote oceans where the anthropogenic fraction is high due to sulfate aerosols largely from DMS oxidation that should be separated from anthropogenic fraction."

3) We know from the AEROCOM experiments that the global aerosol transport models will all match the global AOD but have significantly different combinations of particular species and different values of extinction efficiencies. I question how robust would be the results from the present paper if two different models were employed? I'm not suggesting that the authors run multiple models. That is an AEROCOM responsibility. I just think there should be discussion.

We actually indicated in the manuscript (P6578, L18-L27) the difference in calculated sectional AOD among various models despite their general agreement in the modeled total AOD.

4) In this light, I found that the model runs of 'high' and 'low', Table 1 and figure 4 to be of the most interest. The high and low runs seem to be sufficiently broad in parameter space (ie SSA for dust ranges from 0.68 to 0.94). Given this broad parameter space it is not surprising that the 'high' and 'low' scenarios show two very different worlds. The 'high' world restricts dust dominance to only the known dust source regions. The 'low' world restricts anthropogenic dominance to only the biomass burning regions and the U.S. and Chinese southeast. This implies that the greatest control that policy makers can exert on absorbing aerosol effects should be directed to tropical biomass burning.

Agree, perhaps with an additional emphasize on emerging East Asian economies as well.

5) It would have been very useful to do more than 'high' and 'low' and to show how the results varies due to changes in SSA versus changes to emissions. I realize that this is outside the scope of the study, but I wanted to mention it.

We have added the following discussion in the text (P6579, L26): "Note that the differences in dust emissions and lifetime (removal processes) in various models also contribute to the uncertainty in modeled dust AAOD. A recent model survey indicated that a range of factor of 4 in modeled dust burdens (usually smaller when converted to AAOD) could be resulted from such differences among models (Zender et al., 2004). However, this difference is still smaller than the 5-fold range in dust AAOD derived here by using the high and low single scattering albedo."

6) The results did not surprise me. I have never considered dust to be that absorbing in the visible. My guess is that the TOMS and then OMI global maps of 'absorbing aerosol index' in the UV accentuated the role of dust as an absorbing aerosol because the observations were made in the UV where dust absorbs strongly. This leads to a question, what is the relationship between the results calculated here for midvisible to the actual heating in the atmosphere, which

should be integrated across the spectrum? Would this modify the paper's conclusions? I'm sure there must be references. I just don't know them.

We have added a paragraph in the text (P6579, L27): "It is worthy indicating that dust absorption in the UV band is much stronger than in the visible band. Using 300nm instead of 550nm in our calculation, the global and annual mean anthropogenic fraction in AAOD would be 48% instead of 73% (not shown). However, considering the dominance of solar energy in the visible comparing to UV range and also the rapid increase of dust single scattering albedo with wavelength moving from UV to the visible range (e.g., Jeong and Sokolik, 2007), our conclusion drawn based on the 550nm band analyses should be still valid for the integrated particulate absorption".

Following are some typos that I noticed. Several times the article 'the' is missing.

All the typos indicated by Dr. Remer have been corrected.

Lines 9-14. How much are these statements trusted? Can ARCTAS results support the identification of dust in the arctic?

We expect that the analyses of aerosol optical properties from ARCTAS data would answer this question.

Line 20-23, is the dust also scavenged and reduced?

The dust scavenging is included in our dust model. The climatological distribution of dust used in our analyses reflects such reduction.

P6582 line 2-6. Coexisting in the vertical also?

Yes, please see Figure 7.

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