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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 appreciate the constructive comments of this reviewer on our paper. The following are our point-to-point responses to the comments (listed using bold and Italic font).

Long-wave effects
Given the large difference in the size-distributions of anthropogenic aerosols (fine) and mineral dust (coarse), I was very surprised by the limitation of this study to "solar absorption". In fact, the results are entirely focusing on absorption optical depth at 550nm. However, from Mie theory it is clear that dust with its larger Mie size parameter will also have significant, if not dominant, absorption in the long-wave part of the spectrum, a fact that is neither addressed or even mentioned in this manuscript. While the climate implications of aerosol absorption

are repeatedly mentioned, the impression is left throughout that AAOD at 550nm would the representative measure of aerosol absorption. This is unfortunately not true. For a comparison of the relative importance of anthropogenic and dust radiative effects, that seems to be the focus of this manuscript, both in the title and body, it would be essential to consider SW AND LW radiative effects. I am not implying that the authors do not clearly state their focus on solar radiation but this somewhat subtle limitation is certainly confused by a wider audience. More importantly, I am not sure what the particular science question of interest is given this limitation as it is simply not sufficient to quantify the importance of dust absorption solely through AAOD at 550nm.

We agree with the reviewer on the potential contribution of long wave absorption of dust to the direct climate effect of these particles. As realized by the reviewer, the focus of this paper is on the solar absorption and not a comparison of long wave with short wave effects of dust. One of the reasons is the fundamental difference in the forcing controlling factor and certain mechanisms of these two types of forcings. To clarify this in particular to a wider audience, we have added a sentence in the manuscript: "Note that dust absorption of thermal radiation could be a non-negligible factor in determining the climate impact of dust despite high uncertainty (e.g., Tegen et al., 1996). This effect is excluded in our current analyses".

From AeroCom and other inter-comparisons we have learned that absorption optical depth, a measure of potential extinction rather than actual extinction, is not an unambiguous predictor of actual absorption as calculated by a range of models. Other factors, such as clouds seem to play an important role. Therefore, the conclusions about the relative contribution of certain aerosols to absorption are reaching too far. If absorption optical depth is shown, the discussion should focus on this parameter. Statements about the contribution to absorption should be supported by the actual results for all-sky absorption, ideally SW+LW. This has actually been done in previous work - which is largely ignored in the discus-

sion or introduction.

The reviewer's point is well taken. However, the reviewer was debating about the influence on the prediction of a given quantity by different models (environments) while our discussion is about the quantitative difference of a given parameter derived from two subgroups presumably compared under the same environmental conditions. We have added a sentence in the text to further clarify this point: "because for the same incoming solar radiation reaching the aerosols and local meteorological conditions, AAOD determines the heating rate due to particulate solar absorption".

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6571, 2009.