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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol absorption and radiative forcing" *by* P. Stier et al.

P. Stier et al.

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Response to comments of reviewer 2

We would like to thank the reviewer for the helpful comments. We very much appreciated the remarks and hope to have addressed all raised issues. All page numbers refer to the revised manuscript in the submitted form.

Abstract, Line 17. "The effect of the usage of more accurate effective medium approximations is comparably small" gives the impression that it can be neglected. Is that what the authors think? While the effect is smaller than using the updated BC refractive index data it still has an effect that is significant even if AERONET measurements cannot be used to confirm it. The new imaginary part of BC is much larger than the BASE simulations so it is not surprising it will dominate, but this does not diminish the effective medium effect, especially if considered regionally.

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We actually purposely chose the phrase “comparably small” to avoid any valuation in this statement, such as “negligible”. With BB-M as reference case, the usage of volume weighted averaging vs. the Maxwell-Garnet mixing rule changes the clear-sky total-aerosol absorption by 10% while the simple update of the refractive indices from BASE to BB-M changes it by 24%. It is entirely up to the reader to decide what they consider negligible. However, given the fact that the implementation of mixing rules is somewhat more elaborate and adds nonnegligibly to the computational burden - asked for our judgment - we think that the usage of more accurate refractive indices would be a logical first step for model improvements.

I would suggest that the radiative forcing amount be quoted in the Abstract and that the sentence be modified to state that the effect is smaller than the effect of using the new BC refractive indices.

Initially, we actually had the forcing numbers in the abstract. However, we realized that this many numbers (for comparison one would also need to add the forcings for the different refractive indices) made it hard to read so that we think this simple statement serves the readability best. All relevant forcing numbers are summarized in the conclusions.

Page 7176, 1st sentence: ’The aerosol single scattering...” is grammatically awkward, it should be reworded.

We agree and rephrased the sentence to (page 2, col 2, par 4):

“The aerosol single scattering albedo (SSA), i.e. the ratio of the extinction due to scattering to the total extinction due to scattering and absorption, decreases with increasing aerosol absorption.”

Section 4.1. There needs to be a paragraph briefly discussing the AERONET measurements and retrievals of absorption optical depth. I believe there are critical issues about the accuracy of these retrievals for low optical depths. This

ACPD

7, S3440–S3444, 2007

needs to be discussed.

We agree that an extended description of the AERONET retrieval benefits the manuscript and extended the Section 4.1 by (page 7, col 2, par 4):

"To minimize the error of the AERONET retrieval we apply the following procedure to produce daily mean AAOD values from Version 1 Level 2.0 data. We stratify the retrieved aerosol single scattering albedo data for each day into a 5 bin histogram ordered by AOD. If the average AOD is larger than a critical threshold value of 0.3 we adopt the associated single scattering albedo average for the calculation of AAOD. If the average AOD falls below the critical threshold, we adopt the SSA value associated to the first AOD bin average above the threshold. In case the average of even the largest AOD bin stays below the critical threshold, we prescribe the SSA of the largest AOD bin to all bins with lower AOD. This procedure is designed to minimize the potential SSA error for small AODs (small signal to noise ratios)."

We further added the following comment to the conclusions (page 11, col 1, par 1):

"It has to be pointed out that the retrieval of AAOD from AERONET measurements is subject to nonnegligible uncertainty, in particular for low AODs. However, it is arguably the most accurate measurement of ambient aerosol absorption available."

Page 7191, Line 21. It is pointed out that the vertical distribution of clouds versus black carbon is important to determine the radiative forcing and its sign. However there are no results presented to show how often significant black carbon layers are found above clouds. I think additional results and proof demonstrating their statement that the relative location of absorbing aerosols and clouds is crucial should be presented.

While we agree that this is an interesting question, it is not straightforward to quantify from our simulations. We refrain from defining a non-quantitative measure of the occurrence of "significant" amounts of black carbon layers over clouds as a thin layer (just

ACPD
7, S3440–S3444, 2007

EGU

below any arbitrary threshold) with more frequent occurrence could have the same effect as an optically thicker layer with a less frequent occurrence. We do believe that the presented difference between the clear-sky and all-sky instantaneous aerosol radiative forcings already provide the most illustrative point of view for our studies. The effect of the relative vertical distribution of aerosols and clouds on the radiative effects could be quantified in idealised sensitivity studies with different emission heights or prescribed vertical profiles - that would go beyond the scope of this paper but would make an interesting future study.

Table 2. The errors of 5 and 10 for SS and DU seem too small. Where do they come from?

The additional numbers ± 5 and ± 10 Tg given in Table 2 for the sea-salt and dust emissions do not actually represent an error estimate for the emission fluxes but refer to the variations of the interactively calculated emissions among the simulations. These variations are caused by small fluctuations in the surface wind speed owing to perturbations introduced by the aerosol radiative effects, as only the large scale meteorology of the simulations is constrained through the nudging technique. We have clarified this in the figure caption by adding (Table 2):

"The range for the sea salt and mineral dust emission fluxes indicate small fluctuations of the interactive sources."

Table 7. Most of the sensitivity studies should be compared with BB-M not BASE. This should be stated in the caption.

This is a good suggestion. We extended the caption by:

"Please note that the BB-M study, showing good agreement with the AERONET AAOD retrieval, serves as reference for comparison with the other studies."

Figure 4. I find it interesting that the global figure of aerosol clear-sky absorption shows that central Africa extending into the Atlantic is the most significant

ACPD
7, S3440–S3444, 2007

EGU

centre of absorption. This seems to deviate a bit from the idea about the location of the ABC and that the region around India should have greater absorption. What are the authors thoughts about this? Should the "A" in ABC be Asian, African or Atmospheric? Are the BC emissions underestimated from India? (I know this paper is not addressing the emissions uncertainty but I think the ABC issue should be addressed.)

An interesting question, that luckily the originators of ABC answered themselves on: http://www-abc-asia.ucsd.edu/ . What started as Asian Brown Cloud, arguably inspired by the involvement in the INDOEX and ACE Asia campaigns, has now been generalized as Atmospheric Brown Clouds, one of which is the Asian Brown Cloud. Certainly the African Brown Cloud would have its place in this gallery and one might (unfortunately) be inspired to add the Amazonian Brown Cloud to the list.

In any case, our results, backed my many in-situ and remote sensing measurements, indicate that Central and West Africa, South East Asia, and Central South America are hotspots of atmospheric aerosol absorption. We do not believe that the accuracy of the measurements and the significant process uncertainties yet provide sufficient constraints on the inverse estimation of BC emissions.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7171, 2007.