

Interactive comment on “Aerosol absorption and radiative forcing” by P. Stier et al.

Anonymous Referee #2

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The paper presents the results of ECHAM5-HAM global model simulations of aerosol absorption using year 2000 meteorological fields as well as a series of sensitivity case studies. The model has been updated with improved estimates of black carbon optical properties, a more sophisticated treatment of in-cloud aerosols and computes the radiative forcing into the thermal infrared. As such this represents the best simulations of the absorptive radiative effects of global aerosols to date.

The paper is well organized and written. Forcing the fields with the year 2000 meteorology allows an instantaneous comparison with Aeronet measurements which is a real plus for this work. The sensitivity studies are useful in assessing the importance of a number of the assumptions in the model parameters. I cannot find any faults with this paper and recommend its publication. However I have included a few comments and suggestions.

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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. 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”.

Page 7176, 1st sentence: “The aerosol single scattering \tilde{E} .” is grammatically awkward, it should be reworded.

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 needs to be discussed.

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.

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

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

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 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.)

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

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