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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Host model uncertainties in aerosol radiative forcing estimates: results from the AeroCom prescribed intercomparison study" *by* P. Stier et al.

**Anonymous Referee #1**

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This paper describes a comparison of nine global models subjected to a prescribed direct aerosol forcing. The models were subject to two different cases, a purely scattering aerosol and an absorbing aerosol, both with the same wavelength dependence of the optical depth, a fixed asymmetry factor, and the same height distribution. In addition, the authors attempt to separate the uncertainties due to the model background states from the uncertainties due to the aerosol response. This is a timely and interesting paper, given the current high level of concern about aerosol forcing of climate and the need to determine the contributing factors to model uncertainties regarding that forcing.

General comments

1. While comparison studies such as this are only possible through the willing participation of investigators, it is unfortunate that none of the models here are exactly comparable to those currently being used in the IPCC AR5. However, the CAM5.1, ECHAM5.5, and LMDZ models are close. It would be helpful to have a brief statement regarding the similarities of these models to the versions used in the AR5.

2. Throughout the paper there are references to differences in results that may be due to differences in radiative transfer codes. It would be very helpful to have a brief comparison of the column radiative transfer (RT) forcing for each model due to the prescribed aerosol. A comparison as a function of solar zenith angle and atmospheric profile (primarily water vapor and ozone) would help identify the error expected simply due to the differences in RT codes. There is a reference to a paper by Randles et al. but I could not find a copy of this paper (only an abstract for a talk). I think it would be very helpful to have a longer discussion here of how the RT model results compare with the global results presented here and what uncertainties arise from the use of less accurate RT models.

3. All integrated numbers presented in the paper are for the entire globe. However, much of the model diversity arises from variations in non-ocean albedo, particularly in polar latitudes. It would be very useful to have a comparison table similar to Table 3 for ice-free ocean (or ocean equatorward of 60 degrees) for both aerosol forcing cases. This comparison would remove a large degree of uncertainty due to surface albedo variability. I strongly encourage the authors to add this to the paper. 4. Some aspects of the specified forcing lead to rather peculiar results that are inconsistent with reality. One of the most obvious is the strong forcing results over Antarctica (and over the Arctic to a somewhat lessor degree) in the absorbing aerosol clear-sky case (Figure 8). This is caused by the assumption of a high concentration of absorbing aerosol in an area where this does not actually occur. While I have no problem with the prescription of the aerosol for these runs, I think it would be very helpful to identify such features in



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the discussion and warn the casual reader that these features do not represent reality. I have similar concerns about the rather strong above-cloud absorption in ocean stratus regimes, although I cannot tell how much of the aerosol burden actually ends up above these clouds.

5. The authors define a relative standard deviation (standard deviation divided by the mean, both in absolute value). I understand the rationale but I wonder about its usefulness when the mean values have small magnitudes. It is not clear that an RSD going from 10% to 90% is particularly important when it is driven by the mean dropping from 4 or 5 W/m2 to 1 W/m2 or less, rather than be significant changes in the standard deviation.

Specific comments

1. P25494, L5ff: I think this paragraph could do with a little more explanation. I assume from the statement here that all models other than ECHAM allow the aerosol forcing to affect the diabatic heating, which in turn may alter dynamical responses. If this is not the case, it would be helpful to state the situation more clearly. If ECHAM is relaxed to a pre-determined ECMWF reanalysis, then how much influence can the aerosol have? It seems to me that it must be quite limited.

2. P25494, L22: It would be helpful to reference Table 1 when you first mention FIX0 since it is defined in Table 1.

3. Same paragraph: This is a minor point as far as the paper is concerned, but cloud fractions less than 0.5 are simply too low. The use of simulators in AR5 models shows that cloud amounts in GCMs are generally too low, but most are still higher than 0.5.

4. P25495, L2: It appears that there are equally large cloud problems in the southern oceans and northern hemisphere storm tracks.

5. P25495, L21: Figure 16 is very difficult to read. I suggest that it be expanded, especially since there is no penalty for extra space in an on-line article. The caption in



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Table 3 should identify the symbols.

6. P25496, L16ff: As noted earlier, this is a particularly important comparison and should be expanded. Ozone absorption is probably not well represented by a low-spectral resolution model and may be the source of the discrepancy.

7. P25501, L6ff. Figure 19 is really pretty complicated. I think you need to spend a little more time explaining what you are doing and how you are doing it. Also, I got confused by the concept of "shading" on a color plot. Presumably, the authors are referring to the hatching, but it is certainly not obvious, especially because the plots are tiny as presented here. Please clarify this situation.

Technical comments

There are a number of annoying spelling and grammatical errors in the manuscript. I don't intend to identify them all but here are a few examples:

1. P25491, L1: this phrase is just dangling in space following the list. It should be written as a complete sentence.

2. P25496, L18: "benchmark", no"s"

3. P25496, L22: ozone is not a proper noun – no capitalization

4. P25498,L9: insert "albedo" after "single scattering

5. P25499, L17: "effect", no"s"

6. P25502, L28: "conducted"

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25487, 2012.