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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Black carbon vertical profiles strongly affect its radiative forcing uncertainty" *by* B. H. Samset et al.

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We thank both anonymous reviewers for their kind assessments and constructive comments on our submitted manuscript. Below we answer all non-technical issues raised. All typos noted and technical comments are also taken into account in the revised manuscript. Where both reviewers have addressed the same issue, we refer one of the reviewers to the other response.

From Anonymous Referee #1

1) There should be more reminders throughout the text of speciïňĄcally which model(s) are being tested for speciïňĄc features. Most of the tests evaluate the OsloCTM2, while only the variability associated with differences in vertical proïňĄles (in space and time)

are associated with the 12 AeroCom host models. Liberally adding "OsloCTM2" and more consistently applying "recalculated" to RF throughout the paper as appropriate is a solution to much of this problem. Additionally, results that are based on separate analyses of AeroCom Phase 1 and 2 models (e.g. in Section 4.1) should more clearly be delineated to avoid confusion.

Response: We agree, and will clarify the text accordingly. We also thank the reviewer for pointing out confusing passages in the technical comments.

2) The manuscript needs a more thorough treatment of intermodal variability in efficiency proïňĄles. At present this is briefly discussed in the discussion session, but at a minimum other published comparable values should be cited and discussed (e.g. Hansen et al., JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 110, D18104, doi:10.1029/2005JD005776, 2005, Figure 27). This point is important because without good understanding of the magnitude of impacts of microphysical state/cloud ïňĄelds, etc between the individual AeroCom models on their native efficiency profiles, the impact of the vertical distribution sensitivity explored here is not clear.

Response: We fully agree with the reviewer here, and indeed some of us (Samset and Myhre) have asked other models to estimate their vertical efficiency profiles in previous publications. As it is, we do not have sufficient information to properly address the intermodel variability on EPs, however we thank the reviewer for pointing out the estimate in Hansen et al. We will add this to the manuscript together with some further discussion, and repeat the call for other estimates of EPs native to individual models.

SpeciïňĄc comments (pure technical remarks are all taken into account, not answered here):

2) P 28933, line 25: was the OsloCTM2 run under the phase 1 or phase 2 conditions?

Respones: Phase 2. Comment added to the manuscript.

9) P28936, line 25: Please specify that the discussion in this and the next paragraph

is focused on the P1 and P2 results of Schulz et al. and Myhre et al., rather than on the new analysis here. The statements at the end of this section will beneïňĄt from the deeper discussion of EP variability in the models mentioned above.

Response: This is not strictly correct. The M5k variable which is discussed here is based on the vertical profiles of BC in AeroCom P1 and P2, which were not presented in Schulz 2006 or Myhre 2012. The model submissions are of course the same, but individual BC profiles are presented here for the first time. We have updated the manuscript to read as follows:

In the present analysis, we can estimate the impact of the vertical distributions by the fraction of mass simulated above 500hPa, or approximately 5km (M5k), shown in Table 1 as M5k. M5k is calculated by integrating the BC vertical profiles presented in Figure 2a below for model layers above or below an average pressure of 500hPa, and then taking the ratio of these values.

See also our response regarding discussion of the EP above.

10) P28938 line 3 – this appears to be an error: d-f of Figure 2 show forcing efïňĄciency, not RF, so the sentence one line 3 is in error, and the discussion should be correctly applied.

Response: We thank the reviewer for pointing this out. The text was in error, as what is shown is not the forcing efficiency per layer but the recalculated RF divided by model layer height and global mean burden. This is done to get vertical profiles that are fully comparable in shape, independent of whether the model simulated a high or low burden. The following discussion relates to the pressure levels where models exert relatively most RF, and the regional variation of this pattern. We have clarified this in the revised manuscript.

11) P28939, line 15 – This sentence is difïňĄcult to evaluate without the deeper discussion of EP variability mentioned above.

Response: We have updated the sentence to read: “We have shown that, even on global mean, a significant fraction of the variability in the BC specific forcing is due to differences in vertical profiles when applying a common EP.” 13) P 28940, Second paragraph of the Discussions section: This discussion should be expanded, as discussed above. Also, the test performed is not clearly described:is the top of the atmosphere 0 hPa as in some of the graphs, and was the linear interpolation performed in a linear pressure space, or was this done in altitude coordinates? This also highlights a separate minor issue, that model results are presented in both altitude and pressure space – these should be related more explicitly for the reader either in the text or in the graphs.

Response: Related to response above. An updated paragraph now reads as follows:

Different models will likely have different BC efficiency profiles, however we do not at present have enough information to estimate a model spread. A recent estimate using a column model (Zarzycki and Bond, 2010) shows forcing efficiencies above and below clouds comparable to the ones used in the present analysis. (Hansen et al., 2005), using a global climate model, show (their Figure 27a) a forcing profile that is of similar shape to the one used here, but with somewhat weaker vertical gradient. However while the absolute strength of the forcing efficiency will matter for the final RF estimates, only the shape, which is related to how clouds and microphysics are treated in individual models, will influence the variability due to vertical profiles alone. To attempt to quantify the sensitivity of the present analysis to the shape of the profile used, we reran the analysis with an EP that was weakened by 20% at the top of the atmosphere (20hPa) and unchanged at the surface, with a linear interpolation in between, resulting in an overall weaker EP. This changed the global fraction of RF above 5km by less than 5%, indicating that the results are relatively stable within reasonable variations of the EP. Regarding using altitude and pressure, we have added an improved explanation of the M5k parameter and for the altitude bands in figure 4. See the response to reviewer 2 below.

14) P28942, line 1: Harmonizing not only cloud and albedo differences, but every factor involved in the RF calculation (as all are done in Oslo-CTM2).

Response: The sentence now reads: “Harmonizing between models the treatment of clouds, albedo and other factors relevant to the calculation of radiative forcing, is found not to be sufficient to remove this variability”

18) Figure 3 C caption: above 5 km?

Response: Added 500hPa to the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28929, 2012.