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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.

Anonymous Referee #1

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The manuscript, well written and predominantly clear, is based on interpreting BC burden and vertical distribution from 12 global models from the AeroCom ensemble in a framework of BC forcing and forcing efficiency provided by a single model, the OSLO CTM2 model. The goal is to identify the specific regions and altitude ranges most strongly associated with RF diversity due to modeled BC vertical profile.

Although the vertical dependencies of the forcing efficiency of BC from the Oslo-CTM2 model were previously published by some of the authors, the application of those results to the AeroCom model ensemble justifies publication of this manuscript in ACP due to the insight it provides on the impact of burden/vertical distribution of these models in terms of RF and forcing efficiency, and in the sensitivities revealed from the Oslo-CTM2 analyses.

I have only two general suggestions to improve the clarity of the paper:

1) There should be more reminders throughout the text of specifically which model(s) are being tested for specific features. Most of the tests evaluate the OsloCTM2, while only the variability associated with differences in vertical profiles (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.

2) The manuscript needs a more thorough treatment of intermodal variability in efficiency profiles. 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 fields, etc between the individual AeroCom models on their native efficiency profiles, the impact of the vertical distribution sensitivity explored here is not clear.

Specific comments:

1) The short title ("black carbon vertical profiles strongly affect") should be improved.

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

3) P28934, line 8. Change "removed" to "not treated" to avoid confusion.

4) P28934, line 15. The sentence reads:

However, by dividing the recalculated RF by the total aerosol burden of the host model, we extract the variability in forcing per gram from each model that is due only to vertical profiles, and can subsequently use the vertical RF profiles to study it.

This is an example of a point that could confuse the reader. The variability in forcing per gram referred to in the second half of the sentence is not "from" the individual AeroCom models, but rather from the Authors' analysis of the models' vertical distributions of BC. Hence the sentence would be improved if it read:

However, by dividing the recalculated RF by the total aerosol burden of the host model, we extract the variability in forcing per gram that is due only to the vertical profile of BC produced by the host model in conjunction with the OsloCTM2 efficiency profile, and can subsequently use the vertical RF profiles to study it.

5) P28934, line 19 – include "aerosol optical properties, mixing rules" here? Also, this is a good example of a place to specify that only the Oslo-CTM2 treatments of these subjects is being evaluate.

6) P 28935, line 4: . . . and removal.

7) P 28935, line line 25: ". . . notably the vertical profiles, independent of any simple multiplicative scaling, were also. . .."

8) P28936, line 7 – ". . . components from horizontal and temporal variability *in vertical distribution* between models

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 benefit from the deeper discussion of EP variability in the models mentioned above.

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

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

12) P28939, Line 18 – here is a good example of where "recalculated" could be added to good effect: ". . . show the zonal mean *recalculated* forcing efficiency for all models. . ."

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.

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).

15) Table 1: and "f" has been introduced in the RF Fraction column mean value for China.

16) Figure 1, panel B: for consistency, the values above the light line should be called "recalculated RF".

17) Figure 2: Write out "dashed" and "solid" for Phase 1, Phase 2 to avoid temporary reader confusion.

18) Figure 3 C caption: above 5 km?

19) Figure 5: labels missing on the ordinates, restate dashed lines are Phase 1 in panel A.

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