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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new method for evaluating the impact of vertical distribution on aerosol radiative forcing in general circulation models" *by* M. R. Vuolo et al.

Anonymous Referee #2

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The athors present a study of the impact of cloud fields and the vertical stratification of aerosols on aerosol direct radiative forcing. Their method is to segment the native aerosol fields in the LMDz model into parts above, in and below clouds and compare these, and to read in external cloud and aerosol fields from the SPRINTARS model. In general the paper is well written and documented, the methods sound and the figures clear and relevant. I recommend that the paper be published in ACP. However, I do have a set of questions for the authors that relate to how the analysis was done and how it is presented. I expect that they can be readily answered by a set of minor revisions to the text and figures.

Main comments:

- In Methods, the authors describe how forcing is calculated in their experiments. However, I miss a thorough discussion of the concelt of "RF_CL", i.e. cloudy sky radiative forcing. It is not immediately clear how one estimates this in a GCM, and often it is rather calculated by using eq. (3) in this paper with RF_CS and RF_AS as input. See e.g. the discussion on cloudy sky forcing in Schulz et al 2006 (AeroCom phase 1).

- I also miss some further discussion on the cloud fields and how the aerosol fields look in the abv, in and blw configurations. Global, annual means are given in figure 6, but I think this should be expanded on. This is especially important yo be able to compare with other work, e.g. Zarzycki and Bond which performs some of the same analysis that the authors present here for a column model.

- Finally, and maybe most crucially, I find the Discussion section underdeveloped. The authors place much emphasis on the core results of their experiments and the resulting fields, but both the "BC vs clouds" and stratification analyses seem rushed. They are both interesting, and I feel that the authors can heighten the impact of their paper by putting greater emphasis here. E.g. in sec. 5.1 a new analysis method is introduced through an equation (which btw is missing an equation number), which may be interesting for others to use for similar analyses. However the results are not really discussed. Some findings are mentioned, but the question in the section title (on the role of clouds and aerosol vertical positions for black carbon forcing) isn't really addressed thoroughly. The same applies for sec. 5.2, which is where the authors apply equations 1 and 2. The nonlinearity calculates is shown in figure 8, but what do the results imply for the analysis, for the model variability of aerosol forcing, and for total aerosol forcing uncertainty?

Minor comments:

- The abstract is quite lengthy, and at the end uses model names and technical numbers. I would propose making the abstract shorter and more pointed, to increase inter-

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est in the paper.

- (I note that reviewer 1 had questions about equations 1 and 2. I believe I follow the authors reasoning here, and agree that the calculation is relevant for the discussion section. However, see my comment above.)

- page 18816, line 5: The authors state an assumption about hygroscopic growth. As the forcing of some species is likely highly dependent on this assumption, and it therefore impacts the per-species comparisons made later in the paper, I would like to see this assumption discussed in somewhat further detail. Is the analysis sensitive to it?

- page 18824, line 7: "NRF_CS" should be "NRF"?

- figures 4 and 5: Please consider adding a column with the ratio of the two first (or (A-B)/A) for clarity.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18809, 2013.