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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Soot microphysical effects on liquid clouds, a multi-model investigation" *by* D. Koch et al.

Anonymous Referee #2

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Comments on the manuscript "Soot microphysical effects on liquid clouds, a multi-model investigation" by Koren, et al. [Atmos. Chem. Phys. Discuss., 10, 23927-23957, 2010]

This manuscript aims to explore the impacts of carbonaceous aerosols on warm clouds through numerical experiments with assumed soot-reduction scenarios using six global models. The responses in cloud albedo, cloud cover, and radiative fluxes were analyzed and compared between the models. The simulations suggest a positive radiative response to the combined indirect and semi-direct effects from biofuel soot (BC and OC) reduction. The effect is comparable and opposite to the direct effects of soot particles. The biofuel soot particles are found to have the largest impacts on cloud microphysics owing to their larger size and higher hygroscopicity, while the reduction of fossil

fuel BC and diesel soot yields smaller and more uncertain responses. Large variability among model estimates and high internal variation was also identified. The overall conclusion of the study is in general consistent with previous studies. A few major issues/revisions should be addressed before the manuscript is accepted for publication, as listed below.

Major Comments

1) More detailed descriptions on experimental configuration should be provided in Section 2, including the initial conditions, the length of integration (only mentioned in the discussion), and the treatment and emission strength of the other aerosol species, etc. Also, the simulations are presumably equilibrium runs. The authors should justify that a 5-year integration is sufficient for establishing equilibrium state. Is any "spin-up" year excluded in the statistics? Although these might have been mentioned in the previous AeroCOM studies, a concise but clear statement is needed here to make this a complete manuscript.

2) The mean state and variability of the baseline (present-day) climate in each model is crucial, as all the perturbation experiments were compared against it. Some of the sensitivity and response of the soot effects can probably be explained by examining the biases in cloud fields and radiation in the mean state. The statistics of the PD climate should be given (either by adding new columns to Table 3 or adding a new table) and discussed (e.g. in the beginning of Section 3). Besides, information of statistical significance (e.g. t-test) should be given for all the perturbed results, including the maps and tables.

3) The current manuscript emphasizes mostly on the impacts of soot on microphysics; however, responses in the macrophysics of the clouds are equally important and needs to be addressed. For example, the authors give a nice summary and comparison of the aerosol schemes among models (soot hygroscopicity, nucleation, etc) in Table 1 and section 2.2. Similar discussions should be included on the cloud schemes (especially

the autoconversion parameterization, collision-coalescence of droplets, etc) and the radiation schemes (e.g. how liquid water mass and CDNC is translated to the radiation calculation). Maps showing the changes in liquid water path, droplet size, warm rain rate and boundary layer stability, etc, can be added and discussed in Sections 3.1 and 3.2. These can actually be the key to explain the difference in cloud lifetime and semi-direct effects among models.

4) All the radiative flux changes reported in the manuscript are the "responses", i.e. the fluxes after all the adjustments in the climate system are completed. Therefore these are not radiative "forcing" numbers, which is, by definition, the change of fluxes without adjustments in temperature and cloud to be made. Although it is difficult and ambiguous to define "forcing" for indirect effects, as feedbacks are inevitably included in the processes, the issue of "response vs. forcing" should be discussed and clarified, particularly when comparing the numbers with the soot direct effect "forcing". The author can consider adding a paragraph to the introduction section and/or Section 3.3 on this subject.

Minor Comments:

1) P.23934, Lines 17-19 – How is the hygroscopicity for internally-mixed BC and OC determined in the CAM-Oslo?

2) P.23935, Lines 18-19 – the sentence is somewhat ambiguous. It can be revised to "the CDNC is based on aerosol mass according to the relationships inferred from the MODIS retrievals".

3) Section 3.1/Figure 2 – Maps for CDNC changes should be provided.

4) P.23936, Lines 20-21/Figure 3 – Can the opposite response in CCN numbers between the CAM-Oslo and CAM-PNNL be explained? According to Table 1, the two models have very similar aerosol schemes. Also, CAM-Oslo has CCN decrease but CDNC increase, which is also worthy of discussion. What are the CCN changes in the

other models?

5) Table 1 – The following information can be included here for each model: (i) number of vertical layers (especially in the boundary layer), (ii) the assumed minimum CDNC values, (iii) the autoconversion parameterization, (iv) are the cloud albedo/lifetime effects considered for stratiform/convective clouds or not.

6) Table 2 – Please provide in the footnote the emissions for the other aerosol species.

7) Table 3 – Please add to the table the following results: (i) the changes in LWP (ii) the change in droplet effective size (iii) the change in CCN concentration (iv) the direct effect "forcing" for each model/scenario

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