Anonymous Referee #2

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

(Comment: Actually this is Koch et al., not Koren et al.)

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.

Response: We have added to section 2: "The models' analyses were based on five-year experiments, following one year of spin-up (four months for CAM-Oslo). Climatological sea-surface temperatures were prescribed, so that spin-up was only needed for the aerosol concentrations, and several months is sufficient." And "Other aerosol species emissions that are unchanged in the experiments include sulfur (145 Tg S y^{-1} for 2000 and 34 Tg S y^{-1} for 1750), dust (1680 Tg y^{-1}) and sea-salt (7900 Tg y^{-1}), with most sulfur emitted as gaseous SO_2 that then oxidizes to form sulfate."

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.

Response: Unfortunately most models saved 5-year mean fields and not annual fields, and the models have generally evolved since doing these calculations, so we cannot calculate statistics. For the three models that did save annual results, we provide estimates of standard deviation in section 3.3: "We note that the

interannual variability and the resulting standard deviation for the experiments is large. We calculated standard deviation for the changes from BF to PD over the five years of simulation in the L, E and G models, the only models that contributed results for individual years. The standard deviation for the TOA radiative flux change was 0.25Wm^{-2} for L, 0.46Wm^{-2} for E, and 0.05Wm^{-2} for G compared to the mean flux changes of 0.18, -0.08 and 0.20Wm^{-2} respectively. In the L model, the standard deviations for CC and COD changes for the BF vs PD were 0.11% and 0.07, compared to mean changes of -0.29% and -0.12. In the E model these standard deviations for CC and COD changes were 0.02% and 5.9, compared to mean changes of -0.06% and 3.4. So both models had larger variability in COD than CC changes."

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.

Response: To some extent the role of cloud microphysics on the indirect effect has already been explored in Quaas et al. (2009), and this study is meant to be a follow-on to that work. We think it is an excellent idea to reiterate more of the results from Quaas et al., and to use these in our discussion of the current study. We have also added maps of LWP and CDNC (closely related to droplet radius), although the other quantities you recommend including have not been saved by most of the models.

a) **We have added summary of some results of Quaas et al to the Introduction: "This study is largely a follow-up to the earlier AeroCom study of Quaas et al. (2009) that considered the liquid cloud indirect effect response to all (PD vs PI) aerosols in ten global models, and compared these responses to satellite retrievals. The study indicated a positive relation between cloud droplet number concentration (CDNC) and aerosol optical depth (AOD) that was generally well captured by the models. The models generally overestimated a positive relation between cloud liquid water path (LWP) and AOD, suggesting possible deficiencies in their cloud water conversion to rain, or autoconversion parameterizations. On the other hand, the models generally underestimated the positive relation between cloud cover (CC) and AOD. The modeled global mean cloudy sky forcing due to aerosols,**

scaled to the satellite CDNC-AOD regression slopes, was -1.2±0.4 Wm^{-2}."

b) **Implications of this have been added to the Discussion: "While both COD and CC changes apparently influence the cloudy-sky radiative flux changes, Quaas et al. (2009) found that the models generally overestimated the LWP-AOD relation but underestimated the CC-AOD relation, compared with satellite retrievals. Since most of the soot-reduction experiments (14 out of 18) had reduced cloud cover, stronger cloud cover response would tend to cause more positive radiative flux change."**

c) **We have added a table (Table 2) that summarizes model cloud microphysical schemes.**

d) **In section 3.1 we enhanced the explanation regarding the impacts on radiative flux: "The cloud radiative flux response to aerosol changes results from changes in cloud droplet number concentration (CDNC) which in turn affects the cloud optical depth (COD) and albedo (cloud albedo effect) and cloud cover (cloud lifetime effect). Typically the COD is proportional to the liquid water path (LWP) and inversely proportional to the droplet effective radius. Because the effective radius decreases as CDNC increases, the COD increases with LWP and CDNC."**

e) **We have added figures (Figure 2 and 3) showing CDNC and LWP and added the LWP values to Table 4. The following discussion is added: "The impact of LWP and CDNC changes on COD are apparent from comparing Figures 2, 3 and 4, which in most case are highly correlated. Many of the models have relatively stronger LWP changes over ocean and stronger CDNC changes over land, with the CDNC changes dominating in influence on COD."**

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.

Response: We added this sentence to section 3.3: "These flux changes resulting from the cloud changes are not strictly climate forcings, because the cloud changes include fast responses and feedbacks of the climate system."

Minor Comments:

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

determined in the CAM-Oslo?

Response: This text has been added: "Hygroscopicity of the mixed particles is determined by the volume mixing ratio of the species."

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".

Response: Yes, thank-you, this is clearer.

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

Response: This has been added, Figure 2.

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?

Response: We decided to remove this figure. Not only do we have the diagnostic for only 2 of the 6 models, one of the models saved CCN at cloud base and the other at cloud top, so they are not easily compared. For most of the panels the CCN change resembled the CDNC change, so we just show CDNC.

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.

Response: These have been added to the new Table 2.

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

Response: These have been added.

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

Response: We have added LWP to the table. Many models did not save CCN and droplet effective size so we do not include those. We have added the all-sky flux change, and the following text discussing this at the end of section 3: "All-sky (net) TOA flux changes for the experiments are also provided in Table 4. The average all-sky flux change for FF is -0.1 Wm^{-2} and for BF is +0.06 Wm^{-2}. The respective average cloudy-sky flux changes are -0.08 Wm^{-2} and +0.11 Wm^{-2}, and the all-sky and the clear-sky values generally have the same sign for most experiments. There is large variation among the models and the experiments in the relative importance of clear and cloudy sky flux changes. Note that it is not straightforward from our experiments to provide direct effects distinct from indirect effects, again because the cloudy-sky fluxes include above-cloud soot absorption."