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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate response due to carbonaceous aerosols and aerosol-induced SST effects in NCAR community atmospheric model CAM3.5" *by* W.-C. Hsieh et al.

Anonymous Referee #2

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Climate response induced by carbonaceous aerosol forcing remains an active while yet fully understood research topic. The authors studied this issue by using the NCAR Community Atmospheric Model, CAM3.5, coupled with different ocean components, a slab ocean model or a set of prescribed sea surface temperature (SST) data, attempting to reveal the role of ocean feedback in modulating the climate effects of carbonaceous aerosols. In order to make the carbonaceous aerosol effect more significant, a tripling of default loading in the model has been applied. Paired simulations, with or without including carbonaceous aerosols in the model were conducted for each model configuration to derive the climate response to carbonaceous aerosol forcing. The aerosol profiles used in the model were prescribed, i.e., the interaction between

aerosol processes and aerosol-induced climate response is excluded in the simulations.

The topic is definitely of interest to the reader of ACP and the research is original. The finding that ocean feedback plays a critical role in determining the climate response to carbonaceous aerosol forcing is very informative to the community. However, the paper could be organized better, certain discussions could also be made more specific or clarity. I would recommend acceptance of the paper for publication after the authors address the following indicated issues.

General Comment.

The authors might want to focus their discussions on the two sets of simulations that derived Et, Es, and Ea, i.e., the SOM set runs and the prescribed SST runs using SOMc output. All the other simulations and their purposes could be mentioned very briefly in Section 2, with results being provided as a supplementary material or up on request. The current discussions of these "other runs" in the text along with figures deviate the central point of the paper.

The authors also need to remind the reader and themselves about the scope of their simulations. The aerosol component addressed in this study is total carbonaceous aerosols including black carbon (absorbing and scattering) and organic carbon (mainly scattering unless "brown carbon" is concerned). This differs from some cited works where either black carbon or total anthropogenic aerosol was addressed. In many cases, "carbonaceous aerosol" or CA in short could be used instead of simply "aerosol" in discussion to better define the scope. In addition, when comparing to other works, the authors need to recognize the difference in targeted aerosol component.

Specific Comments.

Page 7356, line 7, "by factor of 3": I assume this means that (BC + OC) x 3, not BC x 3. While the atmospheric heating should be greatly increased to match a few

other works (a global mean of 3.7 w/mˆ2 is still much higher than the values in most models including AeroCOM models), the surface forcing would also be very high due to increased BC and OC, and the TOA total clear sky forcing would be more negative than previous setting. Perhaps all these should be indicated. I'm curious also about the values of TOA and surface clear sky forcings after applied 3x (BC + OC).

Page 7356, line 15, "High concentrations of black and organic carbon are emitted from. . .": the high concentrations of BC and OC during the dry season of MAM are likely due to a weaker sink, i.e., precipitation scavenging rather than a higher emission comparing to the summer monsoon season.

Page 7357, line 15, eq (2): I'm not sure if FSSTnc_SOMnc is more adequate than FSSTnc_SOMc for no-carbon case here in order to derive Ea. The equilibrium reached in a no-carbon-aerosol case with SOM that settled in a SST along with climate state would better reflect the ocean adjustment due to aerosols other than carbonaceous ones. The FSSTnc using SOMc would force a different equilibrium state. Nevertheless, the difference might not be significant.

Eq(3): under a linear response assumption, Es = Et – Ea. The equation could be written in this format.

Eq(4) – Eq(8): for clarity, these equations are not very significant to the discussions, perhaps Table 1 already serves the purpose to let the reader know about the existence of these additional simulations. A brief description here would be sufficient.

Page 7359, line 23, "The dimming effects of particles": please be specific, this should be referred to "carbonaceous particle". There are many some places throughout the text demanding for clarification. All the aerosol effects discussed in this study are the effects of CA.

Page 7360, line 5, "South" to "Southern".

Page 7361, line 16, "The possible reasons for the discrepancy. . .": Meehl et al. only

included black carbon while this study is about total carbonaceous aerosols, the former introduces a strong atmospheric heating and a surface cooling in close magnitude, the latter perhaps introduces a much higher cooling effect comparing to the atmospheric heating in terms of absolute magnitude.

Page 7363, line 10, "Because the opposite temperature change. . .", this sentence needs to be elaborated.

Page 7365, the first paragraph of 4.4: please note the difference in aerosol component between this study and the two other works cited here.

Page 7365, line 21, ". . .equals zero poleward of the subtropical maxima" is not quite clear.

Page 7368, line 5, "with positive changes of clouds" and a few following sentences: should it be made more specifically of low clouds?

Page 7369, Eq (8): the dynamic, thermodynamical terms etc. should be labeled here or briefly described in the text. In the description of the equation, "represents the difference from the no carbon simulation" should be "represents the perturbation caused by carbon aerosols".

Page 7370, line 18-22: the correlation between the dynamic term in the moist budget equation and precipitation change identified in this study is very interesting, It would be good to know how carbonaceous aerosols cause the perturbed flow.

Page 7371, line 28: "air-ocean model"?

Figures.

Fig. 2: it would make an easy comparison should all the panels have the same scale in vertical axis.

Fig. 4 and 5: perhaps only three panels are need here, Et, Ea, and Es.

Fig. 6: time series plots here are less informative. Could the zonal mean changes derived based on equilibrium period (last 50 or 70 years) be displayed instead?

Fig. 10: perhaps the same color scale could be used here.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7349, 2013.