

***Interactive comment on “Impacts of absorbing biomass burning aerosol on the climate of southern Africa: a Geophysical Fluid Dynamics Laboratory GCM sensitivity study” by C. A. Randles and V. Ramaswamy***

**Anonymous Referee #4**

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This paper describes a set of climate model experiments focused on the biomass burning season over southern Africa, to study how biomass burning aerosols affect the hydrologic cycle there. The authors demonstrate that absorbing aerosols, scaled to be as absorbing as indicated by TOMS/AERONET retrievals, stabilize the lower troposphere, enhance convergence of moist air from the Atlantic, and increasing cloud and precipitation in the region. I feel this study could make a worthwhile contribution to the literature on the topic and recommend its publication once the points below are addressed.

1. The authors should expand the discussion of comparison with other studies. A

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couple more that could be included are Koren et al., 2004; Miller et al. JGR 2004.

2. The authors state in several places that the biomass burning aerosols stabilize the atmosphere, however Fig. 4 suggests rather that they destabilize and actually enhance convective activity. This should be clarified.

3. How are OC and BC independently scaled to match AOD and SSA? Table S2 indicates that BC mass is smaller for SSAEX than for HIGHEX, and OC is larger for SSAEX than for HIGHEX. This seems backward?

4. Why do the scaled AODs and SSAs still not match the observations averaged over the region? I also agree with another reviewer that trying an experiment with smaller SSA, e.g. like from Abel et al. or SAFARI estimates would be very instructive.

5. The changes to model AOD and SSA were applied only to the aerosols below 4km. What portion of the original model aerosols is above 4km? Also TOMS, OMI is most sensitive to high-altitude aerosol and so is perhaps most applicable to the higher-level aerosol. So it seems inconsistent to ignore the high-altitude aerosol.

6. I am concerned that a totally different response might occur in an experiment with ocean response. Can the authors argue that ocean response would not change the results?

7. I suggest integrating the supplemental material into the main manuscript.

8. In the supplemental comparison of model with climate (T, precip, cloud, etc) I suggest comparing results from one of the more realistic model simulations like SSAEX rather than CTRL. Although maybe the various simulations would not be distinct from CTRL on these scales. Please add some discussion of this comparison of model and observed climate.

9. Supplemental Table 1: I suggest showing the model and TOMS/IMO AOD values in the table.

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