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Atmospheric Chemistry and Physics Discussions

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 #2

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This manuscript presents a GCM study of the biomass burning aerosol impacts on southern Africa's climate via direct and semi-direct aerosol effects (no aerosol-cloud interaction), with an emphasis on surface temperature and precipitation responses. While this study including some interesting aspects, the study is more like a routine modeling exercise without new insight, the approach is somehow questionable, and the findings do not match the claims of what this study offers. More specifics are given below. On the other hand, I think it is unrealistic to demand new findings for every publications and this study does seem to contain publishable results. I would suggest

the authors consider a significant revision and state clearly what this study is about.

General comments:

1. Significance of this study:

It has claimed in the introduction and other sections in the manuscript that this study considers "a wide range in bb aerosol forcing", which gives "substantial additional perspectives" relative to previous GCM studies and attempts "to address uncertainties in bb aerosol radiative forcing". (here bb stands for biomass burning). However, reading through the text, I found none of these statements are substantiated. There is no "wide range" considered – among the four model experiments, MOZEX shows unrealistic features of bb in southern Africa and WHITE is just a hypothetical case assuming all bb aerosol are non-absorbing, which does not fit in the "range". Only two cases are somewhat close to the reality, HIGHEX and SSAEX, which are very close to each other. A realist range in terms of AOD and SSA can be defined from observed multi-year or seasonal variations of these values from bb aerosols, which the authors did not do.

I don't see this studying adding any substantial additional perspectives and addressing uncertainties in biomass burning aerosol radiative forcing, either.

2. Method:

The approach of using TOMS aerosol data is problematic. First, the authors used the TOMS AOD and SSA (plus AERONET when needed) to generate the column AOD and SSA map, then they used this map to adjust the MOZART BC and OC in order to match the TOMS-based AOD (in HIGHEX and SSAEX) and SSA (in SSAEX) for input to the GCM simulations. However, the adjusted AOD and SSA are still substantially different from TOMS-based values, as the AOD at 500 nm is significantly higher and the SSA significantly lower than TOMS (Fig. 1a and 1b). It is also puzzling that over some area the HIGHEX AOD is lower than both TOMS and MOZEX (Fig 1a), and the TOMS SSA values outside of the thick plume were not even considered in the model adjustment

(Fig 1b). The use of the TOMS in model adjustment thus needs to be better explained. It is interesting that the authors recognize the differences after the BC/OC adjustment and went on to say "However, since our purpose in this sensitivity study is to try and bound the real world, it is not necessary that we precisely mimic observations, which are also very uncertain". Do you consider TOMS observations as "real world" or not? If they are, why did it not matter? If they are not and very uncertain, why did you bother to use them?

Second, I don't understand why adjust the MOZART BC and OC profiles just below 4 km to match TOMS. Unless in-situ or some other vertical measurements have suggested that the MOZART simulated aerosol only underestimates the BC and OC within the lowest 4 km but not aloft, the entire column mass should be adjusted accordingly. Also, why the BC vertical shape in SSAEX is so different from other shapes (Fig S2), i.e. the BC shows a maximum at 900 mb in SSAEX but almost a minimum in other experiments, if the only adjustment is overall BC/OC fractions below 4km?

Minor specific comments about the text and figures:

1. Bottom of p. 9332 and top of p. 9333: uncertainties in bb aerosol radiative forcing are also in the amount of bb emitted to the atmosphere and its relative height to clouds.

2. p. 9734, line 6: related to what I stated earlier: "substantial additional perspectives" – in terms of what? Be specific if there is such information from this study.

3. p. 9735, line 11-16: The problem here is that bb also produce sulfate aerosol, and BC and OC also have anthropogenic sources other than bb. The authors should at least demonstrate/explain that over the study regions most (xx%) of bb aerosols are BC and OC and yy% of total aerosols are from bb in the studied area and season.

4. p. 9736, line 15-16: the overestimate of AOD by EP-TOMS is mostly because of the cloud contamination in such a coarse resolution pixel size.

5. p. 9736, last paragraph: Is there a discontinuity between TOMS and AERONET

which may be a problem for your gap-filling method? For example, in a gridbox where both TOMS and AERONET are available, how do they agree with each other? I am not asking you to do thorough comparisons between TOMS and AERONET, but at least you should acknowledge such a problem and justify your approach.

6. p. 9737, line 11: Forcing efficiency – is this the shortwave direct forcing per unit of AOD at 500 nm? Clarify.

7. p. 9737, method of adjustment: see my general comments.

8. p. 9738, line 7-8 and Fig 2, comparison with AERONET: Have any of these stations been used in constructing the maps in 1a and 1b to adjust the modeled BC/OC? If so, then this comparison is not independent and not valid. Also, why not comparing SSA with AERONET retrievals?

9. p. 9738, line 11-13: Is SSA=0.9 in Fig 1d and 1f the area average? You should, to be more appropriate, compare the SSA with SAFARI-2000 over the measurement area to see how much overestimate it is. In addition, the TOMS map (Fig 1b) shows SSA value of 0.96! Are they wrong? Why didn't you consider the TOMS value in your bounding experiments? At a minimum, you should comment on it!

10. p. 9738, last sentence before section 3, regarding the BC and OC mass adjustment: This should be moved to earlier part when you talked about the adjustment. I was wondering how much mass you have to add when I was reading that part.

11. Figure 1e and 1f: They provide very different content than 1a-1d and should be an independent figure being introduced in the "Results" section, especially currently Figure 1f was introduced after Fig 2.

12. p. 9738, line 22: Is ASO over southern Africa an isolated "climate system"?

13. p. 9739, line 2: "compare and contrast the range of climate response possible given a realistic range" – what is the realistic range? As I said before, there is no range defined in this study, given the closeness of HIGHEX and SSAEX and unrealistic char-

acteristics of MOZEX and WHITE. The authors should look the AOD and SSA ranges over the studied area from the long-term observations from TOMS or AERONET or other data to come up with a “realistic range” of bb aerosols over southern Africa.

14. P. 9739, line 12-13: If the bb forcing estimated from this study and from Abel et al. (2005) are consistent despite the large difference in SSA (0.9-0.91 from HIGHEX and SSAEX and 0.84 in Abel et al.), does this mean that SSA doesn’t matter at all? What is the implication?

15. p. 9741, line 25: Again, be clear about “range”. There is no realistic range considered in this study.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9731, 2010.