

Interactive comment on “The Effect of South American Biomass Burning Aerosol Emissions on the Regional Climate” by Gillian D. Thornhill et al.

Anonymous Referee #2

Received and published: 18 December 2017

I have reviewed the manuscript titled “The Effect of South American Biomass Burning Aerosol Emissions on the Regional Climate.” The manuscript uses observations from the SAMBBA campaign to constrain the aerosol scheme in the HadGEM3 model. Multi-year experiments are run using representative low and high biomass burning years and the resulting impact to the regional climate is assessed.

While the study fits within the scope of ACP, after careful consideration I cannot support publication as written. I believe that significant revisions are required prior to consideration in ACP. The manuscript is well written to some extent, but does contain several typos and one section needs to be made more concise. More importantly, I believe there are some flaws in the assumptions that must be addressed by the authors, and the novelty of the study in my opinion (running several years of experiments using a

C1

low and high case of biomass burning) is not sufficiently explored. Below are my major comments, followed by more specific comments.

Major Comment 1: The manuscript does not include any model comparisons with observations. First, I would expect that some of the key assumptions made in the study would be verified through comparisons to observations, such as the assumption that the GFEDv3.1 emissions data should be scaled by a factor of 2. Is this because of issues with the GFEDv3.1 database or because the hygroscopic growth in the model was lowered? Second, I would expect that the model results be compared with in situ and remotely-sensed observations to ensure it is appropriately representing the regional dynamics, microphysics, and thermodynamics of the atmosphere prior to running comparison studies between low and high biomass burning years.

Major Comment 2: Similar to a comment made by Reviewer 1, isn't it a rather strong (and incorrect) assumption that BC and OC are lumped into one species, given their different properties? GFED3.1 emissions are provided in terms of vegetation type but it appears that an average SSA is used for all emissions. As a result, the model isn't adequately representing the varied aerosol composition that occurs due to the different vegetation types from the central Amazon down to the Cerrado. The AERONET inversions that you show in Figure 4d support that a single aerosol composition doesn't make sense if the area of study is represented by the relatively large black square in Figure 5. I believe that Ten Hoeve et al. 2016 is relevant to reference here (<http://onlinelibrary.wiley.com/doi/10.1002/2015GL066873/full>). Figure 2 in that paper shows that changes in the aerosol composition will lead to changes to the impact of aerosols on cloud fraction. If the aerosol composition was allowed to vary, the resulting impact to cloud properties may be different.

Major Comment 3: The study a few times refers to variability in deforestation. The abstract specifically states, “This study therefore provides an insight into how variability in deforestation and biomass burning emissions may contribute to the South American climate, ...” The way that sentence (and others that mention deforestation in the

C2

manuscript) reads makes it sound like the study performs experiments on the effect of deforestation on land-atmosphere interactions through changes to the land surface in the model. Another example is Page 3 Line 2. I believe this is misleading.

Major Comment 4: The “impacts of the monsoon” section is long and can be significantly shortened, especially considering that “within the constraints of the experiments performed so far, it is difficult to suggest how much, and what kind, of an effect there may be (typo) on the monsoon as a result (typo) of the biomass burning.” Because there is not a strong signal, I would keep the discussion of this to a minimum. For instance, I got lost between Page 23, Line 9-24 when Figure 17 was discussed, and then again discussed on Page 25, Lines 14-26 after Figures 18 and 19 were discussed. It seems like this entire section could be consolidated. Furthermore, I would have expected to see some comparisons of your results to other studies that have already modeled the effect of biomass burning on the monsoon, such as Zhang et al. 2009.

Major Comment 5: Several studies have looked at aerosol cloud interactions over the Amazon, using aerosol and microphysics schemes that are more sophisticated than the bulk scheme used here. I would suggest comparing your results briefly to theirs (e.g. Wu et al., 2011, Ten Hoeve et al., 2012). However, I believe the most novel aspect of this study is that multi-year simulations are run, in turn reducing the effect of meteorology on the results. You can run the same high BB emissions over dry and wet years, warm and cool years, etc. and compare to low BB emissions over those same years. I think it would strengthen the paper to include a discussion on how the effect of different years with different meteorology either affect the H BB – L BB results, or don't.

Specific Comments: 1. Line 24, Page 2: Missing comma

2. The sentence starting with “Changes in surface fluxes” on Page 3 Line 17 should have a reference

3. Please describe how the aerosol cloud interactions are parameterized in the model. There is some discussion of the aerosol scheme but not much discussion of how these

C3

interactions are parameterized.

4. Please describe in more detail how you determined the statistical significance. (Page 10, Line 10)

5. Why do you suspect surface temperature has not changed appreciably given other surface flux changes? The percent change in Table 1 is 0.0%.

6. The sentence beginning with “The difference (high-low)...” on Page 12, Line 1 seems obvious and could be removed.

7. Page 13, Line 3: What area outlined? In one of the figures?

8. Page 14 discusses how biomass burning reduces deep convection caused by the stabilization of the atmosphere, reduction of surface fluxes, etc. However, many studies have shown a destabilization of the atmosphere above the aerosol layer due to heating at the aerosol layer, leading to increased convection above the aerosol layer. If you look at a cross-section of cloud water, do you see increases above the aerosol layer?

9. I believe there is not enough evidence to make the statement on Page 14: “. . .suggesting that the reduction in cloud fraction may be due predominantly to this mechanism.” How do you know if the reduction of cloud fraction is due to the reduced deep convection and in turn reduced cloud top anvils, or simply due to cloud burn-off of cumulus at the aerosol layer? Each effect impacts a different type of cloud. I believe further investigation is required to ascertain the source of the cloud fraction changes.

10. There are no units included in the caption of Figure 8.

11. Please reword the sentence on Page 19, Line 3 starting with “Within”. Confusingly worded as written.

12. Similar to Major Comment 4 above, Figure 18 a,b,d,e,g,h don't add much value because you can't see the difference between these two simulations easily. Perhaps some of these figures could be moved to the supplemental. Same with Figure 19.

C4

