Response to the comments of the reviewer #2

We truly appreciate all the constructive comments and suggestions from both reviewers. We have adopted all the suggestions in our revised manuscript. The following are our point-to-point responses to the reviewers' comments (the comments are shown with Italic and bold font).

General Comments

The majority of the paper presents the climate effects of BBCA, without much interpretation, or attempts to explain the resulting signals. Furthermore, if cloud changes are so important, what causes the cloud changes? Are they changing due to general circulation effects (like the northward CONCLD/ITCZ change), or are they changing due to stability effects, changes to relative humidity, etc?

In addition, the climate signal that is explained–the northward shift of the ITCZ–is lacking sufficient support. The authors suggest that the northward shift is due to more NH warming. But they never show the NH warms more than the SH, nor is it intuitive that this would be the case. Further more, why is the shift northward in all months except DJF, when it moves southward?

An analysis of the temperature change, beyond the Hovmöller Tsfc plot in Figure 10a, would be very useful in general, as well as to support the ITCZ argument. This is further warranted by the fact Figure 10a appears to show more SH, as opposed to NH, Tsfc warming, which would be inconsistent with the above argument. Also, are Tsfc changes related to the cloud changes? As another example of insufficient interpretation, figure 10a also shows significant mid- and high-latitude warming, yet no BBCA emissions occur here (especially pole ward of 60 degrees). Yet this signal–which appears to be a significant and important response– is never discussed.

The revision needs to include more interpretation and explanation of the climate response, such as those just mentioned.

These excellent points have been well taken. We have rewritten the Results section to adopt the suggestions of both the reviewers.

First of all, we have made it clearer in the revised manuscript that most results presented are differences between two runs that both included aerosol forcings. Therefore, such differences in some cases might have different signs than those of changes caused by aerosol forcing. This difference in sign only demonstrates the different strength of aerosol effects when configured differently (i.e., seasonal

vs. non-seasonal in emissions). With this clarification and the new analyses, we are able to present a much improved discussion specifically on the anomalous precipitation distributions (ITCZ switch) among others.

Furthermore, we have made several major revisions specifically in responding to the reviewers' comments, including:
1) Addition of new figures and figure panels to show specifically the seasonal variation of differences in convective cloud cover along with convective precipitation caused by the seasonality of BBCA emissions (new Figure 11). Corresponding discussions are also added;

2) Addition of detailed discussions of the difference caused by BBCA seasonality in large-scale circulation in responding to the direct forcing of aerosols. In particular, in the previous manuscript we did not clearly indicate the fact that the overall optical effect of BBCA aerosols is scattering rather than absorbing. This has been indicated in the revised paper;

3) Revised discussion that made it more clear that the tropical ITCZ difference is to respond to the heating or cooling effect caused by aerosols, which is not necessarily reflected in actual surface air temperature change in many cases. The changes in the latter parameter are much more complicated and easy to misunderstand;

Clarification is also added in both Model and Configuration and Results sections that our study did not include the so-called indirect effects of aerosols (i.e., the microphysical path of aerosol effects on clouds). In our analysis, we find that the changes in particularly convective cloud cover and precipitation are dominated by the alternation to the large-scale circulation rather than local stability by aerosols.

In the revised manuscript, we have also emphasized that the focus of our paper is to address the difference in climate response caused by the seasonality of BBCA emissions rather than to carry out a full-scale analysis on these detailed features of responses themselves. The latter effort exceeds the scope of our current paper.

Specific Comments
1. Please explicitly state that you are modeling and investigating direct effects only and that indirect (i.e. cloud microphysical) effects are not considered. This needs one, if not two sentences. Unless I am mistaken and you are using "direct" radiative forcing in a completely unconventional way? Perhaps explicitly defining DRF would be useful.

We have added the "direct radiative forcing (DRF)" in the first place where DRF is cited. We also specifically indicated in the revised manuscript that in this study

we only include the direct radiative forcing of aerosols (in the end of the third paragraph of the Model and Configuration section).

2. Not only do BB aerosols possess significant seasonal variation, but interannual variation as well. I think this should be mentioned, since it adds to the importance of BBCA. Also, it appears your results may suggest the importance of BBCA during warm versus cold phases of ENSO, etc.

The reviewer's point is well taken. We have added in the end of the second paragraph of the Model Description section that "Note that in this study we address the issue of climate effects associated with the seasonality rather than interannual variability of BBCA emissions. The latter effect needs to be discussed in a future study."

We have also indicated in the revised manuscript that because the bulk effect of BBCA aerosols is scattering rather than absorbing solar radiation, the overall effect is similar to the cold phase of ENSO.

Abstract

1. line 7 says there is a decrease in internal mixtures. Isn't there an increase in MOS?

We thank the reviewer for indicating this oversimplified description in the Abstract. In the revised manuscript we made it clearer that the mixture of black carbon and sulfate decreases while the mixture of organic carbon and sulfate increases.

2. line 12 says differences in Tsfc and heat fluxes are limited to BB source regions. I do not understand this, since Figure 10 shows otherwise. Also, the subsequent sentence seems to imply Tsfc is not affected by cloud changes, which I do not understand.

This statement has been deleted.

3. I do not follow the last sentence.

The last sentence has been deleted.

Introduction

1. page 9434, line 6. Wang 2004 is referenced to support BC-induced global scale climate changes. Yet, this paper concludes the climate effects of BC aerosols are more significant on the regional, rather than the global, scale.

We have revised the paragraph to: “Some studies show significant regional changes in climate variables by BC aerosols distributed globe-wise (Wang, 2004; 2007). Particularly, Wang (2004) found that BC emissions cause a shift of the precipitation center in the ITCZ and a change in the snow depth in the mid- and high- latitudes of the Northern hemisphere, but not a significant change in global-mean surface temperature. BC radiative forcing may also cause “remote climate impact” and sometimes, it is very similar to the change caused by El Nino/Southern Oscillation (ENSO) warm phase event in the tropical Pacific region (Wang, 2007). However, these studies show the seasonal anomalies in climate variables with annually constant BBCA emissions, rather than seasonally varying BBCA emissions”.

2. page 9434, line 12. Are BC aerosols associated with the positive or negative phase of ENSO?

ENSO warm phase, see above.

3. page 9434, line 1. Remove "at" from "...which it is a..."

We have corrected it.

4. line 11. GEIA=Global Emissions Inventory Activity. How do the GEIA estimates compare with others (e.g. GFED)?

We should have made it clearer in the paper that we used GEIA version 1 in our study, which is a specific climatological emission inventory. GEIA itself now actually represents coordinated efforts in deriving emission inventories. GEIA portal provides access to other emission estimations that include interannual variations in biomass emission estimations (mostly made using year-to-year satellite fire retrievals) such as GFED and RETRO.

We have revised the second sentence of the first paragraph in the Model and Configuration section to “The seasonal biomass burning emission of carbonaceous aerosols was prepared based on the monthly biomass burning BC data of the climatological emission inventory of GEIA version 1 (http://www.geiacenter.org). Note that there are several annually varying estimations of biomass burning emissions available, derived by using timely varying satellite fire retrievals, including the Global Fire Emissions Database (GFED; e.g., Van der Werf et al., 2006)”.

5. The vertical distribution of aerosols, relative to clouds, has a significant impact on whether cloud cover increases or decreases. This should be mentioned, along with a reference.

This is an important point. We have revised the sentence in the last paragraph and 3.2 (P.9441, Line 1) to “When the climate responses are included…reflecting in particular cloud cover changes forced by the aerosol DRF as well as the enhancement/reduction of such forcing due to cloud cover changes below the absorbing aerosol layer (e.g., Kim et al., 2008)”.

Model and configurati*on*
1. Page 9437, line 24. Remove "the difference as"

We have corrected it.

1.Page 9438. Discussion of the tables is confusing. Try to re-structure the paragraph

We have rewritten the corresponding sections discussing Tables.

2. Page 9438, line 26. More negative relative to what?

We have added “relative to the case of constant BBCA emissions” in the text.

3. Page 9442, line 25. Figure 10 should be 11. Although there is inconsistency with Figures 10 and 11, between what the texts say, and what those figures show. For Example, Figure 11 does not show monthly means of CONCLD. No figure does.

We thank the reviewer for pointing out these errors. We have corrected them.

Tables
1. Tables 1 and 2 are a bit confusing. I suggest adding a sentence to the caption Saying, “Top two rows show all-sky values, bottom two rows show clear-sky values”.

We added the sentence.

2. Replace the caption of Table 2 with, “As in Table 1, except for online runs”.

We have changed it as suggested.

3. Remove the four instances of “in” in "...relative to in non-seasonal..."

Done.

Figures

1. The Hovmöller plots are very useful and a concise way to show the monthly effects.
2. Generally, the figures are too small, which is probably because most include several Panels. But maybe this is not an issue for pdfs/online viewing.
We have reproduced all the figures and tried our best to make them easier to read.

3. Figure 1. Units?

We have added units in the caption.

4. Figure 8. Change scale label resolution.

Done.

5. Figure 9. What are the white areas?

These are the places with values out of the color range. After adjusting the color scale, the white areas disappeared.

6. Figure 10. At what level is QRS taken? Is it the column average?

Column average.

7. Figures 11 and 12. To explicitly show a northward shift, the control values (e.g. PRECC, CONCLD) should also be plotted.

These excellent points have been adopted. In the revision, we have reproduced the original panels and also added several new figures. More in-depth discussions are also added. It appears to us that these additions have greatly enhanced the Results section.

Specifically, the new Figure 12 includes now the vertical cross section of annual and zonal means of CONCLD in: (a) reference run (= no aerosols); (c) the differences between the annual constant BBCA emission run and reference run; and (e) the differences between seasonal BBCA emission run and reference run. These new panels make the original panel showing the difference between seasonal and non-seasonal BBCA emission runs more informative.

The differences in CONCLD between seasonal and non-seasonal BBCA emission runs in four seasons have also been included as the new Figure 11 (c), (d), (e), (f).

Similar new figures are produced for PRECC as well. They are the new Figure 12 (b), (d), (f) in the revised manuscript.

We have specifically made clarification whenever we applies that the “northward shift” of Hadley cell mentioned in the previous manuscript is not a real phenomenon. It is a relative difference in strength of changes due to BBCA aerosols in two differently configured simulations, both actually with the same sign.

8. Again, I suggest a spatial plot of Tsfc or T_troposphere, and more analysis of the Temperature response.

The signal of temperature regardless of surface or tropospheric values, could be affected by many factors, not necessarily locally correlated to aerosol forcing. We decided to focus our discussions on shortwave heating and convective cloud cover and precipitation. The discussion of surface temperature response has been removed from the paper.