

We thank the reviewer for his/her careful reviews and helpful comments. The manuscript has been revised accordingly and our point-by-point responses are provided below. (Reviewer's comments are in italic and the responses in standard font).

Reviewer #1

This paper examines the global and regional radiative forcings by black carbon and organic carbon aerosols from open fires. The authors use the NCAR Community Atmosphere Model version 5.3 (CAM5) with the four-mode version of the modal aerosol module (MAM4) and employ two methods to calculate forcing. In one method, they follow Ghan et al. (2013), which may produce a more robust estimate of forcing. In the second method, they follow a more traditional approach. The authors find that top-of-atmosphere (TOA) forcing from aerosol-cloud interactions dominates the total global forcing (-0.70 W m^{-2}). When aerosol-radiation interactions and aerosol effects on snow are also considered, the global annual mean forcing from open fire aerosols is -0.55 W m^{-2} . The authors also estimate the climate impacts of fire aerosols.

The paper leads to no startling new conclusions, but may provide a more accurate estimate of the global and regional climate impacts of aerosols from open fires. The paper should be revised in response to the major criticisms and resubmitted.

Reply: We thank the reviewer for helpful comments. The manuscript is revised following the comments and criticisms from the reviewer.

Major criticisms.

1. The paper needs to make more clear what is new in the results, or why this approach represents a substantial improvement over previous results. Central to this paper should be the answer to this question: Why does this research give us greater confidence in our knowledge of the effects of fire aerosols on climate?

In Lines 147-150, the text lists a few improvements, but supplies little elaboration. The improvements are: (a) higher spatial resolution, (b) use of the latest CAM5 model with updated MAM4, (c) calculation of daily instead of monthly fire emissions, and (d) use of an alternative methodology to calculate radiative forcings of aerosols (Ghan 2013). It's not clear why the relatively small increase in spatial resolution would lead to better results, or why calculation of daily instead of monthly fires matters. Almost no information on the updates in MAM4 is given or what difference they make for forcing calculations. A detailed explanation of the benefits of the Ghan (2013) method over other methods is absent.

→Reply: We thank the reviewer for the comments. We now make it more clear what is new in our results, and why our approach represents a substantial improvement over previous results in the revised manuscript.

Specially, following the reviewer's comment, we elaborate more on the improvements of our approach and model configuration in the revised manuscript:

- (a) *higher spatial resolution*. A model resolution change from 2 degree (used in previous studies) to 1 degree (in this study) represents a resolution increase by 4 times. A higher resolution allows more efficient transport of aerosols from the sources to remote regions due to reduced wet scavenging of aerosols as a result of less frequent collocation between aerosols and clouds at higher resolutions (Ma et al., 2013; 2014). Model resolution has also been shown to be important for aerosol radiative forcing due to aerosol-cloud interactions (Ma et al., 2015).
- (b) *use of the latest CAM5 model with updated MAM4*. Compared to the 3-mode version of MAM (MAM3) used in previous studies, MAM4 includes a primary carbon mode to explicitly treat the microphysical ageing of primary carbonaceous aerosols (POM/BC) in the atmosphere. Primary carbonaceous aerosols are emitted in the primary carbon mode and transferred to the accumulation mode due to aerosol condensation and coagulation. Because of a lack of primary carbon mode, MAM3 assumes that primary carbonaceous aerosols are emitted in the accumulation mode and thus instantaneously mixed with other soluble aerosol species (e.g., sulfate), subject to wet scavenging in the accumulation mode. As a result, MAM4 has higher BC and POM burdens over MAM3 in the remote regions by ~30%.
- (c) *calculation of daily instead of monthly fire emissions*. Using daily emissions will allow the model to consider the effect of fast changes in fire emission flux on the local atmospheric conditions. It is expected that using the monthly mean emission flux the model can't consider the effect of the extremely strong fires, thus it might underestimate the fire forcing for such cases. Considering that the aerosol effect is often non-linear, using higher temporal resolution emission data will make a difference, at least for the effect on daily extremes.
- (d) *use of an alternative methodology to calculate radiative forcings of aerosols (Ghan 2013)*. Ghan (2013) provides a more accurate method to calculate the radiative forcing of aerosols. Central to this method is that the radiative forcing due to aerosol-radiation interactions must be calculated in the presence of clouds (i.e., under all-sky condition, $\Delta(F - F_{\text{clean}})$), and the radiative forcing due to aerosol-cloud interactions be calculated under the condition of no aerosol effects on radiation (i.e., $\Delta(F_{\text{clean}} - F_{\text{clean,clear}})$). F_{clean} is calculated from the diagnostic radiation call with aerosol scattering and absorption neglected, and $F_{\text{clean,clear}}$ from the diagnostic radiation call with both aerosol and cloud scattering and absorption neglected. With the radiative forcing decomposition of this method, the impact of aerosols on surface albedo is also quantified (i.e., $\Delta F_{\text{clean,clear}}$).

In addition to the above improvements in model configuration and approach of

calculating radiative forcings, we validate the model performance through a comparison of our modeled AOD and SSA with the AERONET data; modeled radiative forcing due to aerosol-radiation interactions compared with satellite-derived estimations, and modeled BC-in-snow concentrations with observations in Northern China and the Arctic. These model improvements and evaluations give us greater confidence in our knowledge of the effects of fire aerosols on climate.

Some notable key findings from this study are highlighted in the conclusion section:

- a) Fire aerosol radiative effect due to ARI in the Arctic regions ($0.428 \pm 0.028 \text{ W m}^{-2}$) is larger than that in the tropical regions ($0.172 \pm 0.017 \text{ W m}^{-2}$), although the fire aerosol burden is largest in the tropics, which results from the larger amount of low clouds in the Arctic.
- b) The large cloud liquid water path over land areas and low solar zenith angle of the Arctic favor the strong fire aerosol radiative effect due to ACI (up to -15 W m^{-2}) during the Arctic summer.
- c) The global annual mean surface albedo effect (SAE) of fire aerosols over land areas ($0.03 \pm 0.10 \text{ W m}^{-2}$) is relatively small and insignificant.
- d) The fire aerosols reduce the global mean surface air temperature (T_s) by $0.03 \pm 0.03 \text{ K}$ and precipitation by $0.01 \pm 0.002 \text{ mm day}^{-1}$. Significant reductions of precipitation in southern Africa and NH high-latitudes are noticed.

2. The paper uses outdated terms to describe radiative forcing by aerosol, and does not adequately describe what adjustments to the model meteorology have been allowed in the forcing calculations. Following IPCC AR5, the authors should use the terms aerosol-radiation interactions (ARI), aerosol-cloud interactions (ACI), and forcings due to surface albedo changes (Boucher et al., 2014; Myhre et al., 2014). ACI in the IPCC framework includes the effects of aerosols on cloud droplet number, cloud lifetime and takes into account the “semi-direct effect” of absorbing aerosols. The ACI category of forcings is useful as it makes it unnecessary to distinguish between the sometimes competing effects of aerosols on clouds.

→Reply: Thank for the suggestion. Following the reviewer’s comment, we now use the terminology of the radiative forcings by aerosol from IPCC AR5 in the revised manuscript. In our results, the cloud radiative effect (CRE), i.e., radiative effect due to aerosol-cloud interactions (ACI) includes the effects of aerosols on cloud droplet number and cloud lifetime through acting as CCN, and the semi-direct effect of absorbing aerosols.

All the atmospheric variables (including temperature, precipitation, and circulation) are allowed to adjust. However, with sea surface temperatures (SST) and sea ice are prescribed in the simulations, only the rapid adjustments are taken into account. We have made it clearer in the revised manuscript.

The authors should further state whether they calculated radiative forcings (RF) or effective radiative forcings (ERF), which take into account the rapid adjustments to a

range of meteorological variables. If these are ERFs (and they seem to be), the authors need to make clear what meteorological variables they allowed to adjust. The authors should emphasize in the abstract and conclusions that the forcings they report are relative to the case of no fires, and not to conditions in 1750s.

→Reply: Yes, with the method of Ghan (2013), the effective radiative forcings (ERF) are calculated in this study. All the atmospheric variables (including temperature, precipitation, and circulation) are allowed to adjust. However, with sea surface temperatures (SST) and sea ice are prescribed in the simulations, only the rapid adjustments are taken into account. We also emphasize in the abstract and conclusions that the radiative effects we report are relative to the case of no fires. We now use the term “radiative effect” instead of “radiative forcing” of fire aerosols throughout the text.

3. It's not clear why the paper does not consider the effects of fire aerosols on sea ice albedo. Is this not an important forcing term? Also the authors neglect the issue of brown carbon, which has recently been suggested as a main component of primary organic matter (POM) in fire plumes (Feng et al., 2013). MAM4 may not be capable of simulating brown carbon, and this should be acknowledged.

→Reply: In our simulations with the stand-alone CAM5, sea surface temperatures and sea ice are prescribed, and thus the effects of fire aerosols on sea ice albedo are not considered. The effects of fire aerosols on sea surface temperatures and sea ice albedo will be presented in our future study using a slab ocean model coupled with CAM5.

The effects of POM as brown carbon are not considered in MAM4, and we acknowledge this in the revised manuscript.

4. The authors report a large number of changes in global mean variables without giving uncertainty ranges or stating which changes are statistically significant. Given that many of the variables have been calculated using an ensemble of simulations, uncertainties should be easy to calculate.

→Reply: Following the reviewer's comment, we added the uncertainty ranges ($\pm 1\sigma$ uncertainty) for changes in global mean variables in the revised manuscript.

Other criticisms.

Title: Given the distribution of fires in Figure 2, it looks like the authors include agricultural fires in their analysis, and so the term “wildfire” should be changed to “open fires.”

→Reply: Yes, the agricultural fires are included. We changed the term “wildfire” to

“open fires”.

Abstract. The abstract should state the time period under investigation. Also large regional forcings should be quantified, as they could have importance for regional climate.

→Reply: We added the time period (2003-2011) in the abstract. Also the following sentence is added in the abstract for large regional forcings: “REs due to fire ARI and ACI in the Arctic (0.43 ± 0.03 and -0.82 ± 0.09 W m⁻², respectively) are stronger than those in the tropics (0.17 ± 0.02 and -0.70 ± 0.05 W m⁻², respectively), although the fire aerosol burden is higher in the tropics.”

Introduction. The introduction is too long. The first paragraph should make clear exactly what problem is being considered, and it should succinctly explain why this investigation represents a major improvement over past research. Throughout the introduction, many old references brought up – e.g., Chuang et al. (2002) or IPCC AR4. The authors should condense the introduction and focus on Chapters 7 and 8 in AR5 and subsequent papers – e.g., Myhre and Samset (2015), Chakrabarty et al. (2014), and many others. Missing from the introduction is a discussion of the radiative effects of organic vs black carbon.

→Reply: Thanks for the suggestions. Following the reviewer’s comment, we made it clear in the first paragraph what problem is being considered in this study by adding the sentence: “An qualification of radiative forcing of fire aerosols is the first step to reduce these uncertainties [Ward et al., 2012]”.

We added the explanation why this investigation represents a major improvement over past research (see our response to the reviewer’s major criticism #1).

We condensed the introduction and focused on Chapters 7 and 8 in AR5 and subsequent papers. We removed the old references, e.g., Chuang et al. (2002) or IPCC AR4 in the revised manuscript.

The following discussion of BC and POM’s radiative effects are added: “Although there are many studies quantifying the RE of fire aerosols, a further investigation is still needed, as current estimations of the RE of fire aerosols from climate models are still associated with large uncertainties [Myhre and Samset, 2015; Chakrabarty et al., 2014], and the REs of fire POM versus BC are even less clear.”

Line 174. The authors state that MAM4 “significantly increases (and improves) the BC concentrations in the Arctic....” Why does inclusion of the primary carbon mode in MAM4 improve the treatment of microphysical aging of BC? How did the authors decide that inclusion of this mode “significantly” improves the BC simulation? By what measure? Elsewhere the authors state that MAM4 “realistically represents the external/internal mixing of BC” (Line 578). But no detail is given about these improvements.

→Reply: In the 3-mode version of MAM (MAM3), due to a lack of primary carbon mode, BC is emitted directly into the accumulation mode, and thus is instantaneously mixed with other soluble aerosol species (e.g., sulfate), subject to wet removal by clouds and precipitation. MAM4 includes an additional primary carbon mode on top of MAM3. BC is emitted in this primary carbon mode, and is gradually transferred to the accumulation mode due to the microphysical aging (condensation and coagulation). Aerosol in the primary carbon mode is less hygroscopic than that in the accumulation mode, and thus is less susceptible to the wet scavenging by clouds. Therefore, BC concentration from MAM4 is increased, especially in the Arctic, which improves the agreement with observations. The details of MAM4 and comparison with MAM3 are given in Liu et al. (2016). Please see also our reply to the major criticism #1 for the description of BC representation in MAM4 versus in MAM3.

We added the following details in the introduction of the revised manuscript: “MAM4 includes an additional primary carbon mode on the top of MAM3 to explicitly treat the microphysical ageing of primary carbonaceous aerosols (POM and BC) in the atmosphere. POM and BC in MAM4 are emitted in the primary carbon mode instead of the accumulation mode as in MAM3. Thus MAM4 increases the BC and POM concentrations over MAM3 due to reduced wet scavenging of POM and BC in the primary carbon mode with a lower hygroscopicity than that in the accumulation mode.”

Section 2.3. See major criticism #2 above. Please rewrite using IPCC AR5 convention for describing forcings.

→Reply: Done. See our reply above to the major criticism #2.

Results. The results section rambles. The authors should decide which are the key results and provide more detailed explanations of the mechanisms driving these results. Also, the statistical significance of results should be given, where possible. Since the authors performed an ensemble of simulations, many results can be reported with one standard deviation uncertainty. For example, what is the uncertainty of the forcings calculated following Ghan 2013? Is the -0.03°C temperature effect of fire aerosols statistically significant?

→Reply: Thanks for the suggestions. We revised the results section and emphasized the key results. Please see our response above to the major criticism #1 for the key results. We have provided more detailed explanations of the mechanisms driving these results.

Following the reviewer’s comment, we added the statistical significance of results with one standard deviation uncertainty. This is done for the uncertainty of the forcing calculated following Ghan (2013) as well as the temperature and precipitation

changes due to fire aerosols.

Finally, the forcings calculated for specific regions should be compared to recently published estimates – e.g. Brieder et al. (2014) for the Arctic and Sena and Artaxo (2015) for South America.

→Reply: Thanks for the suggestion. We tried to compare our forcings with those estimated from Brieder et al. (2014) for the Arctic. However, we found that this study reported the distribution, aerosol optical depth, and absorption of Arctic aerosol components and source contributions calculated using the GEOS-Chem model, and did not present the forcing estimates.

Following the reviewer's comment, we added the following comparison of our forcing estimates with those from Sena and Artaxo (2015) for South America in the revised manuscript: "The fire aerosol RE due to ARI over South America for the period of 2000 to 2009 is estimated with the TOA shortwave flux from CERES (Clouds and Earth's Radiant Energy System) and AOD from MODIS by Sena and Artaxo (2015). The clear-sky RE during the fire season (August to September) is estimated to be -5.2 W m^{-2} , which is larger than our result (-2.1 W m^{-2}). This is consistent with the underestimation of our modeled AOD in South America when compared to the AERONET data (Figure 3)."

Line 241. Here and elsewhere. It is not clear whether the fires examined in this study include agricultural fires such as those in Equatorial Asia and South America.

→Reply: Yes, the agricultural fire is included. We made it clear in the revised manuscript.

Lines 276-on. The text should state whether the modeled AOD includes aerosol from all sources, not just fires.

→Reply: The modeled AOD includes aerosol from all sources. We made it clear in the revised manuscript.

Line 311. The text states, "Although MAM4 increases the column burdens of POM and BC by up to 40% in many remote regions compared to MAM3...." Why does this large increase occur?

→Reply: see our response above for the explanation of MAM4 and MAM3 simulated BC differences.

Line 338. Text should be more clear about how clouds amplify the forcing of BC.

→Reply: We added the following explanation in the revised manuscript: “When BC resides above clouds, its absorption of solar radiation is significantly enhanced due to the reflection of solar radiation by clouds [Abel *et al.*, 2005; Zhang *et al.*, 2015]”.

Line 343. Why is the forcing estimated from Terra different from that of Aqua?

→Reply: First of all, we notice that we had a wrong subtitle in Figure 7b and Figure 7c. Figure 7b should be for Aqua/MODIS, and Figure 7c should be for Terra/MODIS. The figure caption is accurate in the text.

Over southeastern Atlantic, smoke aerosols usually reside above the stratocumulus clouds. Therefore, the direct radiative forcing strongly depends on the underlying cloud fraction. If the cloud fraction is higher, for the same amount of smoke aerosols at exact the same altitude, smoke aerosols can exert stronger direct radiative forcing. Since stratocumulus clouds over this region exist the diurnal cycle, the forcing estimated from Terra (morning time, with larger amount of clouds) is different from the one estimated from Aqua (afternoon time, with smaller amount of cloud). For more detail, we recommend the reviewer to check Figure 3 in the reference:

Min M., and Zhang Z. (2014), On the influence of cloud fraction diurnal cycle and sub-grid cloud optical thickness variability on all-sky direct aerosol radiative forcing, *J. Quant. Spectros. Radiat. Transfer*, doi:10.1016/j.jqsrt.2014.03.014.

Line 346. There is no mention here or elsewhere about the effect of solar zenith angle on radiative forcing at high latitudes, particularly the Arctic.

→Reply: We agree with the reviewer that the cloud radiative forcing due to fire aerosols at high latitudes can be affected by the solar zenith angle (Shupe *et al.*, 2004). In the boreal summer, the lower solar zenith angle favors the larger DRE in the Arctic. We added this effect in the revised manuscript.

Line 349. Here and elsewhere, the authors should take care with the terms “summer” and “autumn” when referring to the Southern Hemisphere.

→Reply: Thanks. We made it clearer in the revised manuscript. All terms were changed to “boreal summer” or “boreal autumn”.

Line 354. “noises” Please fix English.

→Reply: Thanks. We changed to “..., and there is much less noise”.

Line 364. The text states: It is not clear why removal of POM in the simulation affects BC concentrations. If indeed this is what happens, then the Ghan method for calculating forcing should not be used for individual fire components.

→Reply: Because fire POM and fire BC are co-emitted and assumed to be internally

mixed. The burden of fire POM is about a few times larger than that of fire BC, especially in Arctic. With the removal of fire POM emission and thus fire POM in the NOFIREPOM experiment, fire BC will be impacted due to changed properties (e.g., size) of aerosol particles within which co-emitted fire BC is internally mixed with fire POM. Our results show that the fire BC burden in the Arctic is reduced in NOFIREPOM with the exact mechanism warranty of a detailed budget analysis. We added an explanation in the revised manuscript.

We would like to keep the Ghan method for calculating the radiative effects of individual fire components (POM and BC). The reason is that the Ghan method only introduces the relatively large bias for fire POM radiative effect (due to aerosol-radiation interactions), and the bias for fire BC radiative effect is small (comparing the Ghan and the BBFFBF methods). By using the two different methods we will be able to examine the uncertainty range of radiative effects of individual fire components. Also the Ghan method allows us to calculate the radiative effects of individual fire components due to aerosol-cloud interactions.

Line 379. See above comment.

Cloud radiative forcing section. Please see major criticism #2. Also, this section should provide discussion of why the forcing due to ACI is stronger in some regions compared to others.

→Reply: Please see our responses to the major criticism #2 above.

We added the following discussion of why the forcing due to ACI is stronger in some regions compared to others in the revision: “The different spatial distributions of fire aerosol radiative effect (RE) due to ACI in the NH high latitudes and in the tropics result from the difference in cloud distributions between the two regions. During the fire season the cloud LWP over the land areas in the NH high latitudes is three times larger than that over the ocean areas in the tropics. Larger cloud LWP favors the stronger RE due to ACI, because the larger LWP associated with the warm cloud and rain processes favors the aerosol effect on slowing down the autoconversion of cloud water to rain [Ghan et al., 2012; Jiang et al., 2015]. Meanwhile, in the Arctic, the low solar zenith angle in summer favors the large fire aerosol RE due to ACI.”

Line 411. The text should state why larger cloud liquid water path leads to stronger forcing due to ACI.

→Reply: We added the following explanation: “Larger cloud LWP favors the stronger RE due to ACI, because the larger LWP associated with the warm cloud and rain processes favors the aerosol effect on slowing down the autoconversion of cloud water to rain [Ghan et al., 2012; Jiang et al., 2015].”

Section on surface snow albedo forcing. Why are forcings due to BC deposition on sea ice not considered? The section seems misnamed, since forcings on all light colored

surfaces are seen in Figure 12.

→Reply: In our simulation, the sea surface temperature and sea ice is prescribed, and thus the radiative effect due to fire BC deposition on sea ice is not estimated.

We rename the title of the section to “Surface albedo effect”. The surface albedo change not only results from the radiative effect of fire BC deposition on snow albedo, but also from atmospheric feedbacks (e.g., snow depth change and snow melting) due to fire aerosols.

The forcings on surface albedo calculated with the Ghan 2013 method look suspiciously high over low latitudes (Figure 12). The authors should comment on these high values – e.g., $+0.5 \text{ Wm}^{-2}$ over parts of the U.S. south. Are these results comparable to those from SNICAR?

→Reply: The SAE of fire aerosols is also noticed over low latitudes, which includes the surface albedo changes from atmospheric feedbacks (e.g., snow depth change and snow melting) [Ghan, 2013]. These high values over low latitudes are not evident in those from SNICAR, which are diagnosed in the standard model simulation and don't include atmospheric feedbacks. We added a comment on these high values in the revised manuscript.

Figure 12b reveals no significant differences in forcings for the fire vs no-fire cases over the Arctic or north China. The authors should acknowledge this. Given the results from SNICAR, it seems that the only region that might show a significant impact of fire aerosols on surface albedo is Greenland and the very northern reaches of Canada.

→Reply: The annual mean fire BC forcing in the Arctic and North China ($\sim 0.01 \text{ W m}^{-2}$) is much smaller than that in Greenland and the very northern reaches of Canada. It is because the snow-covered time of Arctic and North China is shorter. The forcing in these two regions (Greenland and the very northern reaches) can reach up to 0.5 W m^{-2} . We acknowledged this in the revised manuscript.

Line 458. It sounds like snow melting is one of the rapid meteorological adjustments allowed to occur in the forcing calculation. Is this correct?

→Reply: Yes, the snow melting is allowed when calculating the surface albedo effect of fire aerosols.

Section on the fire aerosol effects on shortwave radiation, global temperature and precipitation. Here the statistical significance and the uncertainties of global results should be stated. If the global mean changes of some variables are not statistically significant, then that should be made clear.

→Reply: We added the significant information (e.g., one-standard deviations) in the text and in Table 2. The global mean changes not statistically significant are acknowledged in the revised manuscript.

Discussion section. Again the authors should stress the key points and put them in context of other new studies besides just Ward 2012 and Tosca 2013. What exactly is new in this study? Limitations and uncertainties of the study should be discussed – i.e., what are the shortcomings of the approach used here?

→Reply: We have included a discussion of the key points of this study as summarized as follows:

- a) Fire aerosol RE due to ARI in the Arctic regions ($0.428 \pm 0.028 \text{ W m}^{-2}$) is larger than that in the tropical regions ($0.172 \pm 0.017 \text{ W m}^{-2}$), although the fire aerosol burden is higher in the tropics. This results from the larger low cloud amount in the Arctic;
- b) The large cloud liquid water path over land areas, and low solar zenith angle of the Arctic favor the strong fire aerosol RE due to ACI (up to -15 W m^{-2}) during the Arctic summer;
- c) The global annual mean surface albedo effect (SAE) over land areas ($0.03 \pm 0.10 \text{ W m}^{-2}$) is relatively small and insignificant;
- d) The fire aerosols reduce the global mean surface air temperature (T_s) by $0.03 \pm 0.03 \text{ K}$ and precipitation by $0.01 \pm 0.002 \text{ mm day}^{-1}$. Especially, significant reductions of precipitation in southern Africa and in the NH high-latitudes are noticed.

Following the reviewer's comment, we added a discussion of limitations and uncertainties of this study:

- 1) The RE estimate of co-emitted fire POM with the Ghan (2013) approach is not accurate due to the assumption of internal mixing of individual fire components (POM and BC);
- 2) There is large noise associated with the surface albedo effects of fire aerosols with the Ghan (2013) approach due to the snow melting and atmospheric feedbacks;
- 3) There are uncertainties with the model simulation and configuration. For example, the model still underestimates observed AODs (mostly within a factor of 2) at the sites predominantly influenced by biomass burning aerosols during the fire season. It implies that the fire aerosol radiative effects can be stronger than those estimated in this study. In our simulation, the sea surface temperature and sea ice is prescribed, and the fire BC effects on sea ice is not considered. The brown carbon component of POM [Feng et al., 2013] is not considered in our current simulations, which may result in an underestimation of atmospheric absorption of fire aerosols."

Tables and Figures.

There are too many Figures. Decide what is important and put rest in a supplement.

→Reply: We moved the original Figure 2 (POM and BC burdens from different sources) and Figure 7 (fire aerosol radiative effect due to ARI at four seasons) to the supplement.

Captions should be stand-alone so that the browsing reader can understand what is being shown. Unusual acronyms should be explained.

→Reply: We added the standing-alone captions of all figure and tables at the end of the manuscript. We removed some unusual acronyms and added explanations for the others in the revised manuscript.

Units in Table 2 should be within the table, not in the caption.

→Reply: Done.

Uncertainty ranges should be included in Table 2, and significant changes shown in boldface.

→Reply: We revised Table 2 to include the uncertainty ranges and those significant changes are shown in boldface.

Text on all legends should be large enough to read. The latitude and longitude labels on the global maps can be eliminated for a cleaner, less cluttered appearance.

→Reply: We enlarged the text on legends of the figures. The duplicated latitude and longitude labels on the global maps were eliminated.

Global mean values should be reported to 2-3 significant digits.

→Reply: The global mean values were now reported to 3 significant digits.

Figures 4 and 5 should include error bars.

→Reply: Done.

Figure 7. What does white space represent?

→Reply: White space represents the missing values. As we mentioned in the figure caption, the radiative effect is estimated for above-cloud aerosols only. During the fire season, cloud fractions over the land, especially below 10°S, are extremely low, and close to 0. No above-cloud smoke aerosols were detected by satellites over these regions; therefore, no radiative effect due to above-cloud aerosols is estimated.

Figure 14. Replace acronyms above the panels with standard English terms.

→Reply: Done.