Review of “Impacts of global wildfire aerosols on direct radiative, cloud and surface-albedo forcings simulated with CAM5,” by Jiang et al.

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 Wm^{-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 Wm^{-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.

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.

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 The paper uses outdated terms to describe radiative forcing by aerosol, and does not adequately describe what adjustments to the model meteorology have been allowed in the forcing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 (AR1), 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.

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.

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.

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.

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."

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.

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.

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.

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

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?

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.

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.

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

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?

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

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

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.

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

Line 354. "noises" Please fix English.

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.

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.

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

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.

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 Wm^{-2} over parts of the U.S. south. Are these results comparable to those from SNICAR?

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.

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

Section on the fire aerosol effects on shortwave radiation, global temperature and precipitation. Here the statistically 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.

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?

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

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

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

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

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

Figures 4 and 5 should include error bars.

Figure 7. What does white space represent?

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

References.

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