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Interactive Comment

Interactive comment on "The sensitivity of global climate to the episodicity of fire aerosol emissions" *by* S. K. Clark et al.

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Thank you for your comments. We have addressed them here and have also made major revisions to the manuscript.

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Comment 1:

Dear Authors,

First, thank you for a concise, well-written manuscript on a topic of significance for modelers wishing to constrain the effects of aerosols on the global weather and climate. Understanding the mechanisms that drive atmospheric forcing and their potential susceptibility to external forcing is a complicated science that is essential for predicting and therefore responding to climate change. The episodic nature of wildfire emissions is an important consideration when calculating the forcing effects of aerosols (as well as the chemical effects of trace gases released by fires).

Unfortunately, I cannot recommend your paper for publication. The simplifying assumptions necessary for climate models, while greatly reduced from a decade ago, are still a serious cause for concern and a priority for improvement by the scientific community. Your paper is designed to address the uncertainties associated with one common assumption (smooth daily release of monthly estimated emissions), but really you are just replacing one arbitrary assumption with another (episodic release of emissions on arbitrary days). The differences in forcings you find between your simulations are much smaller than uncertainties in those forcings associated with your experiment's assumptions. I would contend that your "episodic" simulations are no more realistic than your "daily" simulations, in terms of interaction between aerosols and clouds. The vector from your "smooth" experiment to your "episodic" experiment may not point in the direction of the behavior of the real atmosphere. In light of this, I cannot recommend this paper be published.

The episodicity of burning emissions is not a random function, it relates to the interaction between fire and weather. Burning occurs on a limited set of days either because there is a limited set of days when conditions are suitable for fire propagation (wildfires e.g. (Flannigan and Harrington, 1988)), or because human decisions concentrate burning into a set of days with, among other conditions, the most suitable weather (anthropogenic burning e.g. (Reid et al., 2012)). Given that you are attempting to analyze the interaction between aerosol and clouds, capturing this weather interaction correctly is essential for accuracy. It has been demonstrated in the literature (Wang and Christopher, 2006) that time resolution of emissions even at the scale of hours has significant effects on downwind interactions with meteorology. A study of contextual biases in measurement-based aerosol forcing estimates confirms transport covariance

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of clouds and aerosols (Zhang and Reid, 2009); you can expect this to be true for the timing of aerosol sources as well. The interactions between the smoke sources and the weather patterns that determine the presence or absence of clouds are not random. By assuming that fire and meteorology do not interact, you simply replace one arbitrary assumption (smooth curve of daily emissions) with another (random episodic emissions). The forcing estimates you derive from this experiment are subject to errors much larger than the differences between your simulations. A more realistic simulation could give completely different magnitudes and even change the sign of the differences you attribute to fire episodicity.

Your approach to calculating the difference in forcing effects due to fire episodicity is novel as far as I know; but the problem of temporal resolution of emissions is not new, going back at least to (Heald et al., 2003) with continuing analysis by (Hyer et al., 2007b;Roy et al., 2007;Zhang and Kondragunta, 2008). You could have used realistic temporally resolved emissions: GFED v3 has global 3-hourly fire emissions inventories for your 2000-2006 study period (Mu et al., 2011). That paper also has a discussion of the episodicity of fire in different ecosystems that you could have used to construct your long-term climate effects test.

RESPONSE:

In your comments you have brought up many important points related to recent literature describing the episodicity of fires. We have made some major changes to the manuscript to address some of these comments and hopefully help readers understand the context within which we think our results should be viewed: as the results of a sensitivity study addressing the potential impacts of not resolving plausible day to day fire emissions episodicity in prognostic fire models.

We understand that there are some aspects of the cases we designed for this study that are not realistic. One, which you bring up, is that the emissions are released on particular days independent of meteorological conditions. While this is true, the cases

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are based on climatological fire emissions data from the GFEDv2 dataset and thus release the majority of emissions during the months where meteorological conditions are most likely to be favorable for fires. For that reason the pulses are not completely random.

While this is an approximation, without synchronizing reanalysis atmosphere forcings with observed fire emissions to our knowledge there currently is no better way of introducing greater fire episodicity rates into the model, unless we have a prognostic fire model within the model which can simulate the correct episodicity. Since our group is engaged in creating prognostic fire models, the question we want to answer here is how important are these episodic fires to climate, so how well do we have to capture them. We show in our revised manuscript that while the prognostic fire model (developed within our group) resolves the seasonal cycle of fire emissions adequately, it does not accurately simulate the episodicity of fires (Kloster et al., 2010). Rather than estimate current fire radiative forcings with a higher temporal resolution dataset (like GFEDv3), we seek to investigate the implications of fire aerosol emissions episodicity in a more general sense in the form of a sensitivity study.

It should be noted, which might not be clear to the reviewer, that in order to look at indirect effects as well as climate effects, we need to be simulating in a general circulation model, not using observed winds and thus the fires and the meteorology will never be in concert, unless we have a prognostic fire algorithm which works perfectly. We emphasize this more in the introduction.

In order to better explain our manuscript in the context of the reviewers view point, we have: a. added the references the reviewer mentioned, pointing out that there it is well known that fires are episodic, but no one has looked at the impact of this onto the indirect and climate forcing, b. shown that our prognostic fire model, which can capture many aspects of the fire climatology, is not able to capture the episodicity, showing that it is non-trivial to simulate in a prognostic fire model the episodicity.

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Comment 2:

1) Your first reference to aerosol indirect effects is this sentence: "Aerosols have both a direct effect on the radiation balance of the earth and a complicated indirect effect (Forster et al., 2007; Rosenfeld et al., 2008)." The IPCC report you cite has a clear discussion of the two aerosol indirect effects; the first indirect effect (cloud albedo or Twomey effect) (Twomey, 1977) and the second indirect effect (cloud lifetime effect or "Albrecht effect") (Albrecht, 1989). The Rosenfeld paper is about the second indirect effect. Your CAM5 simulation includes both effects according to your methods description, but your discussion refers at various points to both effects, and the two effects are not separated in your numerical results. These two effects have different error budgets and different climate implications, and must not be conflated.

RESPONSE:

We have made an effort to clarify these points in our manuscript with a more thorough and clear analysis of the aerosol/cloud effects in the model. Since aerosols and stratiform clouds are interactive in CAM5 we can be comprehensive in our assessment of the indirect effects and include the first and second indirect effects (effects on cloud lifetime, height and water content), as well as the semi-direct effect. Lohmann and Feichter (2005) have a nice summary of these separate but related effects (see their Table 1) and we cite their paper instead of Rosenfeld et al. (2008) as it is more appropriate for this topic. We have added discussion of the semi-direct effect to Pg 23700, Line 1.

The disadvantage of simulating all the aerosol/cloud interactions simultaneously is that it becomes difficult to isolate individual effects. Our previous discussion of the differences in the indirect effect between the daily and monthly emissions cases was confusing since it did not stress that we report the indirect effects as changes in total cloud forcing which include all the indirect effects mentioned above. We have added a sentence to Line 24 of Pg. 23699 to clarify this point: "The total indirect effect, which we report here, includes cloud albedo and cloud fraction changes but we find that global, annual average cloud fraction is similar between the daily and monthly cases."

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Comment 3:

2) On Page 10, you "speculate that on the days when the greatest above-cloud fire aerosol absorption occurs the clouds are actually more reflective than in the daily case, leading to proportionally increased warming from aerosols above clouds." This section is describing the outcomes of your model simulations: that is, it describes an atmosphere that exists entirely as 1s and 0s inside your computer. You should not need to speculate on its state or mechanisms: either the absorbing aerosols were over brighter clouds in the model or they were not.

RESPONSE:

We agree that this was poorly stated. The more detailed discussion of the daily changes in cloud/aerosol properties and new figure (Fig. 7) addresses this point without speculation.

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Comment 4:

3) Injection height: "Several studies have shown that a variable injection height for fire emissions has only a small impact on the distribution of fire aerosols in the atmosphere (Zhang et al., 2011; Tosca et al., 2011)." Both of these papers are about SE Asia, where moist atmospheres with strongly capped boundary layers have been demonstrated to keep injections low in all but the most extreme cases (e.g. (Wang et al., 2013;Campbell et al., 2013)). Your study is global in scope, and studies in other regions have found meaningful differences in aerosol lifetime and trace gas chemistry with injection height (e.g. (Hyer et al., 2007a;Leung et al., 2007;Freitas et al., 2006)). To my knowledge, there is not currently a reliable treatment for smoke injection height

in global atmospheric simulations, but it is a meaningful source of uncertainty.

RESPONSE:

The reviewer makes a good point, and we modify the text accordingly. Injection height of biomass burning aerosols does remain a source of uncertainty in modeling studies. Some studies, like those listed by the reviewer (Hyer et al., 2007a; Leung et al., 2007; Freitas et al., 2006) find that injection height has important impacts on aerosol transport, and others (not just based on SE Asia) that find that injection height is not significant (Chen et al., 2009; Zhang et al., 2011; Tosca et al., 2011; Lamarque et al., 2003; Colarco et al., 2004). Due to these conflicting results, for simplicity we choose to emit all emissions at the surface.

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Comment 5:

I do not think this paper is suitable for publication. The sensitivity identified by the experiment is overwhelmed by uncertainties and simplifying assumptions, to the point where it is impossible to draw even a tentative conclusion about the real atmosphere from these results. A more comprehensive review of the literature would have led to a better experimental design that might have yielded publishable results.

RESPONSE:

We have made an effort to consider all the references you provided as well as make the motivation of our study clearer. With these revisions taken into account, we hope you reconsider your stance. Regardless we thank you for your comments as they have helped improve the manuscript.

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