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

Anonymous Referee #1

Received and published: 9 December 2013

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

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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 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.

A few other comments:

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.

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

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clouds in the model or they were not.

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.

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.

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