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Atmospheric Chemistry and Physics
Discussions

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:

This paper addresses the question about the use of monthly mean fire emissions in studies investigating the effects of biomass burning aerosols on climate. Fires tend to be episodic. By implementing a steady, slowly varying source of primary aerosols from fires, climate models may not properly capture the nonlinear interactions between

aerosols and clouds that occur in the real world. The authors therefore want to test the application of more episodic emissions, and examine the consequences for radiative forcing and climate. They apply 4 kinds of emissions, all with the same 5-year total emissions: daily, once-per-month, once-per-year, and once-in-5-years. Daily emissions are the default in many climate models, calculated by distributing the monthly mean emissions into smoothly varying, daily increments. The authors find that application of daily biomass burning emissions may overestimate the net cooling effect of fire aerosols by about 1 Wm-2.

Main criticisms. 1. The paper does not go far enough to examine the model mechanisms that change when varying the episodicity of biomass burning emissions. For example, what explains the spatial pattern of the difference in the aerosol indirect effect when comparing the daily and the once-per-month cases (Figure 4)? What exactly causes the southward shift in the Intertropical Convergence Zone (ITCZ) in the monthly case? See detailed comments for pages 9-11.

RESPONSE:

We addressed these concerns with major revisions to the manuscript. Please see our responses to your further comments for more details.

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Comment 2:

2. The utility of two of the simulations is not clear. These are the simulations with the once-per-year fires and those with the once-in-5-years fires. Are these pulses realistic? Why is it instructive to examine such clearly unrealistic scenarios? This reader is puzzled.

RESPONSE:

Yes, at the resolution we run at in the model the once-per-year and once-in-five-year cases are both unrealistic in many fire regimes – although not for high latitude Northern

Hemisphere fires. The motivation for including them in our study was to see if there was a relationship between "degree of episodicity" and the calculated radiative forcings. In order to do this we included those two extreme cases. Our results do point to an increased all-sky direct effect RF with increased episodicity, but it also appears that even in the most unrealistic of cases, the clear-sky direct effect RF is roughly unchanged.

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Comment 3:

3. Eight-day average emissions from GFED have been available at least since 2010. A daily GFED emission inventory has also been derived using observations of active fires from the Geostationary Operational Environmental Satellite (GOES; Mu et al., 2011). It would have been useful to the community to test these emissions.

RESPONSE:

This is an important point, which Referee #1 also discusses in some detail. We have re-written the introduction and added a "Motivation" section to the manuscript to make the purpose of this study clearer. We compare the episodicity of the GFEDv3 daily emissions inventory (Mu et al., 2011) to daily output from a prognostic fire model (Kloster et al., 2010) run with reanalysis atmospheric forcing for the period 2003-2011. Since the fire model was not designed to capture the day-to-day variability of fire emissions it does not resolve the episodicity of fires seen in observations. This suggests that this prognostic fire model and its derivative models, used to estimate the future impacts of fires, underrepresent the true episodicity of fire emissions.

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Comment 4:

Detailed criticisms. Page 2. Run-on sentence: "In the long term, we find that an increase . . ." Please be more clear about the net forcing effect of applying daily fire emissions.

RESPONSE:

Thank you for pointing this out. We have fixed the run on sentence in the abstract. It now reads "This suggests that the magnitude of the indirect effect calculated in studies using smoothed out fire aerosol emissions may be overestimated. In the long term, we find that an increase in aerosol emission episodicity leads to an asymmetric change in indirect radiative forcing in the Northern Hemisphere compared to the Southern Hemisphere. This change contributes to a slight shift in the annual average position of the intertropical convergence zone (ITCZ)."

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Comment 5:

It appears the authors wish to say that applying daily emissions may overestimate the net cooling effects of biomass burning aerosols by 1 Wm-2. If this is correct, please say so.

RESPONSE:

Yes, in the conclusion we do state "This result suggests that the magnitude of the indirect effect of fire aerosols reported by previous studies (Collins et al., 2011; Ward et al., 2012)" may be overestimated." We added a sentence to the abstract that makes a similar point: "This suggests that the magnitude of the indirect effect calculated in studies using smoothed out fire aerosol emissions may be overestimated."

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Comment 6:

Be more quantitative about results regarding change in ITCZ and precipitation rates.

RESPONSE:

We added a sentence describing the percent improvements (and areas where the monthly case does worse) in model prediction of precipitation rates to the abstract.

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Comment 7:

Page 3. Provide more examples of climate studies calculating the radiative forcing from fires. Please supply more quantitative information about the observed episodicity of fires. Is there a spatial dependence to the episodicity? Be more quantitative in describing the Chen results.

RESPONSE:

We added some examples of earlier studies that calculate some radiative forcings from fires. The most comprehensive are Bowman et al. (2009) and Ward et al. (2012), however some others that have calculated just the aerosol direct effect (Naik et al., 2007) or calculated the direct effect from one particular fire (Randerson et al., 2006) were included as examples in the introduction.

Some quantitative information about fire return intervals was added to the manuscript (van der Werf et al., 2010). In addition the Motivation section includes a discussion of what percent of total emissions occur in the top 5% of days in the fire model and in GFEDv3 (Mu et al., 2011) observations.

When describing the Chen results we now state that in some locations far from the source regions the burden of BC was increased by 10-20% (Chen et al., 2009).

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Comment 8:

Page 7. State location of gridbox in Central Africa for which the emissions are shown in Figure 1.

RESPONSE:

The gridbox is in western Tanzania (30 degrees lon, -6.63 degrees lat). The figure caption now states this.

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Comment 9:

Page 8. There is no reference to Figure 3. Also please provide a table of net forcing for the 4 cases.

RESPONSE:

A reference to Figure 3 was added in the text and rather than create a table for the net forcing in each of the four cases, we list the numbers in the caption of Figure 3 (now Figure 5) as well as in the text.

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Comment 10:

Page 9. Provide global mean annually averaged cloud droplet number, not just the percent difference between two cases. Typo: “diminises” Explain why the monthly case results in large reductions in the indirect effects off the west coasts of South America and Central Africa and over Siberia.

RESPONSE:

Thank you for pointing out the typo; it is now fixed.

To explain the differences in the indirect effect between the monthly and daily emissions cases we examined global average quantities averaged for each day of the month. We looked at the daily timeseries of cloud droplet number concentration and cloud fraction. The results are given in a new figure (Figure 7) and described with new and edited text in paragraph #2 of section 3.1. The new Figure 7 shows cloud droplet number but not the cloud fraction since the cloud fraction time series were similar for both cases, as stated in the new text. The global, annual mean cloud droplet number for the daily and monthly cases are given in the new figure. The regions where we see the biggest differences between cases are the regions where fire aerosols impact cloud forcing the most given the daily emissions scheme (Ward et al., 2012).

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Comment 11:

Page 10. Please diagnose the model response to the indirect effect in the monthly case. Also provide more information about previous studies looking at the climate effects of the vertical structure of clouds and black carbon. What are the uncertainties of this effect? See Koch and Del Genio (2010) and later papers.

RESPONSE:

We now discuss the semi-direct effects and the associated uncertainties in the following added text (Pg 23700, Line 1): "Absorbing aerosols can have additional radiative effects by modifying vertical temperature profiles in the vicinity of clouds. Changes in atmospheric stability and evaporation rates often associated with BC and collectively known as semi-direct effects (Lohmann and Feichter, 2005), can alter cloud lifetime, fraction and height. Sakaeda et al. (2011) showed that for fire emissions of BC the semi-direct effects could be similar in magnitude to the direct effect. However, the magnitude and even the sign of the forcing are still highly uncertain and depend on cloud regime and the height of the affected layer (Koch and Del Genio, 2010). Semi-direct effects are often categorized as an indirect effect, as we do here in our RF discussion, since they impact radiative transfer by changes to cloud cover (Lohmann and Feichter, 2005)."

Note that we discuss the response of the model precipitation to the aerosol effects in the monthly case in section 3.2. Unfortunately we cannot isolate the global climate response (temperature, precipitation, etc.) to either the direct or indirect effects since

both effects occur simultaneously and globally in the model and are not independent of one another.

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Comment 12:

Does Figure 5 show the same information as Figure 4 but this time as differences between the daily case and the other cases?

RESPONSE:

This is correct, assuming this comment is referring to Figures 5 and 6 instead of 4 and 5. We decided that Figure 5 was redundant since this information was presented more clearly in Figure 6. We have deleted Figure 5.

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Comment 13:

Please convince the reader that the largest differences in the aerosol indirect effect occur where "substantial fire emissions and sensitive cloud regimes coincide." Consider plotting the zonally averaged fire emissions or cloud sensitivity. In any event, how exactly is cloud sensitivity defined? Why are the clouds in some regions more sensitive than others?

RESPONSE:

This was a mainly qualitative assessment that we made based on the results of a previous study and is, in retrospect, not very helpful in that it confuses this statement with the discussion of cloud susceptibility from the previous page (revised in response to previous reviewer comment). Therefore we deleted this sentence and refer to Figure 6 (now Figure 5) in the previous sentence.

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Comment 14:

Page 11. What exactly causes the southward shift of the ITCZ in the monthly case? What is the time period of the observed precipitation rates of Xie and Arkin (1997)? The reader assumes that these rates are from the 1990s, but the GFED emissions are from 1997 to 2006. Why not compare precipitation rates during years contemporary with the GFED emissions? How is improvement defined? What does a negative percent improvement mean? What explains the pattern of "improvement" seen in Figure 9?

RESPONSE:

The change we see in our study is actually a northward shift (Ming and Ramaswamy, 2011 observed a southward shift). Based on similar reasoning presented in their manuscript, we see an asymmetric positive radiative forcing in the northern hemisphere. The ITCZ shifts northward to compensate (analogous to how the ITCZ shifts during the seasons - in NH summer it shifts north and in the NH winter it shifts south). We add this text to the manuscript to make this more clear. A number of other studies have demonstrated similar, analogous effects from aerosols (Biasutti et al., 2006; Hoerling et al., 2006; Yoshioka et al., 2007; Ming and Ramaswamy, 2011). Discussion of these studies is now included in the manuscript in the "Climate Impacts" section.

The time period used for observed precipitation rates of Xie and Arkin (1997) was 1979-1999. We have included the years in the manuscript now. The citation for this dataset is the manuscript from 1997 even though it contains data through 2002 (overall it contains observations for the years 1979-2002). See http://www.cgd.ucar.edu/cas/catalog/surface/precip/arkin.html for more information.

We do not use observations that exactly coincide with the years of GFED emissions for two reasons. The first is that we base our cases on climatological monthly averages of the GFED emissions between 1997 and 2006. Since the cases only reflect the climatology and not the exact emissions rates during each year between 1997 and 2006, we think it is reasonable to compare the average precipitation rates calculated

in the model to climatological average observations around the same time period. The second reason is that the comparison here concerns average quantities in the long term runs, run for 30 years without any reanalysis atmosphere forcings. Therefore the results do not reflect the model response during any particular set of recent years, rather just the typical response of the present day atmosphere to changes in episodicity of fire aerosol emissions.

For the precipitation plot, positive improvement means the absolute value of the difference between observed precipitation rate and model predicted precipitation rate was less in the monthly case than in the daily. Negative indicates that it was greater. The pattern of improvement in Figure 9 is likely influenced by the change in circulation (Biasutti et al., 2006; Hoerling et al., 2006; Yoshioka et al., 2007; Ming and Ramaswamy, 2011).

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Comment 15:

Tables. Please define acronyms in footnote or caption to the tables.

RESPONSE:

In the captions of Tables 1 and 2 the acronyms are now defined.

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Comment 16:

Table 2. The usual practice is to put statistically significant results in bold. Please provide units for lifetime.

RESPONSE:

Thank you for bringing this up. It has been changed now and units have been provided (days).

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Comment 17:

Figures. For the difference plots, please make clear in the captions what difference is being shown.

RESPONSE:

This has been fixed in each of the captions in the difference plots.

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Comment 18:

Figure 1. Say where the gridbox is.

RESPONSE:

The gridbox is in Western Tanzania (30 degrees lon, -6.63 degrees lat). The figure caption now states this.

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Comment 19:

Figure 2. "In each case" should be "in all cases."

RESPONSE:

Yes, this has now been changed.

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Comment 20:

Figure 5. Over what time period are the forcings calculated? Figure 6. Consider making multi-panel plots – e.g., combine Figures 5 and 6.

RESPONSE:

We determined Figure 5 was redundant. The forcings were calculated over the five year period 2001-2006 (we now indicate this more explicitly in the caption).

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Comment 21:

Figure 7. What do white areas signify?

RESPONSE:

The white areas represent locations where the difference was not statistically significant. This is now included in the figure caption.

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Comment 22:

Figure 8. Explain both in the caption and the text what percent improvement means and what years of observations were examined.

RESPONSE:

A description of what improvement means was added to the figure caption: "Positive improvement means the absolute value of the difference between observed precipitation rate and model predicted precipitation rate was less in the monthly case than in the daily. Negative indicates that it was greater."

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