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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The changing radiative forcing of fires: global model estimates for past, present and future" *by* D. S. Ward et al.

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Response to the comments of Anonymous Reviewer #2:

Thank you for your comments and suggested edits. We addressed them all here with modifications to the text and several of the figures. Our responses are given here in the plain text preceded by "RESPONSE".

Major comments

Comment - 1. The paper is very long and presents a challenge to the reader due to the description of experiments jumping back and forth between the main results text and Appendices. Can the methodologies for each part of the analysis be presented first in a "Methods" section together with the description of model experiments? The details could still be included in the Appendices, but at least the description of what was done to calculate metrics would all be in one place, rather than distributed throughout the results. The "Results" section could then just present the different forcings, discussing rel tive importance and what is controlling them. This would also allow the results to be presented more concisely.

RESPONSE - We implemented these suggested changes and are very happy with the results. The details of the methodology for CO2, CH4, N2O and the land albedo changes are still in the appendices but all method-related material that was in the results section has been moved to the methods section. This vastly improves the readability of the manuscript.

Comment - 2. Much reference is made to the Kloster et al., (2012) study. The main findings of this study and how it relates to the present work are not presented adequately. I suggest the inclusion of a paragraph or two in the Introduction spelling this out.

RESPONSE - Great comment, this was also mentioned by the first reviewer. We moved some details about the Kloster et al. (2010; 2012) work from the methods to the introduction and added the following paragraph (7th paragraph in Sect. 1 – introduction):

"Kloster et al. (2010; 2012) modeled fire emissions from a pre-industrial base state through the year 2100 accounting for the impacts of changes in CO2 concentrations, climate (after 1948), and human activities, on fire area burned. The 20th century saw a small (less than 15%) decreasing trend in global fire emissions, mainly due to changes in land use and human fire ignition and suppression (Kloster et al., 2010). Global fire emissions increased between 17% and 62% from the present day to the future (2075-2099) in their model projections. They found that while projected climate changes led to increased global fire emissions, these could be offset in part by future changes in human population and land use. Here we will build on these studies by evaluating how past and future changes in fire activity predicted by Kloster et al. (2010; 2012) will

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impact the climate."

Comment - 3. Description of radiative forcing. This needs to be spelled out early, especially since the authors use a definition which the reader may not assume. Generally (following the IPCC) the community uses the term radiative forcing to describe the change in radiation balance of the atmosphere due to changes in a forcing agent over some time period, usually pre-industrial (1750) to present day. Here, the authors use the term to refer to a change in radiative balance of the atmosphere between a system that includes and does not include fires. Generally, this might be termed "radiative effect" to avoid confusion, since it does not comply with the standard radiative forcing definition. If the authors wish to use the term "radiative forcing", their definition should be presented earlier in the paper. This detail is particularly important, since the analysis includes both pre-industrial and present-day simulations, but the radiative forcings presented do not refer to changes between these.

RESPONSE - Unfortunately, the AR4 used several definitions for radiative forcing, one which is presented by the reviewer, but others in other chapters (see the paleoclimate chapter for a definition similar to this one). We use the more standard definition of radiative forcing for an agent (e.g. http://amsglossary.allenpress.com/glossary/search?id=radiative-forcing1). We added a paragraph in the introduction (8th paragraph in revised text) that reads:

"We use the concept of radiative forcing (RF) as a measure of climate impacts with an aim toward evaluating the relative importance of each of the various fire/climate forcings. RF is often defined as a perturbation to the net radiative flux at the top of the atmosphere or the tropopause relative to the pre-industrial state (Ramaswamy et al., 2001). Here we are calculating the radiative flux perturbations of fire emissions and other impacts relative to a global state without fires, but for the same time period. This could be better named the radiative forcing of the direct effects of a particular process. For simplicity, we will use the term RF to representing the radiative forcing of the direct effect of fires and will refer to differences in the present day or future RF relative to the

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pre-industrial state as changes in the RF."

This should cover how we're defining RF for the rest of the paper and clear up the understandable confusion. We also went through the rest of the paper and found instances where the previous version had not followed this definition of RF as we spell it out here in the new text. These instances were changed to be consistent with this definition.

Comment - 4. I would argue that a valuable part of the paper is the regional comparison of the CLM-derived area burned estimates to the observationally-based GFED2 estimates. However, spatial differences are not explicitly shown and only briefly described for a couple of regions.

RESPONSE - This comparison has been done in large part by Kloster et al. (2010). Since we are also using some of the model results from that study we use this figure (Figure 3) and brief description to point out areas where the model and the GFED differ that will be important later on in the paper. We added an extra reference to that study at the end of the section (now section 2.3.2), "For a full spatial comparison of the model results to GFEDv2, see Kloster et al. (2010)" to direct readers to that work if they are interested.

Comment - 5. It is assumed that analysis of means from 5-year simulations are adequate to account for internal variability in simulations where atmospheric composition changes are allowed to interact with the model radiation scheme (Section 2.2.2). This is on the short side of what would usually be deemed acceptable in this type of experiment, where around 10 years might be considered adequate. It is stated that mean surface temperatures between the simulations are less than 0.05 K, however possible regional differences or differences in circulation are not discussed. Are the authors happy that the differences shown are truly characteristic of the mean states of each simulation, and not compounded by inter-annual variability.

RESPONSE - Usually studies of radiative forcing are quite short: 1-2 years (while stud-

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ies of climate impacts, e.g. temperature response, are longer – 10 to 30 years). We chose to run the CAM5 simulations for five years since this is long enough to smooth out the interannual variability in the indirect effects due to the changes in clouds, but short enough that the global temperature remains stable and won't cause changes in the net TOA radiative flux. Since we analyzed radiative forcings and not climate responses, we aimed to avoid any trends in TOA radiative flux that could not be explained by direct interaction with aerosols or direct interaction with the aerosol impacts on clouds. Similar simulation durations were used in previous studies of this kind, some that were cited in this manuscript: Wang et al., 2011a (34 months); Quass et al., 2009 (5 years); Lohmann et al., 2007 ACP v7 pp 3425-3446 (5 years).

It is true that regional differences in T are likely to be less than and greater than the mean. But since we are interested in global RFs in this study it is the global T that is most important. It was only mentioned to show the similarity between the model climate integrations regardless of emissions used. We decided that this statement would probably cause more confusion than clarification to readers so we replaced it with a reference to the above cited studies to act as precedents.

Comment - 6. Chemistry and aerosol effects of fires. Not enough information is given on assumptions that were made in the CHEM and AERO simulations. For example, what is assumed regarding isoprene emissions between the pre-industrial and present-day simulations? Are the effects of changes in land cover, CO2 and temperature on biogenic emissions included? The isoprene (& monoterpene) emissions used will be critical in determining both the tropospheric oxidizing capacity and pre-existing aerosol (particularly in pre-industrial), which are highly relevant to some of the main conclusions of the paper.

RESPONSE - We are grateful to the reviewer for pointing this out as this exposed an error in our model setup that impacts the O3/CH4 RF analysis in a meaningful way. Some biogenic emissions that were included for present day and future simulations (CO and methanol) were mistakenly excluded from the pre-industrial simulations. We

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apologize for the error and assure the reviewer that we have since corrected this. Furthermore, the emissions that were included were identical to the present day emissions (although not by mistake) and therefore did not account for any changes due to the factors mentioned by the reviewer.

To correct this we created a new set of biogenic emissions for the pre-industrial time period (1845-1855) by applying the Model of Emissions of Gases and Aerosols from Nature (MEGAN) to our leaf area index (LAI) results from the CLM runs. We put together biogenic emissions in this way for the present day as well, compared them to the ACCMIP emissions data, and found that CLM over-estimates LAI in the present day, particularly in the tropics. Therefore we scaled the LAI to match current estimates for isoprene emissions the present day, and applied this scaling to both the present and pre-industrial before computing the emissions. In this way the biogenic emissions are a better estimate for the pre-industrial emissions. The following text was added to section 2.2:

"The ACCMIP inventory contains emissions of NHMCs, NO, NH3, SO2, and primary OC and BC aerosols. Biogenic emissions of isoprene, monoterpenes, CO and methanol are computed with the Model of Emissions of Gases and Aerosols from Nature (MEGAN) (Guenther et al., 2006) using present-day land-use and CO2 conditions. We create pre-industrial biogenic emissions by applying MEGAN to year 1850 leaf area index (LAI) predicted by CLM3, and CO2 concentrations. The CLM3 LAI is scaled so that the predicted year 2000 isoprene emissions match present day global estimates from Heald et al. (2008)."

Then the pre-industrial CHEM simulations were re-run in their entirety. The RFs and other figures for O3, CH4, and OH were re-computed and the results included in the manuscript. The combination of more era-appropriate issues and the inclusion of biogenic CO/methanol did not change the major points of this section of the paper – that pre-industrial O3 production efficiency is greater than in the present day - but it did make the difference less dramatic. The biggest difference was on the change in pre-

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industrial OH concentrations due to fires, which was near zero. Thus, fires had very little impact on CH4 through changing tropospheric oxidants.

It also occurs to us that fires will impact global LAI to some extent. This would then change the emissions of biogenic gases on a more permanent basis. We did not account for this in our original simulations or in these updated simulations. We added a sentence to Sect. 2.2 to make the reader aware that they do not include changes due to fires (which could have an impact on oxidizing capacity).

Comment - 7. Finally, the conclusions section and abstract need to better describe the main findings and the key quantitative information – many effects are described without reference back to quantitative results. E.g. in Abstract: "greenhouse gas forcings were smaller in magnitude."

RESPONSE - This was also mentioned by the first reviewer. We removed the qualitative descriptions of the forcings in the abstract and took out many of the references to specific forcings as well since these are probably superfluous in an abstract. The major findings, a quantification of the fire RF and the fact that this has changed dramatically over time, are stated in the last sentence of the abstract.

Further description of the major findings are now included in the conclusion section. We took some of the quantitative info that was in the conclusions (the descriptions of the final two figures) and put them into a new section titled "Summary of radiative forcings". In the conclusions section we added the following text to the end of the first paragraph to better summarize the main results:

"In our study fires have an overall negative radiative forcing, or cooling influence, for all time periods. The magnitude of the cooling decreases between 1850 and 2000, in large part because of the masking of fire aerosols impacts on clouds by anthropogenic aerosols. Between years 2000 and 2100, global emissions from fires depend primarily on the applied climate forcing (Kloster et al., 2012). However, the RF imposed by fires in 2100 was similar for both emission projections used in this study, despite the

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range in total emissions between them. The greater RF of CO_2 from fires in the case with ECHAM atmospheric forcing (0.91 W m-2 compared to 0.75 W m-2 with CCSM atmospheric forcing) was compensated by a more negative aerosol indirect effect RF (-1.74 W m-2 compared to -1.42 W m-2 with CCSM atmospheric forcing). Overall, we project that year 2100 fires will apply a stronger cooling forcing on the climate (0.85 W m-2) when compared to year 2000 fires (0.55 W m-2). These RFs are reported as global averages."

Comment - Page 10538, line 26: "different than" –> "different from"

RESPONSE - Corrected.

Comment - Page 10553, line 8: "timescale of primary, or longest-lived, mode" What is this timescale? What do the modes refer to?

RESPONSE - Added the text "The major atmospheric sink for CH_4 is reaction with OH, part of the 'primary natural mode' which describes time-scale of interactions between CH_4, CO and OH (Wild and Prather, 2000). This mode accounts for the long-term radiative effects (12 to 15 years) of a perturbation to the tropospheric chemical system (Wild et al., 2001)" to this section to help describe the mode idea and also provide another reference for it.

Comment - Page 10555, line 9: "O_3 from fires are not" –> "O_3 from fires is not"

RESPONSE - Corrected.

Comment - Page 10555, line 25: ""The results shown here suggest that the background chemistry modifies the fire emissions in producing the total O_3 change." This sentence makes little sense and does not convey what the authors intend. The background chemistry is not modifying the actual fire emissions. Please re-write with more clarity.

RESPONSE - We decided to delete this sentence in its entirety since the point is made in the next sentence and the deleted sentence was very unclear.

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Comment - Page 10556, line 17: “Fires are the largest source of carbonaceous aerosols in the CAM5 simulations . . .” Give come numbers / fractions of total.

RESPONSE - Added “accounting for 80 to 95% of OC and BC emissions”. Exact numbers are included in Table 4 to which reference is made.

Comment - Page 10561, line 19: “..RF shows the strong seasonality of the forcing.” What is this string seasonality? The description reads as if it has already been described or is common knowledge. Figure 10 does not really demonstrate what one would term a strong seasonality. It appears to show a downward trend over the two years if anything (if indeed it is the inset figure which is being referred to).

RESPONSE - We removed this sentence in its entirety. Differences in the forcing by season, mainly the difference between summer and winter forcing, are talked about with more specifics later on in the paragraph.

Comment - Section 3.9. The authors fail to mention the possible effect of diffuse radiation from fire- emitted aerosol on photosynthesis, which may be an additional aerosol indirect effect on biogeochemistry.

RESPONSE - We added “Aerosols modify temperature, leading to a response in C uptake by the land and ocean (Mahowald, 2011). They also increase the ratio of diffuse to direct radiation reaching the surface, enhancing C uptake by vegetation (Mercado et al., 2009), and affect vegetation by redistribution of precipitation.”

Comment - Page 10565, line 19: “The decreases in fire-induced RF by CO2 and O3 from 1850 to 2000 are notable in that they may have been unexpected (Fig. 13).” Why might they have been unexpected? Why does Fig. 13 imply that they may have been unexpected?

RESPONSE - This sentence was edited to remove the qualifying “unexpected” expression.

Comment - Throughout: “preindustrial” –> “pre-industrial”

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RESPONSE - Corrected.

Comment - Throughout: “earth” –> “Earth”

RESPONSE - Corrected.

Comment - Figure 1: While I appreciate the idea of including a schematic of the main effects considered in the study, it could be improved. E.g. could include how ozone and methane are affected by fire (i.e. ozone not directly emitted, some of methane effect is through OH perturbation). Also, there are additional climatic fire drivers? i.e. Temperature, humidity.

RESPONSE - Major edits were done to this figure including the addition of the mentioned climate drivers of fire variability (and vegetation species composition), also OH is shown as a connection between methane and ozone. We think this figure is much more readable in it’s improved form.

Comment - Figure 2: Is this taken from Kloster (2012), or is it plotted from data from Kloster (2012). This is not clear. Caption: “color” –> “colour”.

RESPONSE - The figure was created from data from Kloster et al. (2012). To clear this up, the caption text was edited to include, “Timeseries of the total C lost due to fires plotted from the output of the Kloster et al. (2012) Community Land Model (CLM) simulations”.

Comment - Figures 7 and 9: The use of the grey-scale colours to denote the ‘control’ simulation and overlaying the change using a different set of colours is messy. It is not possible to see the control values in regios where there are large changes plotted. Would it be better to keep the colours for the change values, but overlay line contours for the control scenario values?

RESPONSE - We removed the greyscale background shading in Figure 7 to make this figure easier to understand and avoid the “over-plotting” of data that the reviewer mentioned. Contours were difficult to see and we decided that the total cloud forcing information was not necessary to understand the message of the figure, so we left it out.

Figure 9 was removed altogether since, as was noted by another reviewer, this particular forcing (the aerosol deposition onto snow/ice) was so minor that a descriptive figure was probably unnecessary.

An additional revision was made to the land albedo changes analysis. Since the Kloster et al. (2010) fire model does not allow the crop PFTs to burn, we decided to redo the land surface albedo analysis without any changes to crop albedo due to fires. For the same reasons we removed crop area from the fire area burned pie charts in Figure 3. The change in the analysis led to small, less than 0.03 W m-2 changes in the albedo RFs and did not affect any of our conclusions.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 10535, 2012.