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Atmospheric Chemistry and Physics Discussions

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 #1:

Thank you for your comments. We have addressed them here with major modifications to the manuscript text and Tables/Figures as outlined below in the plain text responses preceded by "RESPONSE".

Comment - 1. The paper reads like a first draft. It is difficult to understand the set-up of the model experiment, what assumptions were made, and what the important points are. It is not clear about what conditions were applied to each scenario. Methods of calculating the forcing are scattered between the methods and results sections and the appendix. Background material is also scattered. Much textbook pedagogy is inserted

in the paper. Most paragraphs contain 3 or more awkward or overly wordy sentences. There is an overreliance on obscure acronyms. Many results are not quantified in the text. Figures are often not fully described. For those variables which generate small forcings, the text could be condensed to 1 or at most 2 paragraphs. The paper could be half the length it is now.

RESPONSE - We made an effort to address all these issues by making the specific corrections suggested below and by reducing the wordiness of the sentences. In the end we were able to reduce the word count of the text by about 15%. The text is heavy on methodology since there were so many different types of analysis used here. This made it difficult to cut the text down because we felt it was important to include enough detail that the experiments could be repeated, even for those forcings that were small in this case. With the changes to the organization of the paper as suggested by both reviewers, the text should read better and might seem additionally shorter.

Comment - 2. Tables, figures, and captions also require extensive revision, with the exception of Table 3. The browsing reader should be able to discern most of the story of the paper from just these sections. The returning reader should be able to pull out important points from these sections without having to burrow into the text to retrieve details. Again obscure acronyms make understanding the tables and figures challenging to understand. Table footnotes would be helpful, and better captions and labels.

RESPONSE - After applying the reviewer's specific comments for the tables and figures they are much improved and these concerns were addressed with the revisions. With the addition of footnotes and edits to the captions, the figures and tables should now be understandable outside of the context of the main manuscript text.

Comment - 3. The paper apparently builds on Kloster et al. (2012), "The impacts of climate, land use, and demography on fires during the 21st century simulated by CLM-CN." This is not made clear. A summary of the results of this paper should be presented in the introduction, as well as a summary of the trend in fires from the preindustrial to the present-day. A description of the radiative effects of the trends in fires calculated by Kloster et al. (2012) would then proceed more smoothly and succinctly.

RESPONSE - This is a great comment as it does help to provide a better starting point for the paper. 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 preindustrial 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 impact the climate."

Comment - 4. The section on "aerosol indirect effects on biogeochemistry" is unclear and should be cut. In this section, the authors first describe several potential biogeochemical effects and then dismiss them as unimportant. They then introduce a biogeochemical feedback that apparently affects the carbon cycle, but is incompletely described. What the authors call the climate-BGC feedback should not be included as a forcing unless the authors can convince the reader of the mechanisms that contribute to this forcing.

RESPONSE - We agree that the description of this feedback effect was inadequate. The following text has been added/edited to clear up the confusion:

"Finally the climate impacts of fires can themselves impact the carbon cycle (e.g. Jones

et al., 2001; Mahowald, 2011; Mahowald et al., 2011a; Mahowald et al., 2011b). 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. These impacts are not well understood (Friedlingstein et al., 2006; Friedlingstein and Prentice, 2010; Mahowald et al., 2011a), however estimates from coupled-carbon-climate models suggest a roughly linear response with climate forcing of between 0 and 40 ppm of CO2 for a 1.4 W m-2 RF (Mahowald et al., 2011a)."

Comment - 5. The authors need to do more work to convince the reader what is new. For example on page 10551, the authors state, "Since fires are a relatively novel addition to current global models it is important to stress that many models could be missing this large forcing if they do not prescribe CO2 concentrations." Are the authors saying that carbon cycle models have historically not included wildfires? My understanding is that in fact they have. Wouldn't the effect of fires be already folded into the calculated forcings of such models? This discussion of novelty should be in the introduction and the conclusion sections.

RESPONSE - The sentence on novelty was meant to refer to Earth system models for which interactive fires are new (this model is the only coupled model in the IPCC AR5 which has fire, although it is an old version of this fire algorithm), but the reviewer's point that the forcings should be present in the models even if fires are not explicitly modeled is well taken. This sentence was removed.

As to what's new in this work, we added, "This work represents, to our knowledge, the first comprehensive model assessment of the radiative impacts of fires on a global scale and its change in time" to the last paragraph of the introduction. Other text in the introduction explains the knowledge deficit in this area and points to studies that mention the same:

"Recent studies have begun to quantify various aspects of fires' effects on climate. These studies have focused on specific impacts (e.g. Liu et al., 2005; Naik et al., 2007; Ito et al., 2007), certain types of fire emissions (e.g. Jacobson, 2004), or on fires that occur within a particular ecosystem or region (e.g. Randerson et al., 2006; Pfister et al., 2008; Stone et al., 2011). More general assessments highlight the complexity of these impacts, particularly those from aerosols, and the difficulty in performing a comprehensive analysis at a global scale (Forster et al., 2007; Bowman et al., 2009). As such, the sum radiative effect of fires on a global scale remains fundamentally uncertain (Carslaw et al., 2010)."

Comment - 1. All forcings should be identified as a forcing of one condition relative to another. It is often not clear if a stated forcing was calculated relative to the preindustrial atmosphere or to the no-fires case.

RESPONSE - We defined our usage of the term RF more clearly and moved the definition to the introduction of the paper to make it more visible: "We use the concept of radiative forcing (RF) as a measure of climate impacts and their relative importance. 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. 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 represent the radiative forcing of the direct effect of fires and will refer to differences in the RF relative to the pre-industrial state as changes in the RF."

We then made minor changes throughout the paper to be sure we complied with this definition.

Comment - 2. It is not clear what the authors mean by fires. Wildfires? Fires set to clear land? What is the definition of "deforestation fires"? Both the no-fires and fires cases include land use change, but it isn't clear if the no-fires case included forcings

from the fires used to clear land for agriculture. There should be a separate paragraph near the beginning of the paper explaining these points.

RESPONSE - We've addressed these questions in the revised and expanded paragraphs explaining the fire model in the introduction:

"...Emissions from fires are predicted in CLM3 with the coupled carbon-fire model implemented by Kloster et al. (2010) building on work from Arora and Boer (2005). The model combines fuel availability, fuel moisture content and ignition probabilities with the model wind to predict the area burned from open fires. Kloster et al. (2010) introduced an anthropogenic impact on fire ignition and suppression based on population. Emissions from deforestation fires, fires intentionally set to clear land for agriculture, are represented as a fraction of the C lost due to land-use change."

We also included an introduction to fires as "open fires" in the second paragraph of the paper, although the literature on this topic has simply referred them as "fires" without further definition. The term biomass burning is not used here and in some other papers because this could include emissions from enclosed fires for energy and heat.

Finally, we re-emphasize that the fire emissions used in this study contain deforestation fires (as defined in the above paragraph from the manuscript) in the first sentence of Sect. 2.2.2:

"Monthly fire emissions are computed from the CLM3 total C lost due to fire, including C lost due to deforestation fires that are associated with land use change."

Comment - Abstract. The abstract is too long. All forcings referred to should be quantified. What is the "negative change" in RF in lines 15-16? What does "small" forcing mean? (I.e., less that what?)

RESPONSE - These are all good comments. The qualitative language was removed, as were most of the references to specific forcings since these are probably not necessary to mention in the abstract. Where specific forcings remain in they were quantified.

These edits reduced the length of the abstract by 5 lines in Word (23 lines to 18 lines).

Comment - Methods. The authors should begin this section with an overview of the model set-up, the basic assumptions, the models used, etc. As is, the methods section is confusing. There are disconnects in the set-up that are not well explained. For example, it appears that future climate drives fire emissions but not the "atmosphere simulations" (page 10540). What are atmosphere simulations and why do they use year 2000 climate? The authors state that land and ice albedos remain constant, but elsewhere calculate the albedo difference between fires and no-fires.

RESPONSE - Following these recommendations we made major changes to the methods section. This section is now ordered differently. The model description and setup come first, then the computation of the emissions, the application of the emissions in the CAM simulations, next the CLM "no-fire" simulations, and finally background and methods for computing the RFs were moved to the end of this section. Some details of the RF computations are still included as appendices but this reorganization will make more sense to the reader, as mentioned by this and the second reviewer as well.

This text in the third paragraph of section 2.1 was edited/added to address the question of why year 2000 climate was used:

"All simulations are branched from a two-year spinup integration of year 2000 climate conditions (temperature, solar forcing, sea surface temperature, etc.). The model setup after spinup is identical for all simulations except that trace gas and aerosol emissions, both fire and non-fire, and initial CH4 concentrations (that affect tropospheric chemistry) are case specific. In this way the impact of the change in emissions is isolated from other climate factors."

We note in this same paragraph that "the impact of fire on surface albedo is not predicted by CAM but is computed offline", to answer the second question.

Comment - Methods. A paragraph explaining what triggers fire in the land model is

needed. Have the model fires been validated, and if so, what was the result? Also, how are GFED emissions derived?

RESPONSE - The fire model is described in great detail by Arora and Boer (2005) and Kloster et al. (2010), to which references are made in the introduction of this manuscript. We also added an improved description of the Kloster et al. (2010) paper including the following summary sentences of the fire model:

"Emissions from fires are predicted in CLM3 with the coupled carbon-fire model implemented by Kloster et al. (2010) building on work from Arora and Boer (2005). The model combines fuel availability, fuel moisture content and ignition probabilities with the model wind to predict the area burned from open fires. Kloster et al. (2010) introduced an anthropogenic impact on fire ignition and suppression based on population."

Validation of the fire model 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 Figure 3 and a 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 2.2.2), "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.

The following sentence was added to the first paragraph of Sect. 2.2.2 to address the GFED comment: "The GFEDv2 emissions are derived from satellite estimates of area burned (van der Werf et al., 2006a) and biomass distributions predicted by a biogeochemical model (van der Werf et al., 2006b)."

Comment - Page 10541. What does atmospheric forcing mean? Why is population density needed for the calculations?

RESPONSE - Atmospheric forcing refers to the information the land model receives from its boundary with the atmosphere but that are not predicted by the model.

We added the text: "The land model was forced with temperature, humidity, wind, air pressure, precipitation, and solar radiation data from the Qian et al. (2006) NCEP/NCAR reanalysis. This atmospheric forcing data for 1948 to 1972 was repeatedly cycled. . ." to the second sentence of Section 2.1.

And also added: "Population density is needed for the model prediction of human-caused fire ignition and fire suppression." This is located in the last paragraph of Section 2.1 just after the transient population density is mentioned.

Comment - Page 10542. The acronym FIRE_CLOSS is unnecessary. Replace this and other obscure acronyms with plain English.

RESPONSE - Corrected for FIRE_CLOSS, FIRE_NLOSS, and TOA_FRAC. We also used "SNF" for Net TOA shortwave flux instead of "FSNT" which is a model variable name and not very descriptive.

Comment - Page 10543. What are the important points of Figure 3b? Just that GFED is different?

RESPONSE - That's correct, it points out that the GFED is different and the latitudes where it is different.

Comment - Page 10544. What quantity was found to be 45% in the van der Werf (2006) paper?

RESPONSE - The quantity was the "C content", the sentence was changed to: "The resulting biome-dependent C contents are comparable to the constant C content value of 45% used by van der Werf et al. (2006b)."

Comment - Page 10545. "All fire emissions were released into the lowest model level." What model?

RESPONSE - This sentence now references CAM as the model the emissions are released into.

Comment - Page 10546. Acronyms for simulation names are challenging to under-

stand.

RESPONSE - We included an explanation of why the simulation names were chosen in the footnotes to Table 1. The names include references to the simulation group (AERO or CHEM), the year of the emissions used, and the set of fire emissions used. These are the three distinguishing factors between the simulations.

Comment - Page 10547. It's not clear what changes in the different chemistry simulations. The same climate from year 2000 was used in all cases. Did anthropogenic or biogenic emissions change? If so, what emissions were applied?

RESPONSE - Hopefully this is also made more clear by the footnotes to Table 1 but also in the text the following was added to Sect. 2.1:

"The model setup after spinup is identical for all simulations except that trace gas and aerosol emissions and initial state (both fire and non-fire) are case specific."

"The online chemistry is not interactive with the model radiation and, as a result, the model climate is identical in all CHEM simulations despite the differences in trace gas emissions."

Comment - Page 10548. While the global temperature effect of fire aerosols was small (0.05 oC), regional effects could be large.

RESPONSE - 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 reference to studies that used similar run times (3-10 years) to act as precedents.

Comment - Page 10548. "We assume that deforestation proceeds similarly whether fire is available as a vegetation-clearing tool or not." Please clarify and explain earlier in the paper. This sounds like an important assumption.

RESPONSE - This statement is confusing as we had written it. The point it was attempting to emphasize was given in the previous sentence, that land-use change was the same regardless of whether fires were included or not. Therefore we removed this sentence and refer to our revisions of the introduction to help clarify what deforestation fires are and how the model predicts them.

Comment - Page 10550. "The American Meteorological Society defines. . ." An example of textbook pedagogy. The authors should strive to make their paper more succinct, about half the length that it is now.

RESPONSE - This sentence was removed from the manuscript.

Comment - Results. This section needs to be shortened. All background information should have already been presented by now in the paper. If some of these results have appeared in Kloster et al. (2012), they can be briefly summarized here. For those variables which generate small forcings, the text could be shrunk to 1 or at most 2 paragraphs, with references to other papers. All forcings should be specified relative to a base state (no fires or preindustrial). Most of the changes described should be quantified. For example, in the sentence, "While the C storage . . . increases rapidly toward the year 2100," how rapid is "rapidly"? Also, the outstanding features of each Figure should be described in the text.

RESPONSE - Much of the material that was in the results section was moved to methods, and the text for N2O and snow/ice albedo forcings were reduced to a couple sentences each. We added more quantification to described trends, including to the example given by the reviewer, and addressed the other comments by making the specific changes suggested below.

Comment - Page 10552. "In the RCP 4.5 future scenario. . ." Paragraph is unclear and not sufficiently quantitative.

RESPONSE - The paragraph now reads: "Global land-use change is projected to decrease after the year 2000 (Fig. 4c) while atmospheric CO2 concentration and temperatures increase. With increasing temperatures, mobilization of soil N increases and plants are less N-limited, as also shown by Thornton et al. (2009). This, combined with greater atmospheric CO2, leads to enhanced gross primary production, and accumulation of terrestrial C, in all our CLM3 simulations from years 2000 to 2050 (Fig. 4b). However, C accumulates at a higher rate without fire activity. Fires impose consistent losses of C and N from vegetation (the fire simulation stores about 40% less C in vegetation in 2000), which suppresses gross primary production. This effect is especially evident in Fig. 4a in which the global terrestrial C storage stops increasing or decreases (depending on the atmospheric forcing used) when fires are included, but continues to increase without fires included."

This paragraph describes trends that are evident in Figure 4, but the reference to this figure was previously lacking. We added a couple more references to this figure as well as editing the paragraph for clarity.

Comment - Page 10552. "the RF from fire CO2 is . . . compared to the preindustrial RF. Radiative forcing is not typically calculated relative to another RF.

RESPONSE - 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 preindustrial 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 preindustrial state as changes in the RF."

Comment - Page 10552. What is the significance of the shaded area in Figure 4b? What accounts for the jumps in both the fires and no-fires scenarios in Figure 4c?

RESPONSE - As stated in the Figure 4 caption: "The shaded region in b) indicates when the change in carbon storage due to removing fires is less than the initial (year 1798) value."

The jump in C lost due to land-use change is simply a reflection of global land-use change patterns.

Comment - Page 10555. What is the message of Figure 5?

RESPONSE - Figure 5 was removed after we re-ran the preindustrial CHEM simulations with more era-appropriate biogenic emissions (in response to a comment from the second reviewer). The figure was no longer as illustrative and so we removed it.

Comment - Page 10556. What accounts for the decreases in AOD in Figure 6? Page 10559.

RESPONSE - We added the text, "Fire emissions were responsible for decreases in AOD particularly over the Sahara (Fig. 6). This was a result of reduced dust emissions caused by a dynamical response to the fire aerosols which we did not explore in detail."

Comment - Figure 8 should be more thoroughly explained.

RESPONSE - Some major edits were made here to better explain this figure and why we included it. First, we altered the figure somewhat. The arrow for 1850 fire emissions was given a zero slope in both panels. Since the arrow starts and ends for 2000 and 2100 are relative to 1850, it was difficult to explain why we chose a sloped representation of the 1850 aerosol indirect effects. This was getting into the different definitions of RF (relative to 1850, relative to no-fires) that caused a lot of confusion. The text

describing Fig. 8 was edited to better explain exactly what it shows:

"In a similar sense, fire emissions dampen the impact of increased anthropogenic aerosol emissions on cloud processes and the overall indirect aerosol effect. This is shown in Fig. 8, an illustration of the way fire aerosol impacts change with varying background conditions. The arrows show the difference in aerosol mass emitted (x direction) and the difference in total aerosol indirect effects (y direction) due to fire emissions, referenced to 1850 (instead of a "no-fire" world). For example, the year "2000" arrow starts at a y-value equal to TCFA_2000_CF minus TCFA_1850_CF, and ends at a y-value of TCFA_2000_NF minus TCFA_1850_NF (where the subscripts indicate the simulation used to compute the TCF). The slope of the arrows (Fig. 8b) shows the degree that fire aerosols mask the impact of anthropogenic emissions from 1850 to 2000 and to 2100. The masking is greatest for the year 2000 where the model estimate for indirect effects compared to 1850 would be -2.36 W m-2 without any fire emissions."

Comment - Conclusions. The authors should do more to convince the readers of what is new in their paper. They should also restate the simplifying assumptions made, and discuss the consequences of these assumptions for their results.

RESPONSE - The first paragraph of the conclusions begins, "Comprehensive assessment of the role of fires in climate is challenging because of the complex nature of the numerous fire/Earth system interactions (Fig. 1). While previous studies focus on specific fire effects, or on a subsample of processes (e.g. Naik et al., 2007; Bowman et al, 2009; Pechony and Shindell, 2010; Stone et al., 2011), here we calculate of the total impact of fire on the Earth's radiative budget. We use model-generated emissions inventories from time periods centered on 1850, 2000 and 2100 to examine how the RF of fires has changed since pre-industrial times, and how it may change in the future (Fig. 12, 13; Table 5)." This, to our minds, is a statement of what is new in this paper, i.e. the estimates for RF of fires for the different time periods.

We then added the following text to the third conclusions paragraph to better quantify the new results, “Fires, overall, have a negative radiative forcing, or cooling influence, in the model 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 range in total emissions between them. The greater RF of CO2 from fires in the case with ECHAM atmospheric forcing (0.91 W m-2 compared to 0.75 W m-2) was compensated by a more negative aerosol indirect effect RF (-1.74 W m-2 compared to -1.42 W m-2). 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).”

We took the quantitative descriptions of the last two RF figures from the conclusions and created the section “Summary of radiative forcings”. We restate our assumptions and their potential influence on the results as the final portion of this section since it is basically a discussions section. We think this should be a sufficiently visible place to restate the assumptions and it is most clear to make reference to specific forcings in this section rather than the conclusions section, reducing overlapping statements. We edited/added:

Sect 4, Paragraph 1: “Improvements in our understanding of aerosol effects on clouds could strongly influence our results for 1850 because of their prominent role in the total fire RF.”

Sect 4, Paragraph 2: “We introduce two main assumptions when computing the other major fire RF in all time periods, CO2. Fires shape ecosystems by altering population dynamics and species composition (Bond et al., 2005; Harrison et al., 2010; Rogers et al., 2011), affecting ecosystem productivity and decomposition rates. In CLM3, the PFT distribution does not react to fires. If a dynamic vegetation response to fires were included, we expect the CO2 RF would increase as forests would expand in the ab-

sence of fire and lead to even greater terrestrial C storage. Although, our other major assumption, that the atmospheric CO2 remains constant in CLM3 with or without fires, would tend to buffer the increase in C storage when fires are removed as diminished atmospheric CO2 would limit vegetation growth.”

Sect 4, Paragraph 8: “The premise of this modeling study, that atmospheric RFs can be derived from fire emissions computed offline, requires assuming that fire effects on climate do not immediately feedback onto fire activity. We circumvent these issues by using short simulation times and limiting our conclusions to global average radiative forcing. To understand the full climate response, fires would ideally be interactive with the atmosphere in future simulations.”

Comment - Appendices. I recommend that the descriptions of the forcing calculations be significantly reduced and included elsewhere in the paper. As is, there is much repetition.

RESPONSE - We agree that the appendices can be cut down significantly. Large sections of appendix A, and appendix B1, B4 and B5, were deleted. These were generally background information sections that did not bear directly on the RF calculation. Overall about 2.5 pages (in Word) were removed. In this way the repetition of already made points was eliminated as well. Details that would be required for someone to repeat our analyses, as well as major assumptions in the analyses, were kept in.

Although the point on relocating the forcing calculation descriptions is well taken, we decided to keep some details in the appendices. We think that this, in combination with the revisions already done to the manuscript, best addresses the major concern of both reviewers that the main text is too long and disorganized. The main descriptions of the forcing calculations were put into the methods section.

Comment - Tables. Tables should be stand-alone – that is, the reader should fully understand the different items in the table without referring to the text. Footnotes should help explain the tables. For example, in Table 1, what is “Data ocean model” and what does “Year 2000 climate” mean? All acronyms need to be spelled out in footnotes.

RESPONSE - These comments were addressed in all tables with the specific edits detailed below. Also, footnotes were added to all tables where acronyms needed to be defined and additional information was requested. All tables should now be understandable without the context of the main manuscript text.

Comment - Table 1. The entire set-up should be made clear in this table. What changes in the transient simulations? What is meant by emissions? Use footnotes.

RESPONSE - We divided Table 1 into two separate tables – one for the land model simulations and one for the atmosphere model simulations. Extensive footnotes were added so that the tables could be understood independent of the main manuscript text. The specific questions asked here are addressed in the footnotes.

Comment - Table 2. Percentages and concentrations should not be mixed in a column.

RESPONSE - The percentages were replaced with concentrations to match the rest of the values in the column.

Comment - Table A2. Region names should be spelled out. What are the scaling factors used for?

RESPONSE - Region names are defined in a footnote. The caption text was edited to include “the scaling factor applied to the fire emissions in the forward model simulations” in order to address the second part of the comment.

Comment - Figure 1. What does the person icon represent?

RESPONSE - Major edits were done on this figure, including defining the icons as “anthropogenic” and “climate-related”.

Comment - Figure 2. Bar plots should always start at zero so that the relative lengths of the bars have some meaning. Just use symbols, not bars. Acronyms are not explained and are inconsistent with simulation names.

RESPONSE - We decided that the bars were unnecessary and probably just confusing for the reasons you state here. We removed these and used a triangle symbol for the GFEDv2 value.

Comment - Figure 3. Acronyms need to be spelled out for this and all Figures.

RESPONSE - The legend descriptions in the caption were improved, including caption definitions, for this figure as well as for Figures 6, 7, 10, and 11.

Comment - Figure 4. Labels and legends are needed.

RESPONSE - Labels and legends were added for each panel.

Comment - Figures 6 and 7. Panel labels are needed.

RESPONSE - Panel labels were added.

Comment - Figure 8. This figure should be two panels instead of one with an inset. Both figures need axis labels, not just one. What do grey lines represent?

RESPONSE - The figure was made into two panels and the grey lines were defined in the caption: “the change in indirect effects is referenced against a preindustrial value of 0 W m-2 with the grey lines showing the path from time period to time period”.

Comment - Figure 9. Unnecessary, since forcings are minor.

RESPONSE - We agree, this figure was unnecessary and was removed.

Comment - Figure 10. Again, make two panels instead of one with an inset. Inset plot is hard to see.

RESPONSE - This was done and the legend was improved according to previous comments in this review.

Comment - Figure 11. Are these global forcings for each biome?

RESPONSE - The Figure 11 caption text was changed to read, “Global RF due to land surface albedo change. . .”

Comment - Figure 12. Can some uncertainty be placed on the indirect aerosol forcing? Also are these forcings relative to the no-fires scenario in the preindustrial?

RESPONSE - Uncertainty in model estimates of aerosol indirect effects on clouds are not clearly quantifiable. The IPCC AR4 combined the uncertainties in satellite estimates and the range in modeling estimates to give their 5% to 95% confidence intervals. We will keep the reference to the IPCC uncertainty in the text and note that CAM5 might be near the lower bound.

These forcings are relative to the no-fires scenario, we modified the caption to include, “plotted for preindustrial fires relative to the no-fire scenario”.

Comment - Figure 13. Authors should make clear that these forcings are calculated relative to the preindustrial simulation with fires (if that is indeed the case).

RESPONSE - The reviewer is correct about this. We altered the Figure 13 caption to include the text “Global, annual average RF for the various impacts of fire examined in this study relative to the preindustrial forcings (Fig. 12) for present day fires (1850-2000) and for year 2100 fires (1850-2100).”

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.