

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

Anonymous Referee #2

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This manuscript describes the results from a series of simulations using global atmospheric and land surface models with varying levels of complexity to assess the radiative impacts of global wildfires, due to their atmospheric emissions, land carbon storage, albedo and biogeochemical impacts. These effects are quantified for pre-industrial, present-day and future conditions. The authors attempt to describe a large set of model results and radiative calculations, and draw conclusions comparing their calculations with previous estimates and highlighting differences between radiative impacts during the different time periods. The paper contains a wealth of interesting results of interest to readers of ACP, and the authors should be commended for their extensive analysis; however the authors need to substantially improve the presentation of the results before I would recommend publication.

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The paper is very un-wieldy in its present form, and is not easy to read. This may partly be due to the large number of experiments described, but is exacerbated by reference to previous work which is not described adequately and many instances of poor written style. In many places the text appears rushed and is not always clear in what it is trying to convey. In addition, there are several aspects of the experiments which are not adequately described, and may leave the reader without enough information to put the results into context with their expectations from the experiments.

Major comments

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 relative importance and what is controlling them. This would also allow the results to be presented more concisely.
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.
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

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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.

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.

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.

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.

7. Finally, the conclusions section and abstract need to better describe the main find-

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ings 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.”

Specific / editorial comments

Page 10538, line 26: “different than” → “different from”

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

Page 10555, line 9: “O₃ from fires are not” → “O₃ from fires is not”

Page 10555, line 25: “The results shown here suggest that the background chemistry modifies the fire emissions in producing the total O₃ 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.

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

Page 10561, line 19: “..RF shows the strong seasonality of the forcing.” What is this strong 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).

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.

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?

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Throughout: “preindustrial” → “pre-industrial”

Throughout: “earth” → “Earth”

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.

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

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 regions 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?

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