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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of 2050 climate change on North American wildfire: consequences for ozone air quality" *by* X. Yue et al.

Anonymous Referee #1

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This paper describes the development of regressions that predict area burned in Wildland fires in Canada and Alaska. The authors use meteorological variables to drive these relationships for 13 ecosystems in northern North America. These relationships were then used to derive burned areas and further, emissions for current and future (mid-2000) conditions from an ensemble of 13 climate models. The resulting emissions were combined with emissions from the US (presented in prior work by the author) and used as inputs to chemical transport models that predict ozone concentrations.

Overall, this paper is well written. The material presented is appropriate for AC&P, and the results are relevant for those considering future air quality in North America (and

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beyond). The methods for development of the meteorology/area burned regressions are robust and I think extremely valuable. However, I do have some concerns about the use of the burned areas to develop emission estimates and how these were used to predict resulting air quality impacts. I don't think any of this is too major, but I would like to see these addressed before the paper is accepted.

General Comments:

The authors use emission factors from Andrea and Merlet (2001) to develop emission rates from the burned area estimates. These composite emission factors have been since updated (i.e., M.O. Andrea has available an updated list available to researchers, Akagi et al. (2011) has since published emission factors, Urbanski et al. has published emission factors for North America). Although I don't believe that the inclusion of more updated emission factors will not make a tremendous impact on the resulting model output, I think it is worthwhile to include the updates in this modeling.

I would have liked to have more details about the model simulations. Was plume rise included? What emissions (anthropogenic) were included in the simulations?

The authors model ozone concentrations with a global model (GEOS-chem) that includes a very coarse resolution (4x5 degrees). Further, the emissions input to the model are, I assume, included evenly across the month. While I agree that it is pretty much impossible to predict day to day fire variability in the modeling, I worry that this really dampens the impact on air quality. The authors include only one sentence about this uncertainty in the discussion of the manuscript (lines 796-799) and state that the model may underpredict pollution episodes (line 386). Therefore, I believe that the model results of MDA8 O3 don't have too much meaning.

The authors report summertime mean and also MDA8 O3 values. In the discussion section, it is not always clear which they are discussing.

Are modeled nighttime values included the monthly means, or is only daytime ozone

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concentrations considered? And how well does the model simulate nighttime and how does that impact the results.

Finally, do the model simulations include the feedbacks of the aerosols emitted from these fires? The aerosols emitted from fires will have important impacts on the photolysis, meteorology, and even biogenic emissions that can all impact the predicted ozone concentrations. And if not, is the magnitude of the changes in ozone described in this paper significant compared to the impact of these aerosol effects?

Other minor comments:

Section 2.2: Is there a minimum fire size reported in the FAMWEB and the Canadian National Fire Database?

Section 2.4: Was some of the burn area data withheld from the regression analysis and then used to check the robustness of the regression results?

Section 2.5: What is the horizontal resolution of the climate model outputs? Did these have to be scaled down? Line 257: Should be "We aggregate all of the climate simulations . . ."

Lines 321-323: The authors made a comparison as a check. How did it look?

Lines 338-340: Just to clarify, the month of a fire is assumed to be the month in which the start date occurs?

Lines 365-370: Why were more updated emission factors used in the simulations? (i.e., M.O. Andreae has an updated list from the 2001 paper; Akagi et al. (2011 and updates) are available, Urbanski 2014 is available, http://www.firelab.org/project/emission-factor-database). Although the changes aren't terribly large, there is a lot of updates to the emission factors available. Also, if NO contributes 30% of the fire-induced NOx, then why is the NOx emitted as NO? Shouldn't NO2 and other nitrogen species be included (especially at such a coarse horizontal model resolution). How were the VOCs speciated? What specific compounds were included in the emissions?

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Lines 379-392: The authors here discuss the ability of the model to represent ozone concentrations in the atmosphere. However, it is unclear if they are referring to hourly, daily or monthly concentrations. This should be made clear.

Line 400: The MEGAN v2.1 reference should be updated to Guenther et al., GMD, 2012

Lines 409-418: What is the temporal resolution of the fires? Are the monthly values emitted evenly throughout the month? Or were they assigned differing daily or diurnal emission rates?

Line 420: Future ozone will also be impacted by changes in anthropogenic emissions, too.

Lines 482 and 484: replace “which” with “that”

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 13867, 2015.