Response to Referee #1

We thank the referee for this helpful and comprehensive review, which has improved the manuscript. Detailed point-by-point responses to the reviewer comments are provided below. The reviewers' comments are shown in black with our responses marked as blue. The line numbers below refer to the revised manuscript to be submitted separately.

This paper describes the development of regressions that predict area burned in Wild-land 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 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.

→ The reviewer makes a good suggestion. We now compare fire emissions calculated with emission factors from Akagi et al. (2011) and Urbanski (2014) to those used in this study in a new Table S6 (listed at the end of this response). We performed two additional simulations with fire emissions calculated using emission factors from Akagi et al. (2011) (Table 1). We plotted a new Figure S3 (listed at the end of this response) showing the differences in the simulated ozone perturbations due to the discrepancies in emission factors. We quantified that simulations with emission factors from Akagi et al. (2011) project ozone increases of 5.5 ppbv in Alaska, 3.2 ppbv in Canada, and 0.9 ppbv in the western U.S. by future wildfire emissions. These enhancements are 14-23% higher than our previous estimates with emission factors from Andreae and Merlet (2001). In the revised paper, we have added the following explanations, analyses, and discussion.

In section 2.7:

"The emission factors from Andreae and Merlet (2001) have recently been updated by Akagi et al. (2011) and Urbanski (2014). As a check, we compare the predicted fire emissions using all three sets of emission factors (see Table S6 and related discussion in Section 3.3)." (Lines 383-386)

In section 2.8:
"Finally, we perform another two sets of simulations, one for present day (FULL_PD_EF) and the other for midcentury (FULL_A1B_EF), both of which use emission factors from Akagi et al. (2011), to estimate the modeling uncertainties due to emission factors." (Lines 457-460)
"We calculate the differences between FULL_PD_EF and FULL_PD to quantify the present-day uncertainties due to the emission factors, and the differences between FULL_A1B_EF and FULL_A1B to quantify these uncertainties at midcentury." (Lines 468-471)

In section 3.3:
"Estimates of fire emissions depend on emission factors. Using the same biomass burned calculated with observed area burned, we calculate three different sets of emissions using the factors from Andreae and Merlet (2001) (except for NO, see Table S3) Akagi et al. (2011), and Urbanski (2014) (Table S6). These emissions show similar magnitudes in CO and NH_3, but some differences in NO_x and non-methane organic compounds (NMOC). For example, NO_x from Akagi et al. (2011) is higher by 30-50% than that in Urbanski (2014) and in Table S3. Meanwhile, NMOC from Andreae and Merlet (2001) is lower by 20% than that in Akagi et al. (2011) and Urbanski (2014). In the following simulations and analyses, we use emission factors from Andreae and Merlet (2001) (except for NO from Table S3) and discuss the modeling uncertainties due to the application of different emission factors." (Lines 688-698)

In section 3.4:
"Our estimate of future fire impacts depends on the emission factors we adopted. Using emission factors from Akagi et al. (2011), we calculate larger fire-induced ozone enhancements at both present day and midcentury (Figure S3). As a result, simulations with emission factors from Akagi et al. (2011) project ozone increases of 5.5 ppbv in Alaska, 3.2 ppbv in Canada, and 0.9 ppbv in the western U.S. due to future wildfire emissions. These enhancements are 14-23% higher than our previous estimates with emission factors from Andreae and Merlet (2001) and Table S3." (Lines 737-744)

In section 4:
"First, the emission factors of ozone precursors are not well constrained, especially for NO_x. Sensitivity tests with emission factors from Akagi et al. (2011) show 14-23% higher fire-induced ozone than that with emission factors from Andreae and Merlet (2001) and the NO_x emission factor derived from an ensemble of experiments (Table S3). Using aircraft data from boreal fires, Alvarado et al. (2010) determined an emission factor of 1.1 g NO kg DM^{-1}, lower than our value of 1.6 g NO kg DM^{-1} and much lower than the estimate of 3.0 g NO kg DM^{-1} for extratropical forest fires in Andreae and Merlet (2001). Alvarado et al. (2010) found that 40% of wildfire NO_x is rapidly converted to PAN and

20% to HNO_3 and his estimate of 1.1 g NO kg DM^{-1} for fresh emissions includes these two species." (Lines 838-847)

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

→ We have clarified as follows:

"The GEOS-Chem model is not coupled with a plume model, and as a result cannot simulate the impacts of plume rise. As in Leung et al. (2007), we emit 20% of emissions in each grid square to the model levels between 3 and 5 km and leave the rest in the boundary layer, as observations have shown that over 80% of plumes from North America fires are located in the boundary layer (Val Martin et al., 2010)." (Lines 434-438)

"Anthropogenic emissions for ozone precursors, including NO_x, CO, and non-methane VOCs, are as described in Table 1a of Wu et al. (2008) and are summarized here for completeness and transparency. Global emissions of NO_x and CO are upscaled from the 1°×1° Emissions Database for Global Atmospheric Research (EDGAR) version 3 (Olivier and Berdowski, 2001). Anthropogenic VOC emissions are derived from the Global Emission Inventory Activity (GEIA) (Benkovitz et al., 1996). Over the North American domain, these global emissions are replaced with the EPA National Emissions Inventory (NEI) 2005 inventory (http://www.epa.gov/)." (Lines 405-412)

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.

→ We agree that the use of coarse spatial and temporal resolution increases the uncertainties in the prediction of ozone air quality. In the discussion session, we extend our discussion as follows:

"Second, we estimated fire-induced O_3 concentrations using monthly emissions, due to the limits in the temporal resolution of predicted area burned. Such an approach may have moderate impacts on the simulated O_3; Marlier et al. (2014) found <1 ppb differences in surface [O_3] over North America between simulations using daily and monthly fire emissions. The same study also predicted <10% differences in the accumulated exceedances for MDA8 O_3 globally. Third, the projections were performed at coarse spatial resolution of 4°×5°. As shown in Zhang et al. (2011), however, mean

MDA8 O_3 in a nested grid simulation (0.5°×0.667°) is only 1-2 ppbv higher than that at 2°×2.5° resolution in the GEOS-Chem model. Fiore et al. (2002) reached a similar conclusion in comparing simulations at 4°×5° and 2°×2.5°. They found that the coarse model resolution smoothed the regional maximum, resulting in a more conservative estimate of the intensity of pollution episodes." (Lines 868-879)

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

→ We have clarified that (section 3.4):
"Daily maximum 8-hour average (MDA8) surface ozone is a metric used by the U.S. Environmental Protection Agency (EPA) to diagnose ozone air quality. In this study, we use MDA8 ozone instead of daily mean ozone for all the analyses and discussion." (Lines 713-715)

Are modeled nighttime values included the monthly means, or is only daytime ozone concentrations considered? And how well does the model simulate nighttime and how does that impact the results.

→ We use MDA8 ozone instead of daily mean ozone for all the analyses and discussion. We focus on MDA8 ozone because it is a metric used by the U.S. Environmental Protection Agency (EPA) to diagnose ozone air quality. Both daytime and nighttime values are used in the calculation. MDA8 ozone typically occurs in daytime (Bloomer et al., 2010), when temperature is high, photolysis is rapid, and some natural (such as wildfires) and anthropogenic (such as vehicle) emissions are large. Challenges in simulating nighttime ozone would therefore have a negligible impact on our conclusions.

Evaluations of GEOS-Chem model have been performed extensively in previous studies. We have added the following sentences to the text:
"The simulated daily and monthly ozone concentrations from the GEOS-Chem model driven with meteorological reanalyses have been widely validated with site-level, aircraft, and satellite observations (Fiore et al., 2002; Wang et al., 2009; Alvarado et al., 2010; Zhang et al., 2011). Monthly mean ozone concentrations simulated with GISS meteorology have been evaluated by comparison with climatological ozonesonde data and reproduces values throughout the troposphere usually to within 10 ppbv (Wu et al., 2007). In addition, simulated daily ozone with GISS meteorology reasonably reproduces the summertime temporal variability of ozone concentrations as well as the pollution episodes in U.S. (Wu et al., 2008)." (Lines 396-404)

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?

→ GEOS-Chem includes the feedbacks of aerosol-induced light absorption on ozone phtotolysis, but not the feedbacks on meteorology or biogenic emissions. We now clarify in the text:
"In calculating photolysis rates within the plume, the model takes into account the attenuation of solar radiation by fire aerosols. This calculation has some importance; in their model study, Jiang et al. (2012) found that fire aerosols alone could reduce ozone concentrations by up to 15% close to the source due to the light extinction." (Lines 438-442)

Other minor comments:

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

→ Yes. We now clarify the size of fires in these databases:
For FAMWEB, "The minimum area burned is 1 ha and the maximum is 2.5×10^5 ha for the Inowak Fire, which began on June 25th, 1997." (Lines 160-162)
For NFDB, "The minimum area burned is 0.1 ha and the maximum is 6.2×10^5 ha for a fire that began on July 12th, 1981." (Lines 177-178)

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?

→ The reviewer makes a good suggestion. We now report the results of a cross-validation test:
"We cross validate all the regressions with the leave-one-out approach following Littell et al. (2009). We calculate the ratio of the predicted residual sum of squares (PRESS) root mean square error (RMSE) to the standard deviation (SD) of area burned in each ecoregion as an indicator of the leave-one-out prediction error. A robust regression usually has the RMSE/SD ratio lower than 2 (Littell et al., 2009)." (Lines 234-239)

"The leave-one-out cross validation shows RMSE/SD ratios between 0.53-1.1 in boreal ecoregions (Table 4), suggesting that the prediction error is usually smaller than the variability of data. In a comparable study, Littell et al. (2009) calculated cross-validated RMSE/SD ratios of 0.56-2.08 for area burned in western U.S. ecoregions during 1977-2003. Our prediction shows much lower RMSE/SD ratios, indicating that the derived regressions (Table 4) are reasonably robust for the future projections." (Lines 503-508)

Section 2.5: What is the horizontal resolution of the climate model outputs? Did these have to be scaled down?

→ The horizontal resolution of these climate models has been listed in Table S1. We did not interpolate these model outputs to the uniform grid squares. Instead, we calculate the averages in each ecoregion by aggregating all available grids in the same ecoregion. We perform such aggregation for output of each climate model independently. We reproduce below the original text.
"We aggregate all of the climate simulations into ecoregions for the projection."

Line 257: Should be "We aggregate all of the climate simulations . . ."

→ Corrected as suggested.

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

→ We have reported the results from this comparison. In the third paragraph of section 3.3 and Table 4, we compare the derived fuel consumption from the two different approaches:
"In a sensitivity test, we derive fuel consumption with regional DC thresholds based on ecoregion-specific probability distributions. This approach reduces western fuel consumption by 8-16%, but increases eastern values by 2-37% (Table 4). It also predicts lower Alaskan fuel consumption compared with other studies. The boreal biomass burned calculated with this alternative approach is about 156.2 Tg DM yr^{-1} for 1980-2009, almost identical to that estimated using a single probability distribution to define the DC thresholds (Figure 8a)." (Lines 669-676)

We have added a reference to the above results to clarify:
"As a check, we also compare the fuel consumption derived in this way with that calculated based on the ecoregion-specific DC thresholds (see Table 4 and related discussion in Section 3.3)." (Lines 327-329)

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

→ Yes, we have clarified as follows:
"Area burned is assigned to the start month, as end dates are often uncertain (Kasischke et al., 2011)."

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?

→ As we have explained in our response to the general comment, we have performed two additional sensitivity tests to quantify the uncertainties due to emission factors in the revised manuscript. For NO_x emissions, we use NO as a unit for the emission, similar to the treatment in previous studies (e.g., Andreae and Merlet, 2001; Akagi et al., 2011; Urbanski, 2014). Because NO and NO_2 are in rapid photochemical equilibrium, GEOS-Chem can calculate the equilibrium NO_x concentrations with the initial emissions of NO. For VOC emissions, we now explain that the following specific compounds were included in the simulation: CH_4, C_2H_6, C_3H_6, C_3H_8, C_4H_8, C_5H_{10}, HCHO, C_2H_4O, C_3H_6O, and C_4H_8O (Table S6).

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.

→ We have clarified in the text as follows:

"The simulated daily and monthly ozone concentrations from the GEOS-Chem model driven with meteorological reanalyses have been widely validated with site-level, aircraft, and satellite observations (Fiore et al., 2002; Wang et al., 2009; Alvarado et al., 2010; Zhang et al., 2011). Monthly mean ozone concentrations simulated with GISS meteorology have been evaluated by comparison with climatological ozonesonde data and reproduces values throughout the troposphere usually to within 10 ppbv (Wu et al., 2007). In addition, simulated daily ozone with GISS meteorology reasonably reproduces the summertime temporal variability of ozone concentrations as well as the pollution episodes in U.S. (Wu et al., 2008)." (Lines 396-404)

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

→ Corrected as suggested.

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?

→ We use monthly fire emissions because fire predictions on the daily scale are not available. The monthly values are distributed evenly throughout the month, without daily and diurnal variability.

"Second, we estimated fire-induced O_3 concentrations using monthly emissions, due to the limits in the temporal resolution of predicted area burned. Such an approach may

have moderate impacts on the simulated O_3; Marlier et al. (2014) found <1 ppb differences in surface [O_3] over North America between simulations using daily and monthly fire emissions. The same study also predicted <10% differences in the accumulated exceedances for MDA8 O_3 globally." (Lines 868-873)

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

→ Yes. The interactions between the anthropogenic and wildfire emissions have large impacts on the future ozone. We clarify as follows:
"Surface ozone concentrations in the 21st century will be influenced not just by trends in wildfire emissions, but also by changes in atmospheric transport, temperature, cloudiness, wet and dry deposition, and natural/anthropogenic emissions." (Lines 443-445)

However, for the model simulations, we kept anthropogenic emissions "constant at the level of the year 2000 for both present day and future simulations, to isolate the effects of changes in biomass burning emissions."

Lines 482 and 484: replace "which" with "that"

→ Corrected as suggested.

Reference

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Table S6. Comparison of wildfire emissions in North America for 1980-2009 derived with different sets of emission factors. All emissions shown here use the same biomass burned calculated with FAMWEB/NFDB area burned and FCCS/FBP fuel consumption.

Ref.	Domain	CO (Tg yr^{-1})	NO_x [a] (Tg yr^{-1})	CH_4 (Tg yr^{-1})	NMOC [b] (Tg yr^{-1})	NH_3 (Tg yr^{-1})	SO_2 (Tg yr^{-1})
Andreae and Merlet (2001)	NA	17.42	0.3	0.68	0.81	0.25	0.13
	Canada	11.02	0.17	0.44	0.54	0.15	0.09
	Alaska	4.25	0.09	0.16	0.17	0.07	0.03
	CONUS	2.15	0.04	0.08	0.1	0.03	0.02
Akagi et al. (2011)	NA	17.86	0.44	0.75	0.99	0.29	0.04
	Canada	11.46	0.19	0.51	0.63	0.21	0.01
	Alaska	4.2	0.2	0.15	0.24	0.05	0.02
	CONUS	2.21	0.06	0.09	0.12	0.03	0
Urbanski (2014)	NA	17.52	0.34	0.79	0.95	0.24	0.16
	Canada	11	0.18	0.5	0.6	0.13	0.1
	Alaska	4.36	0.12	0.19	0.23	0.08	0.04
	CONUS	2.17	0.04	0.1	0.12	0.02	0.02

[a] Nitrogen oxides as NO. The original emission factor of NO_x from Andreae and Merlet (2001) is replaced by the value of 1.6 g NO kg DM^{-1} based on the observations in Table S3.

[b] Non-methane organic compounds include C_2H_6, C_3H_6, C_3H_8, C_4H_8, C_5H_{10}, HCHO, C_2H_4O, C_3H_6O, and C_4H_8O

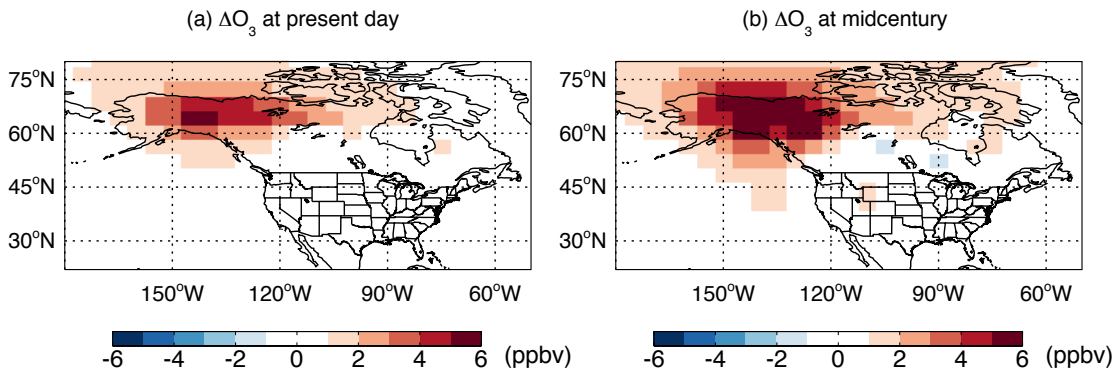

Figure S3. Differences of simulated JJA mean MDA8 O_3 concentration at (a) present day and (b) midcentury due to the differences in emission factors. Panel (a) shows the differences between FULL_PD_EF and FULL_PD. Panel (b) shows the differences between FULL_A1B_EF and FULL_A1B. Simulations FULL_PD_EF and FULL_A1B_EF use emission factors from Akagi et al. (2011). Simulations FULL_PD and FULL_A1B use emission factors from Andreae and Merlet (2001) and the NO_x emission factor derived from an ensemble of experiments (Table S3).