Response to Referee #2

We thank the referee for the 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.

The manuscript by Yue et al. examines the changes in burned area caused by forest fires in the mid-century over Alaska, Canada, and the US, using a regression-based method. Resulting effects on ozone air pollution are also investigated. For both burned area and for air quality, the effects are found to be strongest in Alaska and western Canada, but also substantial in the rest of Canada and the US.

The manuscript, which nicely builds on the authors' previous work focusing on the western US, is a very useful addition to the literature, as it is the first work to provide such future estimates using output from multiple climate models as meteorological input. It is well written, and the methodology is well described. I certainly find it suitable for publication in ACP, following some minor corrections that I suggest below.

GENERAL COMMENTS:

- My impression after reading the manuscript was that the authors downplayed the importance of pollution effects of fires in Alaska and western Canada. Aside from the (sparse) population in those regions that is exposed to fire-generated pollution, would more ozone not be harmful for the ecosystems of the region as well? If so, I would suggest that the authors discuss this in the Discussion and Conclusions section.

→ Yes. Ozone has large impacts on the health and carbon uptake of ecosystems. A recent study by Pacifico et al. (2015) showed that fire-induced ozone may decrease carbon uptake in Amazon forest by a magnitude comparable to the total carbon emissions from the same fires, suggesting doubled fire emissions by including the ozone vegetation damage. The lead author of the paper under review has also investigated ozone damage to carbon assimilation in U.S. (Yue and Unger, 2014). In the future, we plan to further explore the ecosystem responses to fire-induced ozone in North America.

We emphasize the importance of ecosystem responses to fire-induced ozone in the last two sentences of this study:
"The regional perturbation of summer ozone by future wildfires can be as high as 20 ppbv over boreal forests, suggesting large damage to the health and carbon assimilation of the ecosystems (Pacifico et al., 2015). Using a newly developed model of ozone vegetation damage (Yue and Unger, 2014), we plan to explore the response of boreal ecosystems to fire-induced ozone enhancements." (Lines 889-893)

- I feel slightly uneasy with the 1981-1999 period being referred to as "present day". I suggest that the authors explain why it is acceptable to use this term for a somewhat earlier period (which is centred at around 1990).

→ We clarify our choice to specify 1981-1999 as the present-day.

"We use the output from the 20C3M scenario for the prediction of area burned in the present day (1981-1999). Simulations in the CMIP3 ensemble for the years beyond 1999 (or in some cases 2000) are driven by a suite of future greenhouse gas scenarios, making comparison with observations difficult." (Lines 252-256)

SPECIFIC COMMENTS:

Page 13869, Lines 1-2: Please rephrase to avoid implying that these are the only important emissions from North American wildfires.

→ We have rephrased this sentence as: "North American wildfires are important sources of air pollutants, such as ozone precursors …".

Page 13869, Lines 14-20: These would fit better towards the end of the introduction section (though some of it is repeated anyway).

→ We have removed these sentences in the revised manuscript.

Page 13871, Line 3: Please add "in the scenario used" after "concentrations".

→ Added as suggested.

Page 13874, Line 23: Is "also" needed here?

→ We used "also" to indicate that site-level observations have been used in two ways. First, they were used to calculate monthly averages in ecoregions (as explained in the sentence before this line). Second, they were used as the input for the Canadian Fire Weather Index system.

Page 13875, Lines 14-16: I do not find it entirely clear how the 44 and 132 terms arise. Perhaps this paragraph could be more explanatory in that respect.

→ We now explain the number of predictors more clearly as follows:
"We calculate the means of five meteorological variables (mean and maximum temperature, relative humidity, precipitation, and 500 hPa geopotential height) over six different time intervals (winter, spring, summer, autumn, annual, and fire-season),

making 30 meteorological predictors in all. The mean and maximum values of the seven daily CFWIS indices during fire season are also included in the regressions, making another 14 fire-index predictors. As a result, a total of 44 terms is generated for the current year. As in Yue et al. (2013), we also employ all these variables from the previous two years in the regression, making 132 (44×3) potential terms for the regression." (Lines 220-228)

Page 13876, Lines 15-17: Is this scaling used for the future too? That should be clarified here.

→ Yes. We have clarified it as follows:
"In order to reduce model bias, we scale the aggregated variables of both present day and future from each GCM using the mean observations for 1980-2009 from the GSOD sites." (Lines 263-265)

Page 13877, Line 8: "US. FCCS" - There seems to be a typo here.

→ Yes. We have removed 'US.' to correct it.

Page 13878, Line 28: Please add "per unit area burned" after "consumption".

→ Added as suggested.

Page 13881, Line 22: It might be better to use "yr^{-1}" or "$year^{-1}$" instead of "a^{-1}", as it is more conventional.

→ We have replaced all the 'a^{-1}' to 'yr^{-1}' in the text, as well as that in the Figure 3.

Page 13882, Lines 3-4: Is the 20% of emissions released above the boundary layer occurring for specific meteorological conditions, or randomly? Please specify.

→ Plume height is driven by the fire dynamical heat flux (related to active fire area and sensible heat flux) and atmospheric conditions (such as stability). The current GEOS-Chem model does not include a plume model to simulate such impacts. As an alternative solution, "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 435-438)

Page 13882, Line 16: Please add "additionally" between "we" and "implement".

→ Added as suggested.

Figure 3: Please briefly remind the reader (in the caption) where the observations come from.

→ We have added the following information in the caption of Figure 3:
"Observations are compiled using fire reports from the Fire and Aviation Management Web Applications (FAMWEB) for Alaska and those from the Canadian National Fire Database (CNFD) for Canada."

Figure 4: Please add "meteorological" before "observations" in the caption.

→ Added as suggested.

Page 13885, Lines 2-5: I am not sure I understand – Table 2 suggests that 500 geopotential heights are used extensively in the regressions, but this sentence implies that they are not. What is the case?

→ Geopotential height anomalies have been selected as predictors in most of ecoregions, except for some areas in central and eastern Canada. We have clarified the text as follows:
"However, in some of the central and eastern Canadian ecoregions (e.g. Taiga Plain and Eastern Taiga Shield), such height anomalies are not selected as terms in our regressions (Table 2). Although geopotential height may still influence wildfire activity in those areas, this variable tends to correlate with fire weather indices or moisture variables. We attempt to avoid collinearity in our regressions, and so geopotential height may not be selected as a predictor there." (Lines 533-538)

Figure 5: Please add a parenthesis indicating "(midcentury/present-day)" or something similar above the bottom panel, for clarity.

→ Added as suggested.

Page 13886, Line 4: Yes, but please provide a reference for the "a common problem in GCMs" statement.

→ We have added the reference of Mearns et al. (1995) to support the statement.

Page 13887, Lines 19-20: Can you explain why there is this different behaviour between western and eastern parts of the region?

→ We have clarified the text:

"In the Western Taiga Shield, where area burned is projected as a function of the fire index ISI (positive relationship, Table 2) and relative humidity, the median area burned shows a small, insignificant decrease in the future atmosphere (Table 3, Figure 7b), because the increases of rainfall significantly reduce ISI there. In the Eastern Taiga Shield, where area burned is a function of the fire index DMC (negative relationship, Table 2) and relative humidity, the median area burned again shows an insignificant decrease by mid-century (Table 3, Figure 7b). DMC is related to both temperature and precipitation. Here rising temperatures enhance DMC and outweigh the effects of greater humidity (Table S4)." (Lines 609-617)

Page 13889, Line 16: Maybe the authors meant to write "overestimate" here?

→ Yes. We have corrected the error.

Page 13891, Line 8: Please change "results" to "result".

→ Corrected as suggested.

Page 13893, Line 15: Please add "for" before "all" and "we" before "calculate".

→ Added as suggested. The text reads:
"… where for almost all GCMs we calculate significant increases in area burned …"

Page 13894, Line 10: I would suggest explicitly stating whether the expected changes mentioned are increases or decreases (the latter, I presume). Also: Is it likely that dead vegetation may temporarily imply more flammable fuel?

→ As the reviewer suggested, mountain pine beetle (MPB) may decrease fuel load, but meanwhile increase fuel flammability by decreasing fuel moisture (Simard et al., 2011). It is unclear whether these effects have the net positive or negative impacts on wildfire emissions at the large domain. Since a certain conclusion is beyond the scope of this study, we revise the sentence as follows:

"In addition, mountain pine beetle outbreaks are important disturbances for both boreal and U.S. forests, leading to changes in fuel load and fuel moisture with climatic shifts (Fauria and Johnson, 2009; Simard et al., 2011; Jenkins et al., 2014). We did not consider these effects in this study." (Lines 820-824)

Page 13894, Line 22: Suggest changing "of" to "from".

→ Changed as suggested.

Page 13895, Line 8: Not every reader will be familiar with what the ΔO3/ΔCO ratio is useful for, so please add a sentence to explain (perhaps with a reference).

→ This ratio is that observed within the plume, with delta indicating the enhancement over background for ozone with respect to CO (emitted directly from the fire). This has been standard practice dating at least since Wofsy et al. (1992).

We have clarified the text as follows:
"In their review, Jaffe and Wigder (2012) reported that increased ozone is observed in most plumes, but with huge variability in the enhancement ratio of $\Delta O_3/\Delta CO$ within the plume." (Lines 852-854)

Page 13895, Lines 17-20: Yes, but larger scale effects could become stronger with more PAN being formed.

→ We now clarify our intent here:
"In any event, our use of a moderately high NOx emission factor and omission of rapid PAN formation within the plume may lead to an overestimate of fire-induced ozone in local areas (Alvarado et al., 2010)." (Lines 863-865)

Page 13895, Lines 20-22: The work of Marlier et al. (2014) suggests that at least the temporal resolution effect is minimal for ozone.

→ We thank the reviewer for this suggestion and have modified the 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)

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