Author's response to reviewer #2 comments on manuscript acp-2016-302

Reviewer #2 comments

I find this manuscript to be reasonably well written, though the amount of material it is attempting to cover is large, causing a general lack of focus. This manuscript attempts to cover a wide ranging of concepts, including biomass burning plume long range transport observations, boundary layer entrainment processes, air quality conditions over a relatively wide geographical area, air quality exceedances for several Canadian and US standards, biomass burning plume chemical analysis, and an assessment of the enhancements in several air quality measures – using an atmospheric model without biomass burning plume as the baseline. In some senses, this work ties these elements together knowing that this is what it might take to provide fuller understanding and predictive capabilities of the air quality effects of biomass burning events. However, given the amount of material covered and the issues inherent with organization of this material, I feel that it dilutes any overall scientific significance of this particular manuscript. From my perspective, the most informative and useful portion of the manuscript is well summarized in the first two paragraphs of the conclusions. Here, the authors focus on the geographic range impacted by a specific long range transported biomass burning plume, the observed air quality exceedances, observations of the plume chemical characteristics, and comparisons with a baseline model run in an attempt to assess the quantitative impact of the plume on air quality. I expand on these comments below with some suggestions to try and focus the manuscript some and make it more significant.

Author's response:

We thank the reviewer for his constructive suggestions on how to improve the organization and focus of the manuscript. We acknowledge the broad scope has been a challenge and will look to make additional structural improvement in response to your comments to improve the clarity of the manuscript. Responses to your Major comment are listed below.

The topic is timely and appropriate for ACP.
With appropriate attention paid to comments from reviewers, this manuscript should be published.

Major comments:

1.) This work appears to be a direct continuation of the Cottle et al. 2014 Atm Env paper on the same topic, but reprises a lot of what is already in Cottle et al.. For example, there are essentially three Figures that were taken, in part, from Cottle et al. and reproduced in this manuscript. Section 3.1 describes these figures to discuss the transport of the biomass burning across the Pacific. What is not detailed is what is new here on this topic. Perhaps the estimate of >6 days in transport? The authors note that they have done additional back trajectory studies, but it is not clear why. Perhaps due to greater geographical area under study? Do any of these issues make or break the interpretation/conclusions?

Author's response:

The atmospheric transport of the July 6-10 smoke event is covered in Cottle et al using HYSPLIT trajectories, NAAPS ground level smoke concentration forecast. Their HYSPLIT analysis of the event is well done and could be relied upon. The trajectory analysis was completed in support of our analysis of the event and presented in the supplemental material (Figure S1) for completeness. The transport aspect of our submission differ slightly from Cottle in that we include observations of MODIS AOD to describe the plume's transport and describe the associated meteorological scenario. These aspects may not support the interpretation/conclusion directly but do bring additional context to the event and provide perspective on the subsidence/entrainment influences noted in the study. Additional forward trajectories help show that nearby North American wildfire was not affecting our domain (larger than Cottle et al.).

It would make the manuscript more manageable if this portion of the manuscript were removed and the manuscript focused on the geographic range, air quality exceedances, chemical characteristics, and assessment of quantitative impact. For example, section 3.1 could be merged into the introduction in some manner, relying on Cottle et al. to describe the Siberian fires, the transport of plumes across the Pacific, and the general region in Canada/US of impact, in detail. Figure 2 could be removed or placed in SI as it duplicates a portion of a Figure from Cottle and focuses only on transport across the Pacific.

Author's response:

We will reframe the transport and meteorology aspects of the manuscript to leverage more from Cottle et al..

2.) The results and discussion section of the manuscript is organized more or less along geographic lines of specific air quality networks (i.e., Whistler, Lower Fraser Valley, etc.), but each section has different information discussed therein, based on the measurements available at a given site/network. The section 3.2 "July 6-10 smoke event overview" contains most, but not all of the discussion of the geographic extend and boundary layer entrainment. This organization leads to Figure 3 connecting satellite observations to time and geography, Figure 4 showing overview of the enhancements of air quality observations compared with modeled baseline conditions which are discussed in more detail later, Figure 5 showing entrainment conditions, more related to Fig. 3 than Fig. 4, all followed by sections on geographic locations and more details on the air quality modeling results. This puts the detailed discussions of plume chemistry and quantitative assessment of air quality parameter enhancements at the end of the figures and mixed in with geographic details. Furthermore, while the data was collected for specific networks of sites (i.e., Washington State in US, LFV, Whistler, and Interior), the discussion of the results suggest that the real impact was limited on coastal areas (i.e., parts of Washington State and LFV) and more significant on the in-land sites (interior and Whistler). Thus, the discussion/organization could focus more on the geographic effects rather than the specific network site locations.

Another potential organization might setup sections focusing on (a) plume impacts by geography (i.e., time and space), boundary layer entrainment, and air quality observations, (b) plume chemistry, and (c) quantitative assessment of enhancements.

Author's response:

Agreed. A joint coast to inland discussion would be an improvement. Your second suggestion may also work however illustrating the AQ observation early helps shows the exceptional AQ conditions associated with the event and helps support the discussion around baseline modeling.

3.) The most interesting aspect of this manuscript, from my perspective, is the quantitative assessment of the air quality impacts (i.e., enhancements) over baseline conditions as determined by an air quality model. This section, of all of the sections, provides insights into how well air quality models work and why they need to include biomass burning impacts and long range transport. This said, this aspect of the work is not discussed in great detail, nor does it represent a significant focus of the manuscript.

Author's response:

The air quality model did provide a method to account for the air quality at each monitoring location for changes from anthropogenic sources. An assessment of the model performance for non-event days was examined to establish the uncertainty values for the enhancement estimates. Alternatively, baseline conditions could have been estimated based on recent conditions (persistence), climatology, or another model (empirical) however were likely to introduce greater uncertainty in the estimated contributions of the wildfire smoke contributions given the stagnant meteorological conditions. The focus of the paper was on the particular event and the associated air quality impacts. The need for the inclusion of wildfire sources in the air quality model stems in part from the scale of the impacts. There could be a better link provided on how having wildfire smoke sources within the air quality model could lead to better air quality predictions. The scaled smoke event enhancement shows that a major source of pollutants for these air quality exceedences was missing. However, another justification made would be for improved predictability in the degree on entrainment of smoke plume over stagnant/stable airmass and other meteorological interactions.

Here are some examples and suggestions.

It might make sense to call out a sub-section on the AURAMS model under section 2.3.

Author's response:

Section 2.3 will be added to help describe AURAMS.

In describing the AURAMS, the authors write, "The reliability of the AURAMS baseline simulation was determined by examining the range of differences between observed and modelled values during non-event days (July 5 and from July 12-16). This range was used to estimate the uncertainty in LRT enhancement (observed – baseline) at each monitoring location." As with any analysis, understanding the null cases (i.e., here how well the model matches observations without smoke plumes) is very important. Where is this analysis described and presented? I do not find data presented on July 5th/12th. Where the uncertainty ranges in relative or absolute units? This portion could use more details.

Author's response:

The methodology was described in a very general way by the statement listed above however we recognized the lack of information on the July 5th and July 12-16 conditions limits the transparency of the manuscript. Additional analysis conducted on baseline case will be clarified for the next revision. Some of the non-event period was omitted from the analysis to account for other environmental factors (local smoke in the Interior, fireworks on July 4th/5th in Washington State) and are also listed in the supplemental information.

The uncertainty ranges in Table 4 are in absolute units (ppbv for ozone and ug/m^3 for particulate matter). These were calculated by adjusting the observed enhancements (obs – model) with the maximum and minimum bias observed during baseline period for each station. As part of our analysis on baseline conditions, we also calculated a number of other summary statics on the model bias, although it was decided that the maximum and minimum value would provide the most prudent enhancement estimates. These summary statistics are provided in the attached spreadsheet.

Has the AURAMS model been used for these types of analysis before? If so, it would be useful to reference previous work.

Author's response:

The utility of the AURAMS as a baseline model is supported by its use as an air quality research model (Stroud et al., 2008, Markovic et al., 2011, Stroud et al., 2015 [see below for full references]). CMAQ and AURAMS comparisons made in the Pacific Northwest, described by Makar et al, 2014 show comparable performance statistics. I am unaware of other smoke attribution studies that used chemical transport models (AURAMS or otherwise) as the baseline conditions. Typically, climatology or persistence assumption is used to develop the expected air quality condition in the absence of the event however uncertainty in the estimated baseline is not always examined in those cases. Another approach would be to use an empirical modeling approach to estimate conditions.

Question – does the AURAMS model have the capability to include biomass burning plumes? If so, then why was it not? If not, then it would be useful noting this issue, along with the significant differences in the observed and model O3 and PM2.5 in the LFV network at night. While the O3 issue is discussed, the obvious discrepancy in PM2.5 is not discussed. All of these issues add further insight into the issues with current (or at least this particular) air quality model.

Author's response:

At the time of study, AURAMS was available and it does not consider the impacts of biomass burning emissions. AURAMS was able to describe the enhancements to ozone and particulate matter examined in this study within a reasonable margin of uncertainty. The study findings could provide a good base analysis on which to compare against future modeling scenarios of the wildfire event as the reviewer has suggested.

The discrepancies between the PM2.5 observations and the baseline model increase in the July 8-10 period beyond our study's measure of the models variability and in a way that is consistent with the findings of Cottle et al. We can make more explicit mention on the PM2.5 deviation from baseline next revision.

The first case presented and discussed, comparing observations to modeled baselines, is the Whistler case shown in Fig. 6. The discussion and quantitative analysis comes from the time period of July 6-8th (WHI1/WHI2). However, it is apparent from Figure 6 that the PM and O2 are higher than the model baselines from July 6th through July 11th. What is going on during July 8th to 11th? If this is not biomass plume influenced observations, then there is an issue with the concept of model baselines. If it is continued influence of the biomass plume, why is it not apparently included in the analysis and discussions?

Author's response:

The discussion focused on the July 6-8th periods as this is where the majority of the Siberian wildfire enhancements occurred. Lesser enhancement may have also occurred on July 8-9. Known biomass burning smoke of local origin influenced the findings for July 10th (see Fig. S9). Baseline uncertainty estimates from non-event days excluded periods of known North American smoke influence.

The second case, LFV, is shown in Fig.'s 8 and 9. Cottle et al. Fig. 3 shows the lidar smoke plumes in the free troposphere (i.e., above boundary layer) at the CORALNet UBC site on July 6-8th, which becomes the focus of the description in the current manuscript in Fig. 8 with the LFV1/LFV2 boundaries called out. While Cottle notes the entrainment of aerosol into the BL that occurs in the days after this event, the current manuscript does not appear to make this clear direct connection between the lidar observations and the increased PM2.5 measurements (Fig. 8).

Author's response:

PM2.5 did rise after July 8th and we can make specific mention that Cottle indicate entrainment followed. Cottle et al described increase fine mode fraction and additional lidar signatures that support the entrainment of PM2.5 within the LFV. Our results confirm and quantify the timing, spatial distribution, and magnitude of those enhancements. The higher enhancement over the northern slope of the LFV suggests the entrainment process was aided by interactions between the smoke plume and the local mountains.

Minor comments:

1.) page 1 line 20, "The normalized enhancement ratios. . ."

2.) page 3 line 7 ". . . encompassing
large parts. . ."

3.) page 4 line 10+ Much of what is in this paragraph is also in Table 1. Suggest reducing this paragraph and using Table 1 for most of the details. Use this paragraph to discuss specifics, rather than just which instrument measured what.

4.) Figure 6 "(d)" label is missing.

5.) Table 1 is missing filter measurements at Whistler.

6.) Table 3: (a) superscript "a", "b", and "c" in backwards order. What do the "*" mean near central interior station ID's? Right parenthesis in "PM2.5(ug/m3)" is too small.

7.) Table 4: In baseline column, "Historical value (July 6th)" missing "y".

8.) Figure 5: Definitely help if the height (km) were included on y-axis and the specific atmospheric conditions described in the text (page 7: thermal inversion vs stable atm.) were highlighted in figure. Furthermore, UIL and YLW are not labeled on Figure 1, which would also really help.

9.) Section 2.4 is not directly relevant to the manuscript and should be removed or placed in SI.

Author's response:

6) will fix a, b, c subscript order. "*" redundant with subscript "a" and should be removed.

8) Having height label on the Skew-T would be challenging as the display is configured based on a standard atmospheric sounding visualization. A rough rule of thumb is that pressure decrease by 100 hPa for every vertical km in the atmosphere up to 500 hPa. I can add the site label and valid time as a label to each plot if needed. Currently labeling seems reasonably clear.