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Interactive comment on "Impacts of the July 2012 Siberian Fire Plume on Air Quality in the Pacific Northwest" *by* Andrew Teakles et al.

Anonymous Referee #2

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Acp-2016-302 "Impacts of the July 2012 Siberian Fire Plume on Air Quality in the Pacific Northwest" Teakles et al. Review:

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.

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?

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.

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.

3.) The most interesting aspect of this manuscript, from my perspective, is the quantita-

tive 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. Here are some examples and suggestions.

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

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 – 20 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.

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

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 O_3 and $PM_{2.5}$ in the LFV network at night. While the O_3 issue is discussed, the obvious discrepancy in $PM_{2.5}$ is not discussed. All of these issues add further insight into the issues with current (or at least this particular) air quality model.

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?

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).

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.

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