Reviewer 2

The study by Carter et al. uses a chemical transport model to compare four widely used fire emission inventories and assess the diversity in emitted and simulated quantities of biomass burning aerosol (BBA). To examine the performance of the model driven by the different fire emission inventories, model simulations are evaluated against in-situ and remote sensing observations. Implications of the diversity in fire emission estimates on air quality and aerosol radiative effects are also quantified and discussed. The paper is well written and structured, and the figures are well presented and clear. The concept of the study is certainly within the scope of ACP and I believe it will be very useful to both developers and users of these fire emissions datasets. I have a few minor general and specific comments (listed below) and I strongly recommend publication in ACP once they have been addressed.

We thank the referee for their comments and questions regarding our submitted manuscript. Below we have provided a list of the referee's specific comments and our responses in blue for each point.

General comment

This study does a good job of exploring some uncertainties associated with quantifying fire emissions and the diversity between the different fire emissions datasets; and the subsequent impacts of these on simulated BBA. However, I would argue that without a full sensitivity analysis the study cannot fully quantify the magnitude and causes of uncertainty in simulated BBA. In reality, the sensitivity of simulated BBA to uncertainties in fire emissions is likely to be much larger than estimated in the paper because (as acknowledged by the authors e.g. in Section 3 paragraph 3) additional factors that are not considered may increase the estimated uncertainty range in fire emissions of BBA and secondly, because the sensitivity of simulated BBA to uncertain parameters is assessed one-at-a-time and interactions are not considered. There is some discussion in the paper in relation to the former e.g. L115-116 and L370, however it could be made clearer that the quantified "uncertainty" range is really the diversity range between emission datasets and that the full uncertainty range in fire emissions is yet to be quantified.

We appreciate this comment and very much agree that our work does not fully assess the full uncertainty range in fire emissions. In the introduction, lines 121-122 clarify that we refer to "the spread across these inventories as the "uncertainty" in emissions; however, we note that additional factors, not represented by any of these inventories, may increase the true uncertainty in the estimated emissions." To address the reviewer's comment, we have added text to reiterate this statement in the conclusions.

Specific comments

1. Abstract (P1, L20): "We aim to quantify the uncertainties associated with fire emissions..." related to the general comment above, I would say the study aims to explore the uncertainties rather than quantify them.

Thank you for this comment. We have changed quantify to explore.

2. Abstract (P2, L37-39): "...sizeable range in BBA population-weighted exposure..." Could you state the time period you quantify the population-weighted exposure for (year and averaging period) and stress that it's exposure to BBA PM2.5.

We have clarified in text that this is: "2012 annual BBA $PM_{2.5}$ population-weighted exposure over Canada and the contiguous United States."

3. Introduction (P2, L48-49): "Because of their small size..." Here it would be better to identify that it's aerosol in the size fraction below 2.5μm that can penetrate deep into the lungs.

We have clarified in text.

4. Introduction (paragraph 1): can you include some more references from the epidemiological literature for specific health impacts of BBA?

The Reid et al. 2016 paper addresses the specific health impacts of BBA, and we have added other references to that effect.

5. Introduction (P2-3, L56-68): Nice summary of papers on in-plume SOA production. Could be worth adding that several studies (some already cited) have suggested that the limited net changes in SOA mass could be explained by a balance between SOA formation and dilution and evaporation of POA mass (e.g. Jolleys et al., 2015; May et al., 2015; Zhou et al., 2017; Morgan et al., 2019...etc.).

We agree with the reviewer, and this is addressed by our statement: "the majority of field studies have reported no secondary aerosol formation (above dilution-corrected POA concentrations…)."

6. Introduction (P3, L79-80): "The uncertainty in fire radiative impacts has not been assessed." This sentence could be written in a clearer way as there has been some previous assessments of the influence of biomass burning emission uncertainty on aerosol radiative forcing (e.g. Carslaw et al., 2016; Hamilton et al., 2018...). Perhaps just add "in detail" to the end of the sentence or something similar to: "The uncertainty in fire radiative impacts due to uncertainty in fire emissions has not been assessed in detail."

We have added "in detail" as suggested.

7. Section 2.1 (P6, L168-169): Can you specify whether fire emissions are averaged evenly across the PBL or if there is a gradient applied? What is the typical (or peak) model height of the daytime PBL over North America? What is the average model(vertical) resolution in the PBL over North America?

Thank you very much for this question. Fire emissions are emitted from the surface and then mixed throughout the whole PBL, which we have clarified in text. We use the VDIFF PBL mixing scheme (described in Lin et al. (2008, AE) and Lin et al. (2010, AE)) as currently

implemented in GEOS-Chem where the PBL height is taken from the meteorological dataset (in this case, MERRA-2).

8. Sections 2.3 & 2.4 (general): Really nice descriptions of the observations and their uncertainties. However, these uncertainties are not referred to or taken into ac-count in the results section (when the model is evaluated against these observations). I'm guessing that this is because the variability in observations and the model structural and emission uncertainties likely far outweigh measurement uncertainties, but this should be mentioned.

We have now stated this explicitly in the text.

9. Section 3 (P10, L312): "variable in this inventory (i.e., more variability from 2004-2016 as evidenced by the taller boxplots)". I'm not sure the term "taller boxplots" is clear here. Do you refer to the larger range between 25th and 75th percentiles for QFED? Can you give the range?

We have clarified in text that we mean the wider range between the 25^{th} and 75^{th} percentiles. We chose not to include the range as it is different across species.

10. Section 3 (P10, L314-315): Are you referring to the global mean total annual emissions here?

Yes, we have clarified in text.

11.Section 3 (paragraph 2): Can you add a line or two about any differences/similarities in the spatial pattern of emissions between the datasets (just for CONUS)?

We have added a couple sentences describing the spatial patterns.

12. Section 4 (general): Can you give some numbers to quantify the model skill in the text so that the different simulations can be quantitatively compared? Perhaps give temporal correlation values and/or model bias where appropriate.

We have added R^2 values for the spatial plots (Figure 8).

13. Section 6 (general): To calculate fire PM2.5 are the BC and OC mass fractions summed for aerosol smaller or equal to 2.5 ï¸Am? Is there any contribution to PM2.5from "primary" sulphate?

To calculate fire $PM_{2.5}$, we sum the BC and OA mass fractions for aerosol under 2.5 microns and have defined this in text. We do not include any contribution from sulphate.

14. Section 6 (general): Do you see a range in exposure due to the differences in spatial patterns of the fire emissions? Are there any years that stick out?

We do see a range in exposure due to differences in the spatial patterns of fire emissions. 2014 continues to be an outlier year for QFED2.4, and FINN is also larger in this year than in others –

leading to a smaller and a more significant population-weighted exposure, respectively, as a result.

15. Figure 8: It is very difficult to distinguish the colours of the overlapping circles (the black outlines obscure the colour inside the circle), particularly in the west. I suggest either showing an average in crowded regions or perhaps overlay the circles instead and just show the top colours.

We appreciate the reviewer's comment and have removed state lines from this figure to enhance legibility.

16. Figure 11: It is difficult to assess the magnitude of the difference between the model and observations in this figure. I suggest including a figure showing some quantification of the difference e.g. showing the spatial distribution of the absolute difference or model bias? This figure could be put in the supplementary material.

This plot is mainly for qualitative use, but your point is well-taken. We have added a plot of the spatial distribution of the model bias to the supplement.

References included in the review above:

Carslaw, K. S. et al. Large contribution of natural aerosols to uncertainty in indirect forcing. Nature 503, 67–71, 2013.

Hamilton, D.S., Hantson, S., Scott, C.E. et al. Reassessment of pre-industrial fireemissions strongly affects anthropogenic aerosol forcing. Nat Commun 9, 3182,doi:10.1038/s41467-018-05592-9, 2018.

Jolleys, M. D., Coe, H., McFiggans, G., Taylor, J. W., O'Shea, S. J., Le Breton, M.,Bauguitte, S. J.-B., Moller, S., Di Carlo, P., Aruffo, E.,Palmer, P. I., Lee, J. D., Per-cival, C. J., and Gallagher, M. W.: Properties and evolution of biomass burning or-ganic aerosol from Canadian boreal forest fires, Atmos. Chem. Phys., 15, 3077–3095,https://doi.org/10.5194/acp-15-3077-2015, 2015.

May, A. A., Lee, T., McMeeking, G. R., Akagi, S., Sullivan, A. P., Urbanski, S., Yokel-son, R. J., and Kreidenweis, S. M.: Observations and analysis of organic aerosolevolution in some prescribed fire smoke plumes, Atmos. Chem. Phys., 15, 6323–6335,https://doi.org/10.5194/acp-15-6323-2015, 2015.

Morgan, W. T., Allan, J. D., Bauguitte, S., Darbyshire, E., Flynn, M. J., Lee, J., Liu,D., Johnson, B., Haywood, J., Longo, K. M., Artaxo, P. E., and Coe, H.: Transforma-tion and aging of biomass burning carbonaceous aerosol over tropical South Americafrom aircraft in-situ measurements during SAMBBA, Atmos. Chem. Phys. Discuss.,https://doi.org/10.5194/acp-2019-157, in review, 2019.

Zhou, S., Collier, S., Jaffe, D. A., Briggs, N. L., Hee, J., Sedlacek III, A. J., Kleinman,L., Onasch, T. B., and Zhang, Q.: Regional influence of wildfires on aerosol chemistryin the western

US and insights into atmospheric aging of biomass burning organicaerosol, Atmos. Chem. Phys., 17, 2477–2493, https://doi.org/10.5194/acp-17-2477-2017, 2017.