Reviewer 1

The goal of this study presented in this paper is to quantify the uncertainties in the air quality and radiative impacts associated with BB aerosol emissions due to biomass (fuel) consumption and emission factors. The approach employed drives GEOS-Chem simulations with different BB emission inventories, two bottom-up (GFED4s and FINN) and two top-down (GFAS1.2 and QFED2.4). Results from the GEOS-Chem simulations are compared with observations of carbonaceous aerosols, black carbon (BC) and organic aerosol (OA), and satellite-derived aerosol optical depth (AOD) over North America. The authors find large differences in emissions and resultant air quality impacts across the four inventories explored. The authors conclude that differences between emission inventories in US, Alaska-Canada, and globally are driven by differences in fuel consumption, not emission factors.

The topic of this study is of great interest to the atmospheric chemistry and climate communities. The study was well designed and the authors are careful in their interpretation of the results. The manuscript is very well written and the presentation of methods and results is thorough, yet concise. I have only one significant issue with the paper and that is Section 6 (see comments below).

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.

Specific Comments

L41: define AR5

Done.

L49-50: It should be noted that deep penetration of lungs and most acute health impacts are generally associated with fine PM (PM2.5) fraction of PM.

Thank you for your comment. We have added language to this effect.

L78-79: This insertion of "climate forcing" seems incorrect. Even if one defines climate forcing as a perturbation of the Earth's energy balance, it need not be anthropogenic. Large volcanic eruptions result in climate forcing. And anthropogenic activities, e.g. land use and fire suppression (Andela et al., 2017), can reduce natural fire activity.

We agree that this our statement could be clarified on this point, and have added specific reference to the fact that all human activity (including ignition, suppression, and changing of fuel availability) impacts the DRF of fires.

L169-170: Did the authors select the WRAP profile as opposed to Mu et al. (2011) since the focus was North America? Were there any complications/problems with using the WRAP cycle, which is intended to represent western US wildfires, for fires globally? Mu et al. (2011) Daily and 3â˘AR˘ hourly variability in global fire emissions and consequences for atmospheric model predictions of carbon monoxide, JGR Atmospheres, 116, D24303

Yes, we selected the WRAP diurnal scale factors because of our North America focus and their previous use in studies focused there (Kim et al. 2015; Saide et al. 2015). We did not verify whether this accurately represents the diurnal cycle in other regions of the world, but as the focus of our analysis (particularly with respect to observational comparisons) is on North America, we do not expect this to impact our results.

L247-256: The authors should better describe the challenge of FRP methods using MODIS data associated with the sparse temporal coverage. The most significant weakness in FRP based methods using observations from MODIS is the need to estimate FRP (for time integration to get FRE) between temporally sparse observations. Under cloud free conditions, in mid-latitudes Terra & Aqua provide 4 observations a day, maybe 6 depending on swath overlaps. FRP methods often require estimating FRP between the Aqua over-pass _13:30 LT and Terra _22:30 LT over-pass, the period of peak fire activity in the western US and western Canada. (At high latitudes swath overlap increases and temporal coverage is much better).

Thank you for this comment; we have added some additional discussion of the particular challenges associated with FRP-based approaches to the manuscript.

L266-267: 0.16 ug/m3 several orders of magnitude lower than typical field BBOA concentrations? That suggests typical field BBOA around 2000 ug/m3. Is this typical? PM1 level of 2000 ug/m3 seems like a somewhat concentrated smoke plume. Please clarify.

Thank you for pointing this out. Typical field BBOA measurements are often >= 10 ug m^{-3}, which is multiple orders of magnitude; however, our statement was vague. We have clarified in text.

L336-338: Liu et al. (2017) findings imply that the EFPM values used for western US wildfires may be higher than those used shown Fig 3. This should be clarified.

Thank you for this comment. We have added some text on this, as well as a reference to the new Andreae (2019) compilation that recently came out, to the manuscript.

L343-353: Are the results for Boreal NA and CONUS similar if one uses EFOC to derive DM?

Yes, in terms of the calculated DM consumed, the numbers are nearly identical when using either the OC or BC EF and emissions pairing.

L383-384: It is likely that prescribed understory burning of forests in the southeast US are also a significant contributor to the CONUS springtime peak.

Thank you – we have clarified in text.

L395-398: “The southeastern US, in particular, 395 is of interest to the public health and policy communities because a prevalence of agricultural burning there, which dominates burned surface area (Nowell et al. 2018), may have a stronger impact on low altitude air quality in a relative sense than large wildfires that inject higher into the air." While agricultural fires comprise a large share of fires and area burned in the southeast it does not dominate surface burned area. Nowell et al. (2018) reports that in Florida "silviculture fires consumed the most area (5.5 _ 6.7 _ 105 ha/year), burning 50% more than agricultural fires." More broadly across the southeast, the 2018 National Prescribed Fire Use Survey Report conducted by the National Association of State Foresters and the Coalition of Prescribed Fire Councils reported that 77% of prescribed fire acres burned in 2017 were forestry related compared to 23% agricultural. This breakdown is comparable to previous surveys released by the organizations in 2012 and 2015.

https://www.stateforesters.org/newsroom/nasf-coalition-of-prescribed-fire-councilsrelease-national-survey-on-prescribed-fire-use/
https://www.stateforesters.org/wp-content/uploads/2018/12/2018-Prescribed-Fire-Use-Survey-Report-1.pdf

We thank the reviewer for this comment and have clarified that the SEUS is dominated by prescribed and agricultural fires in text.

L454-461: It would be helpful compare FiNN burned area versus GFED over CONUS. Maybe just add a sentence comparing average annual burned area. I suspect this would indicate a large difference in burned area, especially in the west, as the authors suggest. GFAS and QFED estimate/interpolate FRP between MODIS observations, essentially gap-filling for the large time periods without observations.

We thank the reviewer for this suggestion and have briefly looked into the difference between annual average burned area for GFED and FINN over CONUS. We actually find that FINN produces somewhat larger average annual burned area than GFED. This remains consistent with our point that it is both the relationship between fire activity and dry matter consumed and lower EFs used by FINN that contribute most to the lower values seen with FINN as shown in Figures 2-6.

Section 6: While I find this analysis and interpretation valid, it leaves the impression BB smoke is not relevant wrt population exposure. I believe the 24 hour average PM2.5 is the metric that should be used for BB health impacts (35 ug/m3 per NAAQS). Day to week length exposures to wildfire smoke are associated with negative health impacts, see e.g. Liu et al. (2015), Fisk and Chan (2017), Moeltner et al (2013), Williamson et al. (2016). In the western US, days with high PM2.5 or days where PM2.5 exceeds the NAAQS standard, tend to associated with BB smoke (Liu et al., 2016; Brey et al., 2018; McClure and Jaffe, 2018). Section 6 should have focused on 24 hour average PM. I do not think it is necessary for the authors to so; however, I think it would improve the study and perhaps without significant extra effort. If the authors decide to not include an analysis based on 24 hour average PM, then they need to discuss smoke – health impact linkages associated with day – week(s) exposure.

Fisk and Chan (2017) Indoor Air, 27, 191–204
Liu et al. (2015) Environmental Research, 136, 120-132
Liu et al. (2016) Climatic Change, 138, 655-666
Moeltner et al. (2013) Journal of Environmental Economics and Management, 66,

476-496
McClure and Jaffe (2018), PNAS, 115, 7901-7906
Williamson et al. (2016) Environmental Research Letters, 11, 125009

We thank the reviewer for this insightful comment and have added a discussion of 24-hour average BBA contribution to $PM_{2.5}$ across 2012 in CONUS and how it differs when using different emissions estimates. We have also added a figure in the supplement showing the range in the number of NAAQS exceedances per gridbox due to BB-only in 2012 when using each inventory.

Technical Comments
L29-32: Jumbled / missing text sentence L164: “OA” or “POA”?

Thank you for pointing this out. We have clarified in text that we are referring to POA.