Reviewer #1 (Bob Yokelson) Comments and Responses

General Comments:

This manuscript reports much needed, very important, high-quality boreal forest fire smoke measurements with impressive modeling support and the work should very much be published. Unfortunately, there seems to be an error in the calculation of emission factors (EFs) explained in detail below. If so, that will require revisions to reported values, re-interpretation of the implications, and re-review. As explained below, the data may in fact support earlier EFs rather than suggest they should be higher. I am submitting a quick, rough review so the authors can correct this if needed or validate their calculation if appropriate. I'm happy to communicate directly with the authors about the calculations and to review the paper in more detail after the calculations can be verified to be correct, and, if needed, the analysis and conclusions are appropriately modified.

A second, relatively minor, general comment is that there is some missing context that could be added to the intro or discussion that could help motivate why the authors data is so valuable and perhaps inform the interpretation. I'll summarize that next.

Bertschi et al., (2003) showed that adjusting EFs for rarely sampled residual smoldering combustion (RSC) led to important adjustments in the EFs for all fire types and especially for fires burning heavy or duff fuels. Christian et al., (2007), Burling et al., (2012), Akagi et al., (2013; 2014) and others all supplemented airborne measurements with ground-based measurements on the same fire to explore this, but the relative importance of weakly lofted smoldering and flaming emissions could only be crudely estimated from size-/type-resolved fuel consumption measurements, which are challenging and rare. Yates et al., (2015) showed that even airborne measurements can imply a much larger smoldering/flaming ratio late in long-lasting fires. Saide et al., (2015, and references therein) showed that rarely sampled nighttime combustion is both important and underestimated in some cases using commonly assumed diurnal cycles. So there is precedent and ample support in the literature for factoring in smoldering and nighttime combustion, but little data to judge the potential differences in emissions or the relative production. For this reason, Selimovic et al., (2019a, b) deployed ground-based smoke monitoring downwind of hundreds of fires burning at all stages for two fire seasons. A priori, one might suspect that ground-based sampling could be biased towards smoldering and airborne sampling to flaming, but these authors found that conserved tracers sensitive to flaming (BC) and smoldering (CO) had a similar ratio from both air and ground. This implies both platforms are relevant and maybe even in sufficient agreement for some purposes. Other findings from this work are relevant/comparable to the authors work as well. Even earlier, the widely used Akagi et al., (2011) recommendations for boreal forest fire EFs had been based on averaging ground and airborne measurements together as a "best guess" at overall EFs. Finally, It's very likely that ground-based downwind measurements are best for validating AQ models, but it may be that satellite or aircraft vertical profiles will be needed to best probe overall emissions. Climate assessments may be more interested in smoke in higher layers, which may be missed by towers? However, this work is an extensive and welcome addition to the information available.

Next some details on why it is unclear if the authors got "much higher EFCO" and whether their work actually implies more smoldering than previously assumed since MCEs are directly measured and similar to some widely used previous work.

To start, I compare the authors EFs at face value to those from some widely-used recommendations: namely Andreae and Merlet 2001, now updated (Andreae, 2019) and Akagi et al., (2011). Akagi et al recommended a 50/50 average of the ground-based and airborne EFs in their boreal recommendations. For boreal the 50/50 ground/air led to EFCO of 127(45) g/kg compared to the authors 145(46) g/kg in their Table 1. So if there EF is correct it is 14% higher. They are closer to the A11 ground-based average of 157. Andreae (2019) recommend the straight average of 20+ studies, which is 121.4(46.6). Putting the EFs and MCEs together in a table reveals some things.

	EFCO	MCE	EFCH4	n
A11	127(45)	.881	5.96(3.14)	7 studies multiple fires per study
A19	121.4(46.6)	.89	5.5(2.5)	20+ studies
Wiggins	145(46)	0.879(0.068)	6.05(2.09)	35 fires

While the authors current calculated EFCO is about 15% larger, the directly-measured MCEs are very close so maybe this new data does not imply more smoldering? Also this works MCE of 0.879(0.068) is not far from Selimovic et al (2019) estimated MCE (based on BC/CO) for similar long-lasting fires in heavy fuels of 0.87(0.02).

The similarity in MCE along with non-standard notation in eqn 2 and a lack of definition for the authors "S" scalar inspired me to calculate EF directly from their emission ratios (ERs). Using the authors quoted assumption of 45% C for the fuel; I get different EF values than them: EFCO2 1437, EFCO 126.2, EFCH4 5.23. These EF values are the same or lower. If my calculation is right, then this new work supports the previous work rather than suggesting the values in use should be increased. It's still good data even if it agrees with previous work. Also %C > 45% is possible for boreal fires. 50% C is often assumed though one study (Santin et al., 2015) did measure fuel C close to 45% for a fire in boreal forest. But using the authors average ERs I have to assume 519 gC/kg to get close to their EF for CH4 and CO; and ~52%C seems to high. I'd be happy to share my calculation (Yokelson et al., 1999) and re-review a revised paper if necessary.

Another possible reason for an ER-EF mismatch is using different averaging schemes for these two quantities? Ideally the averaging scheme should be the same for both quantities. If possible, it might be good to weight for how much smoke was produced at the fire, received at the tower, duration of events, or etc. Exploring how the average depends on the scheme employed is always useful and could be reported along with a clear explanation of how the averaging was done for the reported values.

A few other things I noticed in order of Page, Line. This is a one-skim set of potentially useful comments. A more careful review could be done after ensuring the calculations are accurate.

We appreciate Dr. Yokelson's positive comments that our manuscript provides needed and important CO and CH_4 emission factor measurements from boreal forest fires.

We agree with his assessment that there was an important omission in our emission factor calculation. Finally, Dr. Yokelson provides a valuable perspective (and references) on past work that has compared ground-based and aircraft-based estimates of emission factors.

We propose the following major revisions to our paper to address these issues.

First, our emission factor calculations will be corrected. This is easy and relatively straightforward to implement. More importantly, we will reinterpret our results and their implications in our revised manuscript, taking into account the revised emission factor information. This will require revisions to the title and abstract as well as main text.

Second, we will change equation 2 in our manuscript to standard notation as published in previous studies (Yokelson et al., 1999) and offer more clarity on the definition of the variables.

Third, will modify the introduction and discussion to include more context to motivate the importance of this study and inform the interpretation of our results. We plan to include the studies Dr. Yokelson highlighted in our revised manuscript. Specifically, we plan to integrate work by Bertschi et al. (2003), Christian et al. (2007), Burling et al. (2011), Akagi et al (2014), Santin et al. (2015), Yates et al. (2016), Andreae (2019), and Selimovic et al. (2019a,b), and Yokelson et al. (1999).

Fourth, as describe above, we have carefully evaluated emission ratio and emission factor observations from past measurements of boreal forest fires in North America and Siberia, taking into account studies reported by Andreae (2019) and also reports provided by Yokelson in his review. A compilation of these studies will now be provided in our revised Table 1. In this context, we note that in comparison to North American boreal forest fires sampled by aircraft, our CO emission ratios are still considerably higher, implying our observations do provide evidence for stronger role of smoldering combustion. Fires in boreal Siberia tend to have even higher CO emission ratios than North American fires, which is consistent with well known differences in fire behavior between the continents (Rogers et al., 2015). We look forward to the reviewer's perspective on the new analysis in this revised Table. A draft of the table is included here:

Study	CO Emission Ratio	MCE	# Fires
Airborne Wildfires North America			
Cofer et al., 1989	0.069 ± 0.004	0.935 ± 0.004	1
Cofer et al., 1998	0.140 ± 0.012	0.878 ± 0.009	1
Friedli et al., 2003	0.100 ± 0.020	0.909 ± 0.017	1
Goode et al., 2000	0.085 ± 0.008	0.922 ± 0.007	4
Laursen et al., 1992	0.050 ± 0.007	0.953 ± 0.006	1
Nance et al., 1993	0.078 ± 0.012	0.928 ± 0.011	1
O'Shea et al., 2013	0.150 ± 0.024	0.871 ± 0.012	4
Radke et al., 1991	0.116 ± 0.087	0.896 ± 0.075	1
Simpson et al., 2011	0.110 ± 0.070	0.901 ± 0.061	5
Fire Weighted Mean	**0.102 ± 0.033**	**0.908 ± 0.027**	**19**
Airborne Management Fires North America			
Cofer et al., 1990	0.086 ± 0.008	0.921 ± 0.007	2
Cofer et al., 1998	0.095 ± 0.016	0.913 ± 0.013	7
Radke et al., 1991	0.047 ± 0.032	0.956 ± 0.030	4
Susott et al., 1991	0.060 ± 0.061	0.943 ± 0.058	1
Fire Weighted Mean	**0.077 ± 0.022**	**0.929 ± 0.020**	**14**
Laboratory North America			
Bertschi et al., 2003	0.151 ± 0.040	0.870 ± 0.030	-
Burling et al., 2010	0.209	0.827	-
Mcmeeking et al., 2009	0.091 ± 0.038	0.917 ± 0.068	-
Mean	**0.150 ± 0.039**	**0.871 ± 0.049**	
Siberia – Surface and Airborne			
Cofer et al., 1998 (A)	0.224 ± 0.036	0.817 ± 0.025	1
McRay et al., 2006 (A & S)	0.249 ± 0.064	0.800 ± 0.043	6
Vasileva et al., 2017 (S)	0.126 ± 0.007	0.888 ± 0.005	2
Fire Weighted Mean	**0.219 ± 0.048**	**0.822 ± 0.033**	**9**
Ground Wildfires North America			
Wiggins et al., 2016	0.128 ± 0.023	0.887 ± 0.018	3
This study	0.142 ± 0.051	0.878 ± 0.039	35
Fire Weighted Mean	**0.141 ± 0.049**	**0.879 ± 0.027**	**38**

Fifth, we will add text to the methods to describe how we average the different individual fire events together to come up with a season-wide mean. We will explore the sensitivity of this to the averaging techniques, also reporting at CO_2 anomaly-weighted mean.

We respectfully ask the editors to allow us to update our calculations and revise the corresponding manuscript introduction and discussion prior to the next iteration of reviews. Below we address specific comments but note some of the responses will depend on our corrected results.

Specific Comments:

Comment 1:
P1, L14: define CRV
Response: In our revised paper, we plan to change the sentence to read: "Here we quantified emission factors for CO and CH4 from a massive regional fire complex in interior Alaska during the summer of 2015 using continuous high-resolution trace gas observations from the Carbon in Arctic Reservoirs Vulnerability Experiment (CRV) tower in Fox, Alaska."

Comment 2:
P1, L33 – P2, L2: will these aggressively lofted emissions impact tower? Run some forward/back trajectories? Vertical mixing?
Response: To address this comment, in the methods section of the revised manuscript, we will add the following sentences: " Here we emitted fire emissions into the surface influenced volume of PWRF-STILT, which extends from the surface to the top of the planetary boundary layer, with the assumption that fire emissions were equally distributed within the planetary boundary layer [*Turquety et al.*, 2007; *Kahn et al.*, 2008]. In a previous study using the same tower, a sensitivity study revealed that plume injection height contributed only minimally to variability in simulated fire-emitted CO with PWRF-STILT [*Wiggins et al.*, 2016]."

Comment 3:
P2, L6: "deadly" AQ is over-simplified
Response: We will change "deadly" to "unhealthy."

Comment 4:
2, 13: Andreae and Merlet was updated in 2019
Response: We will update all of the appropriate references to Andreae and Merlet (2001) to Andreae (2019).

Comment 5:
2, 18: The updated Andreae paper lists more than 20 studies, so there may be more worth including in Table 1.
Response: As described above, we will update Table 1 and corresponding text to include the missing studies of field measurements of boreal forest fire emissions from Andreae (2019), along with other studies from laboratory measurements, studies that measured emissions from land management fires, and studies from Eurasian boreal forest fires that exist in the literature.

Comment 6:
2, 28: Ground-based data downwind of fires has also been collected in Selimovic et al (2019a, b) and e.g. in the Colorado front range (Gilman, Benedict) and MBO (Collier and references there-in). The Wiggins 2016 data doesn't appear in this paper anywhere that I saw.
Response: We will update this sentence to now read: "This approach has been used to estimate CO emission ratios during a moderate fire season in Alaska [*Wiggins et al.*, 2016] and for fires in other ecosystem types [*Gilman et al.*, 2015; *Collier et al.*, 2016; *Benedict et al.*,

2017; *Selimovic et al.*, 2019a,b].” We also added the fires sampled in Wiggins et al. (2016) to Table 1, using the same approach as described by our revised equation 2 and 3 to calculate emission factors.

Comment 7:
2, 33-34: Akagi et al., 2011 explain how MCE can be used to estimate an arbitrary mix of smoldering and flaming over a continuous range.
Response: We will add the following sentence on Page 2 Line 35 to include an explanation of how MCE and be used to estimate contributions from smoldering and flaming combustion: “The relative amounts smoldering and flaming combustion are difficult to measure, but can be estimated using the modified combustion efficiency (MCE) defined as $\Delta CO_2/(\Delta CO_2 + \Delta CO)$. Fire emissions dominated by flaming combustion have an MCE up to 0.99 while emissions dominated by smoldering combustion have an MCE often between 0.65 and 0.85 (Akagi et al., 2011). MCE can be used to understand the relative contributions from both flaming and smoldering fire processes.” We also changed our criteria for separating the different combustion phases to align with previous studies.

Comment 8:
2, 37: Real fires often don’t have phases - rather a dynamic mix of processes. Change “phase” to “process” throughout?
Response: This is a good point and we agree with the reviewer. We will change “phase” to “process” where appropriate throughout the manuscript.

Comment 9:
3,22: The tower results, even if lowered are higher than “some” airborne studies. E.g. Cofer 98 is the same. If the EFs stay the same they are “a bit higher” than “some” previous estimates or recommendations.
Response: We will revise the text to reflect our modified perspective after recomputing the emission factors. The text in section 4.2 will change to “Our emission factors for CO and CH_4 were in agreement with the mean of previous estimates for boreal fires derived from a compilation of all past studies. However, if studies that are not representative of North American boreal wildfires are excluded, including measurements from prescribed fires, laboratory studies, and studies of fires in the Eurasian boreal forest, our emission factors are 39% higher than average emission factors derived primarily from aircraft studies of wildfires in the North American boreal forest.”

Comment 10:
4, 6: Confusing, is it just 50 minute samples with ten minutes downtime per hour?
Response: Yes, the tower collects continuous measurements for 50 minutes out of the hour. We will clarify this point by changing the text on Page 4 line 5 to “…to separate the dataset into a set of continuous 50-minute intervals of trace gas observations…”

Comment 11:
4, 30: EFs are usually given for one species so the meaning of the ratio in the subscript here and in eqn 2 is not apparent. Suggest adopting standard notation?

Response: We will remove the ratio in the subscript of our emission factors to align with standard notion.

Comment 12:
4, 32: Do these references unambiguously support 45% C? Revisit, consider reference above, and explain in detail in revised text.
Response: We now provide a referfence to Santin et al. (2015) and add text to explain the variability can range from 45 – 50%.

Eqn (1) Sum or slope or simple subtraction?
Response: To clarify we will add the following text to Page 4 Line 30: "Excess mole fractions denoted with a Δ symbol refer to observations of trace gas mole fractions during intervals when fire had a dominant influence on tower trace gas variability with background values subtracted."

Eqn (2) Again, notation unusual, something common should work, or explain?
Response: We will change the notation to: $EF_X = F_C * (1000g/kg) * MM_X/12.01 * ER_X/C_T$ Where Fc refers to the carbon content of the fuel (45), MM_{CO} is the molecular mass of CO, ER_{CO} is the emission ratio of CO relative to CO_2 and $C_T = \Sigma N_i * \Delta C_i/\Delta CO_2$.

Comment 13:
5, 1-5: I get computing MCE for each sample, but what's the point of the categories that don't seem to be used?
Response: We use the categories to separate our emission factor calculations and aid in the interpretation of our results as shown in Table 1, in Figure 4, and as discussed in section 3.1. Specifically, we use these categories to allow the reader to visually identify whether there is a trend toward one or another emissions type throughout the fire season. As shown in Figure 4, intervals with smoldering, mixed, and flaming emissions types were interspersed throughout the fire season.

Comment 14:
Sec 2.3: There is not much detail on how AKFED is driven. One thing that stands out though is that the day/night split for fuel consumption is likely not right for 64 N! See Vermote et al, (2009); MODIS FRP can be higher at "night" than during the "day" in high latitude summer. This is relevant later.
Response: We will add the following text to Section 2.3 to better explain how AKFED is created:

"AKFED burned area is mapped using perimeters from the Alaska Large Fire Database combined with imagery from the Moderate Resolution Imaging Spectroradiometer (MODIS). Both above and belowground carbon consumption are modeled based on elevation, day of burning, pre-fire tree cover, and the difference normalized burn ratio (dNBR) [Veraverbeke et al. 2015]. AKFED predicts carbon emissions from fires with a temporal resolution of 1 day and a spatial resolution of 450 m."

We created the diurnal cycle of emissions specifically for the analysis here. We conducted additional analysis of the active fires and fire radiative power (FRP) from the MODIS fire detection products measured during the 2015 fire season in Alaska to assess our approoah. The satellite data analysis reveals that the product of total number of active fires and FRP during the daytime Terra and Aqua overpasses accounts for 83% of total fire activity (the sum of fire activity from both daytime and nighttime overpasses). This is in line with our 90% day /10% night emissions split prescribed in the model. In this context, its important to note that if there was an afternoon satellite overpass 3 hour after Aqua (at 4:30pm), it would like be higher than the 10:30am Terra overpass, because relative humidity is lower and temperatures are considerably higher in mid-afternoon as measured from our earlier eddy covariance observation [Liu et al., 2005]. So the 83% estimate from MODIS is likely an underestimate of daytime fire activity. Vermote et al. (2009) concluded MODIS FRP can be higher at "night" than during the "day" in the boreal forest during summer, where "night" FRP is defined as the sum of FRP from both Terra overpasses and "day" is the sum of FRP from both Aqua overpasses. However, in this analysis for the summer of 2015, we found that the sum of FRP from both Aqua overpasses was higher than the sum from both Terra overpasses.

We changed the text to provide more justification for the 90/10 emission split we used:

"Analysis of the product of fire radiative power and fire detections from the MODIS MCD14ML C6 product showed that 83% of fire activity occurred during daytime overpasses (10:30am and 1:30pm) relative to the sum across both daytime and nighttime overpasses during the 2015 Alaskan wildfire season (data not shown). The satellite observations provide broad support for the diurnal cycle we prescribed for emissions in the model."

Here is a figure showing the FRP, sum of active fires, and product of the two that illustrates how FRP and total number of active fires was considerably elevated during daytime overpasses during the 2015 fire season in Alaska.

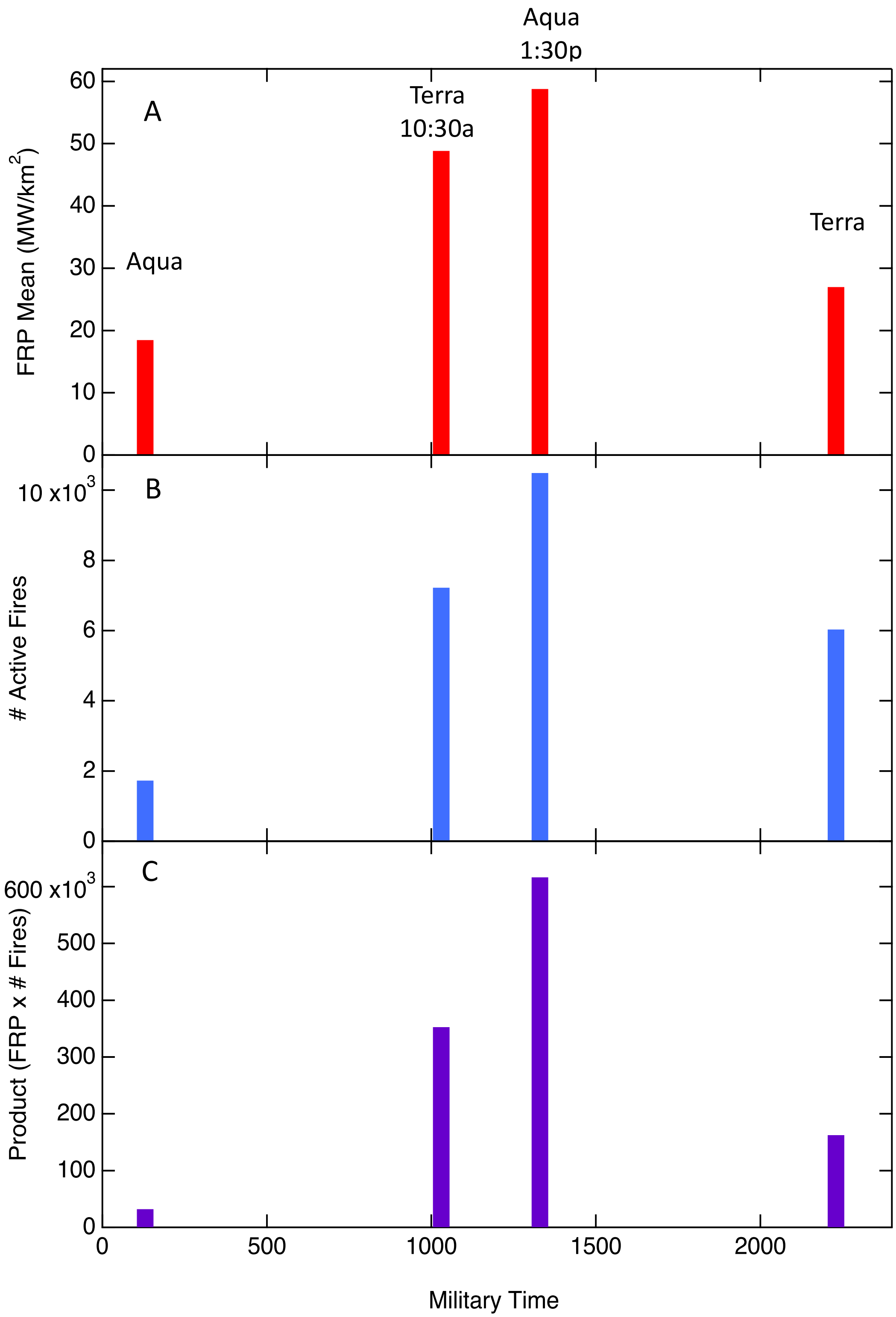

Figure 1. Panel A shows the mean FRP (MW) normalized by area (km^2) for all fires that occurred during the 2015 fire season in Alaska organized by the time and satellite of detection. B shows the total number of active fires, and C shows the product of A and B or the product of the area normalized mean FRP and the number of fires.

Comment 15:
6, 1-9: It's likely that some weaker smoke peaks are more distorted by background variability and increase the range of values, but there is not necessarily bias.
Response: We agree with the reviewer and believe our methodology is strict but unbiased.

Comment 16:
6, 9: Many references support high correlation of EFCH4 with MCE.
Response: In addition to the references already listed in section 4.3 we will add the following references to strengthen this point. The text will change to "A strong linear relationship existed between the CH_4 emission factor and MCE across the different sampling intervals (Figure 5). Linear relationships between CH_4 emission factors and MCE have also been observed in previous studies [*Yokelson et al.*, 2007; *Burling et al.*, 2011; *Van Leeuwen and van der Werf*, 2011; *Yokelson et al.*, 2013; *Urbanski*, 2014; *Smith et al.*, 2014; *Strand et al.*, 2016, *Guerette et al.*, 2018]. The relationship shown in Figure 5 implies MCE can be used as a metric for CH_4 emission factors from North American boreal forest wildfires when measurements of CH_4 are not available."

Comment 17:
6, 38-39: This needs to be thought through a bit. Does the high impact of night smoke at the tower compared to the assumed low fraction of smoke produced at night mean day smoke was under-sampled? Or does this imply AKFED underestimates night smoke?
Response: We will modify the analysis to span the same time intervals of the diurnal cycle that was applied to AKFED (0600 to 1800 for day and 1800 to 0600 for night). The text will read: "Overall, 73% of the fire emissions that impacted the tower occurred during the day (0600 to 1800 local time) and 27% occurred at night (1800 – 0600 local time)." AKFED has a daily resolution, but we accounted for diurnal variability in emissions by applying a diurnal cycle as explained in section 2.3. Our imposed diurnal cycle could be underestimating night smoke, or we could be measuring a slightly greater proportion of night smoke at the tower.

Comment 18:
7, 3: "emissions" to "consumption"
Response: We will change "emissions" to "consumption." The sentence will read: "The relative contributions of consumption from flaming and smoldering fires are uncertain for boreal forest fires…."

Comment 19:
7, 8: 15 total previous fires sampled may be too low if you check updated compilations.
Response: We will update the total number of previous fires sampled using studies included in updated compilations.

Comment 20:
7, 9: Convection entrains some smoldering.
Response: We changed the text in section 4.1 to the following: " … airborne sampling techniques struggle to measure emissions from less energetic smoldering combustion that emits smoke lower in the atmosphere [Selimovic et al., 2019a,b]. Emissions from smoldering

boreal forest fires can sometimes be entrained in the convective columns of certain flaming fires and can be sampled by aircraft, but nighttime emissions or residual smoldering emissions from fires that have weak convective columns usually cannot [Ward and Radke, 1993; Bertschi et al., 2003; Burling et al., 2010].”

Comment 21:

7, 10-17: True, but a tower could potentially undersample flaming. Flaming is associated with rapid fuel consumption so not a negligible concern. Try forward trajectories from high injection altitudes to see if they impact tower or compare to column data?

Response: We believe the tower is at an optimal location and height to sample integrated emissions from both flaming and smoldering fires. The tower is on average 295 km away from the fires we sampled and located on a ridge that is over 600m above sea level. The long distance the emissions have to travel in order to reach the tower allows for mixing throughout the planetary boundary layer. Most of the fire emissions from boreal forest fires in Alaska remain in the PBL as shown by a MISR plume height analysis in Wiggins et al. (2016). To clarify, we will include the following text in section 3.2: “CRV tower is sufficiently downwind to integrate both flaming and smoldering processes from fires across interior Alaska.”

Comment 22:

7, 19: Night may have been oversampled? But maybe not if there really is more emissions at night than was assumed in AKFED? As noted above, there is sometimes more MODIS FRP at night than day in boreal regions. Also, as above, towers may not be sensitive to the entire range of injection altitudes? Explore?

Response: We will add the following text to explain why the tower is not sensitive to injection altitude: “In a previous study using the same tower, a sensitivity analysis that included modifying the vertical resolution of the surface influenced volume of PWRF-STILT revealed that plume injection height contributed only minimally to variability in simulated fire-emitted CO with PWRF-STILT [*Wiggins et al.*, 2016].”

Comment 23:

7, 20-21: Quote these values from 2016 paper in Table 1?

Table 1 header or caption: It’s enormous! Move part elsewhere. The text mentions CH4 data which I did not see in table.

Response: We will add values from Wiggins et al. (2016) and other recent studies and updates as requested by the reviewer. We will also remove the reference to CH_4 data. We will move some of the caption to table footnotes.

Comment 24:

7, 27: Cofer 98 agrees with this study’s current values and the real average may be in middle of all this data somewhere.

Response: This section of the discussion will change to reflect a new interpretation of our corrected emission factor data.

Comment 25:

7, 28-31: This data should certainly be used, but rarely does new data replace old data completely. More often new data contributes to an evolving literature average – sometimes with weighting by n factor.

Response: We agree with the reviewer and offer a weighted average to use instead.

Comment 26:

7, 36: You can often see smoke by satellite even when you can't detect FRP.

Response: Although this is true, FRP is often used to estimate emissions and missing detections correspond to missing emissions. We will change the original sentence "This residual smoldering combustion could substantially contribute to trace gas emissions, but is difficult to detect and quantify using remote sensing because of low radiative power associated with this phase of combustion" to the following for clarity: "This residual smoldering combustion could substantially contribute to trace gas emissions but is usually excluded from FRP based fire emissions inventories because of the difficulty in detecting low FRP associated with this process of combustion."

Comment 27:

8, 3-4: To claim a difference with the studies above you would have to know proportion of above-/below-ground fuel consumption that goes with those studies.

Response: We will revisit this discussion section following our corrected results. We cannot directly compare with the overall magnitude of emissions, but we can compare with the emission ratios measured in previous studies.

Comment 28:

8, 8: This is a common error to assume that increased EFs will lead to increased, modeled health impacts. Models use EF*biomass burned to get a-priori emissions. Then the modeled impacts are compared to downwind monitors and the a-priori emissions are adjusted to best match reality. A higher EF may change the details of the tweaking procedure, but not change the downwind PM. What would change the latter is discovering a problem with the PM monitors.

Response: We appreciate the reviewer's comment, but respectfully disagree. There has been a long standing low discrepancy between fire emissions and observed $PM_{2.5}$ [Huang et al., 2013; Redding et al., 2016; Christopher et al., 2019; Liu et al., 2020]. Higher emission factors will require much less tweaking to the a-priori emissions by increasing the accuracy of the magnitude of the emissions.

Comment 29:

8, 10: "lead to"

Response: We will change "lead" to "lead to."

Comment 30:

8, 17: Towers are not completely new. There was a long history of sampling prescribed fires from towers carried out by the Fire Lab.

Response: We agree with the reviewer and added the following text to clarify our approach refers to towers in the boreal forest: "Our tower-based approach to calculate emission

factors has been used in other ecosystems, and is a technique that significantly improves our understanding of trace gas emissions specifically from boreal forest fires.”

Reviewer #2 Comments and Responses

General comments:

The discussion paper presents important research into the characteristics of wildfire emissions using established techniques, but novel analysis. Teasing apart the contributions of various fire events and the combustion stage (Flaming vs. smoldering) is a new and valuable way to understand nuances of boreal fire relevant to many needs, such as human health, carbon cycling, and smoke planning. However, the paper falls short in many ways, and will need some extensive modification to reach its potential. I strongly suggest a re-focus on a more relevant outcome from the work (rather than the fact that previous work was not catching smoldering as well as they could), a fully revised Discussion (some ideas below), and some attention to references (see notes below). This work is very important, and when presented well will make a great contribution to the literature on this subject.
Response: We appreciate the reviewers comment that this paper offers important insight into the characteristics of boreal forest fire emissions. We will systematically revise the discussion in response the the reviewer’s comments and those from the other reviewers.

Specific comments:

Comment 1:
1. The title will need modification. It is unclear what “larger” refers to – larger than what? Than previous studies (yes, but I know that only when I get to the end of theAbstract). It could be larger than flaming combustion. The point is that having an unreferenced comparative adjective can be troublesome, especially in a title where you want to be clear. The title could be the same, but with the first four words dropped: “Contribution of . . .”. Also, it is my opinion that, while this may show larger contribution than previous studies, this work has a lot of other implications and contribution that could be highlighted in the title. In some ways the community would not be too surprised to learn that the smoldering fire signal has not been captured in previous studies, so highlighting this part of it is not needed to make this an impactful paper/study.
Response: Our title will change to align with our updated calculations and their implications.

Our new title is “Boreal forest fire CO and CH_4 emission factors from tower observations in Alaska during the extreme fire season of 2015.” We chose this title to highlight that our emission factors were measured during an extreme fire year using an approach that integrates emissions from fires over longer time scales than traditional aircraft based studies. We appreciate the suggestion to broaden the implications of our study and avoid highlighting undersampled smoldering fire emissions. It is likely our new emission ratios are higher than reports in previous aircraft studies, and we discuss this in the main text.

Comment 2:

2. The comparison to previous studies would more naturally go into the discussion, rather than the introduction/background. I suggest revising to put Table 1 into the discussion where you can make the case more directly, rather than introducing the previouswork without yet seeing your results.

Response: We appreciate the reviewer's suggestion, and will bring the reader's attention to Table 1 in the introduction. We will add new discussion and analysis of the implications from an updated version of Table 1.

Comment 3:

3. There is a blatant and concerning misuse of terminology on Page 2, line 34: The sentence "Smoldering combustion can be defined as combustion with a degree ofcombustion completeness, or modified combustion efficiency, less than 0.9 [Urbanski2014]." First, MCE and combustion completeness (CC) are very different things. CC is the proportion of fuels consumed/combusted, while MCE is defined as the proportion of a gas to CO2. Second, the Urbanski paper puts MCE of 0.65 to 0.85 as "smoldering", and references Akagi et al. 2011 so I don't know where the 0.9 figure comes from. The choice of the thresholds stated on page 5 lines 1-4 need to be better justified.

Response: We agree with the reviewer that combustion completeness needs to be removed from the sentence. We have updated our criteria for separating the combustion processes to align with previous studies. The revised text now reads "The relative amounts smoldering and flaming combustion are difficult to measure, but can be estimated using the modified combustion efficiency (MCE) defined as $\Delta CO_2/(\Delta CO_2 + \Delta CO)$. Fire emissions dominated by flaming combustion have an MCE while emissions dominated by smoldering combustion have an MCE often between 0.65 and 0.85 [Akagi et al., 2011; Urbanski et al., 2014]. MCE can be used to understand the relative contributions from both flaming and smoldering fire processes."

Comment 4:

4. I found a couple of instances where the citations used are inappropriate. While I mention only 2 here, I would suspect others, so the citations need to be fully vetted for appropriateness. First: "Rogers et al. 2015" in Page 1 line 33 is not a review of borealfire regime. It may mention this, it is not what that study provides to the literature. Second: "Bertschi et al. 2003" in Page 7 line 34 is of laboratory experiments and work in savannah ecosystems, not boreal forest fires. In both of these cases, it could be argued that no reference is needed. If you do include a reference, it needs to be a paper or resource where the statement made is shown or studied, not where it was stated. I suggest the co-authors assist with improving the citations.

Response: We thank the reviewer for pointing out our errors in citations. We will thoroughly revisit the citations throughout the manuscript and make adjustments where necessary. We will edit the reference in Page 1 line 33 to include Johnstone et al. (2011), but prefer to keep the reference to Rogers et al. (2015) because although the primary goal of this paper was to highlight differences between the boreal fire regime in North America and Eurasia, it highlights the high energy crown fires that occur in the North American boreal forest. Rogers et al. (2015) is also used as a reference in section 4.1 of the discussion. In the introduction we will will make the following citation changes: change McGuire et al. (2010) to Kasischke (2000), remove a reference to French et al. (2004), replace Turquety et al. (2004) with Harden et al. (2000), and add Fromm et al. (2000).

Comment 5:

5. The discussion would benefit from more regarding the implications of the results. What is the data showing us that is relevant? Some possible ideas to highlight/discuss (these need to be discussed with co-authors, so are only representative):

Response: We plan to completely revise the main text of the discussion. The discussion will have a detailed discussion of the implications of our findings relative to past work summarized in Table 1.

a. Figure 5 (Page 6 line 9) shows a linear relationship between CH4 and MCE. Provide a short discussion of this in the discussion – what does this mean for using the data?

Response: We added the following text to the discussion: "We found a strong linear relationship between CH_4 emission factors and MCE that has also been observed in previous studies [*Yokelson et al.*, 2007; *Burling et al.*, 2011; *Van Leeuwen and Van Der Werf*, 2011; *Yokelson et al.*, 2013; *Akagi et al.*, 2014; *Smith et al.*, 2014; *Urbanski et al.*, 2014; *Strand et al.*, 2016, *Guerette et al.*, 2018]. There is a wide range of slopes between CH_4 and MCE that have been found in prior studies and could be dependent on fuel type and burning conditions [*Smith et al.*, 2014]. This implies MCE could be used as a metric for CH_4 emissions when measurements of CH_4 are not available, but care should be taken to ensure the MCE and CH_4 relationship used is for the correct ecosystem."

b. Page 6 line 22 – ". . .attributed to boreal fire emissions." – As opposed to what?? Or why? A bit of discussion on what other factors contribute to the signal, and why there are some difference in the model will help non-atmospheric modelers better understand why these results are so powerful

Response: We changed the text to read: "The forward model simulations combining AKFED fire emissions with PWRF-STILT confirmed that the elevated CO signals at the CRV tower can be attributed primarily to boreal forest fire emissions (Figure 7), as opposed to fossil fuel or other CO emissions sources. The AKFED model had a Pearson's correlation coefficient of 0.61 with observed daily mean CO and had a low bias of approximately 7%. Differences between the model simulations and observations were likely caused by errors in the magnitude and timing of fire emissions within AKFED as well as the limited spatial resolution and incomplete representation of atmospheric transport within PWRF-STILT."

c. The temporal distribution data (Fig 10) is very interesting and could be helpful for exposure assessment for health studies. (although PM, rather than CO would be of interest).

Response: We added the following sentence: "The timing of emissions is important for quantifying the impact on human health, and enhanced nighttime emissions (Figure 10) when the boundary layer is much lower could increase surface concentrations and exacerbate negative health effects."

d. Page 7, line 24: I am not sure I see a temporal trend in the old data, and I am not sure why this would be something to note. This statement is best dropped. Table 1 presents past results that are collected in a variety of settings, so (in my assessment) represents some data on the range of variability, not a record of change over time.
I hope these comments inspire the authors to revise the manuscript for a more useful.

Response: We appreciate the insight offered by the reviewer to improve our discussion. We agree the implications need to be revisited and the discussion will be significantly revised based on our corrected emission factor calculations and themes suggested by the reviewer.

Reviewer #3 Comments and Responses

General comments:

The authors present an impressive set of CO, CH4 and CO2 measurements in boreal forest fire smoke to calculate emission factors for CO and CH4. Such accurate data are neccesarry to soundly test models used to quantify the impact of big fires on the air quality and climate. Therefore, the paper is highly suitable to be published in this journal. Yet, due to error in the emission factors calculation I suggest a revision of this manuscript. Re-interpretation of results should be done by the authors before resubmission. Firstly, Eq 2 on Page 4 should be revised, using e.g. Eq. 1 and 2 in Yokelson et al. I'm looking forward to review the revised manuscript in detail. I add here only a potential useful comment. Are there any other measurements of specific tracers to be used to quantify the smoldering/flaming contributions? Flaming is likely under-represented in the used sampling height.

Response: We appreciate the reviewer's comment that our study provides accurate CO and CH_4 emission factor data needed to quantify the impact of boreal fires on air quality and climate. We agree that the manuscript will need revision and reinterpretation of results to reflect the corrected emission factor calculations. We have added text in section 2.1 and 3.2 to explain why the tower is an ideal location to measure emissions from both smoldering and flaming fires. The text in section 2.1 reads "In a previous study using the same tower, the authors conducted a sensitivity study on the CRV tower and found little influence of plume injection height on CRV tower trace gas observations [Wiggins et al., 2016]." The text in section 3.2 reads "CRV tower is sufficiently downwind to integrate both flaming and smoldering processes from fires across interior Alaska."

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