Review of Wiggins et al Boreal Fire EFs

By Bob Yokelson

General:

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.

P1, L14: define CRV

P1, L33 – P2, L2: will these aggressively lofted emissions impact tower? Run some forward/back trajectories? Vertical mixing?

P2, L6: “deadly” AQ is over-simplified

2, 13: Andreae and Merlet was updated in 2019

2, 18: The updated Andreae paper lists more than 20 studies, so there may be more worth including in Table 1.

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.

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.

2, 37: Real fires often don’t have phases - rather a dynamic mix of processes. Change “phase” to “process” throughout?

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.

4, 6: Confusing, is it just 50 minute samples with ten minutes downtime per hour?

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?

4, 32: Do these references unambiguously support 45% C? Revisit, consider reference above, and explain in detail in revised text.

Eqn (1) Sum or slope or simple subtraction?

Eqn (2) Again, notation unusual, something common should work, or explain?

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?

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.

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.

6, 9: Many references support high correlation of EFCH4 with MCE.

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?

7, 3: “emissions” to “consumption”

7, 8: 15 total previous fires sampled may be too low if you check updated compilations.

7, 9: Convection entrains some smoldering.

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?

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?

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.

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

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.

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

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.

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.

8, 10: "lead to"

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

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