General comments:

The authors mean well and have put some effort into a small series of simple fires, but unfortunately, the work is not suitable for publication. The sampling approach is not validated, the math is described in a misleading and inconsistent manner, and there is no practical application of the results even if the experiment had been done correctly.

Measurement approach: There is an important place for lab measurements in fire research. For instance, smoke data can be obtained with instruments that might not be field worthy. However, when working close to a fire, elucidation of the impact of fire behavior on emissions is only valid if it can be shown that the sampling is representative of the overall lab fire emissions for all the behavior types considered. In other words, it needs to be shown that the smoke is well mixed so that data acquired at the sampling point do not reflect a fire-behavior impact on the height at which emissions from different processes are released. As an example, Christian et al., (2004) show that temperature and mixing ratios are constant across the stack at the level where sampling occurs for their lab fires. (Prior to that test, they published results based on an optical path that spanned the whole stack.) The good mixing Christian et al confirmed was due largely to a torus surrounding the base of the stack that promotes turbulent mixing. In contrast, wind tunnels are designed to eliminate turbulence, which discourages good mixing. In fact, Christian et al considered wind tunnel measurements, but found that wind tunnel fires produced a strong vertical temperature gradient with hot gases (flaming emissions) mostly at the top of the wind tunnel and cooler gases (smoldering emissions) lower. Thus, the CO/CO2 ratio depends strongly on the point-sampling height selected. This separation of process-specific emissions likely varies strongly by fire spread mode. In other words, the author’s CO/CO2 data could be reproducible, but not be representative of fire behavior effects if the emissions are not well mixed and flaming emissions have greater tendency to rise above their one fixed sampling point for some spread modes. Without evidence that this artifact does not occur the data are not of value.

Math: Emission factors (EF) are meant to be used with fuel consumption data and fuel consumption data explicitly doesn’t count unburned carbon that remains on the site. For instance, fuel consumption is typically obtained as pre-fire minus post-fire biomass or as pre-fire biomass times a combustion factor, which is assumed to represent the percent of the fuel that actually burns. The authors are confused about this and make misleading statements about emission factors in other work. Further, they express EF both in the normal g/kg and as unspecified percentages. The authors are correct that some burned C is converted to charcoal and this is a source of a small error in some standard carbon balance approaches. However charcoal yields are generally small and should not be confused with remnants of unburned carbon. For instance, Kuhlbusch et al., (1996) noted: “The ratio of black carbon produced to the carbon exposed to the fire in this field study (0.6–1.5%) was somewhat lower than in experimental fires under laboratory conditions (1.0–1.8%) which may be due to less complete combustion.” Some of their black carbon was in the emitted particles and some in the ash, with the ash portion representing the error in the carbon mass balance method due to C in the residue. When charcoal yields are high, as in the case of purposeful charcoal production, a method to adjust the CMB for this has already been published (Bertschi et al., 2003). Mostly for some non-C trace elements in biomass such as Cl, the amount remaining in ash may be significant on a routine basis (Yokelson et al., 2008).

In addition, the author's combustion factors are quite small (70-80%) for fine fuels compared to values measured on real fires (typically 100 %, e.g. Ward et al., 1992).

Application: A serious problem is that real fires present a mix of fire spread modes (as the authors themselves state) and in any case there is no way to operationally monitor fire spread modes for all the fires of importance, especially since the majority of global biomass burning goes undetected from space (Yokelson et al., 2011). Even if single spread modes were applicable to real fires, and they could be routine measured, many other factors effect emissions interactively such as fuel geometry, moisture, RH, etc.; and wind effects on the ability of a fire to propagate are probably far more important than subtle emissions differences. I.e. wind has other impacts such as aiding fire spread in dispersed fuel, making fire control more difficult, and possibly enabling ignition of live fuels that might not burn otherwise. Wind interacts with fire-induced convection in complex ways. None of variables can be operationally monitored in complex fire environment and realistic replication of some complex fuel beds including live, moist, or large fuels etc. is probably not feasible. If the numerous variables could be controlled one at time there are likely still non-linear interactions between driving variables.

Other miscellaneous: Real fires burn with a mix of smoldering and flaming that is further not operationally available. Both main hypotheses are already in literature. Keene et al showed fire spread mode impacts MCE and countless papers have already shown that CH_4 correlates with MCE.

A few specific comments in format P, L: text

2, 4: diameter?

2, 14: twice as much CO as what?

4, 1: Actually there are an infinite number of possible angles, they are normally mixed, plus any real fire has multiple wind directions.

4, 20-24: There is no way to operationally monitor fire spread modes and in fact the majority of global fires go completely un-detected, plus no single fire spread mode applies to a whole fire.

6, 1: all gas sampling at one height – no evidence well mixed for all fire types

7, 15: windspeed of 1.5 m/s or ~5 km/h kind of low

Pages 9-11: un-needed lengthy discussion of old math, plus a misprint in eqn 7

14, 21-24: “Fire spread mode had a statistically significant effect on CO2 ($p<0.0001$), CO ($p<0.0001$) and carbon residue emissions ($p<0.0001$) but did not have a statistically significant effect on CH4 ($p = 0.269$) or N2O emissions ($p = 0.261$).” Something went wrong here because fire spread mode effects MCE and CH4 is strongly correlated with MCE and the authors claim N2O is strongly correlated with CH4.

14, 23: “carbon residue emissions” ?

15, 17-18: On the same page the authors first claim that CH4 increases during smoldering and N2O doesn’t, then a few lines below they make opposite claim.

Page 17: In general: The EF has to be multiplied by fuel consumption to get emissions!

17, 11-12: Wrong, the widely used CMB approach assumes that burned fuel carbon (except for charcoal) is emitted to the atmosphere

17, 17: If fuel carbon remains on site and is not counted as fuel consumption then the authors approach will incorrectly estimate carbon emissions.

18, 6: How can EF be expressed as a percent?

18, 12-14: Here the authors explain perfectly why their work has no realistic application, real fires, they state, have mixed spread modes.

Table 1: Does not label the fire spread modes?

Table 2: The footnote discusses comparisons that are not in the table

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