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Interactive comment on "Emission characteristics of refractory black carbon aerosols from fresh biomass burning: a perspective from laboratory experiments" *by* Xiaole Pan et al.

Anonymous Referee #1

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Summary: The present manuscript describes a series of laboratory experiments on wheat straw and rape or rapeseed plants to quantify emission ratios and mixing state of refractory black carbon (rBC) using the Single Particle Soot Photometer (SP2). The ultimate objective of this study is to provide data that can augment field measurements of biomass burning (BB) events as well as providing important BB inventory used by models. The primarily findings of value to the community include the quantification of the delta-rBC/delta-CO ratio as a function of MCE (modified combustion efficiency); dependence of rBC mass mode diameter on MCE; and dependence of rBC mixing state on MCE for the two fuel sources studied. The biggest disappoint of this study is the lack of measurements of the rBC optical properties as a function of MCE. From a cli-

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Discussion paper

mate forcing point-of-view, quantification of BB optical properties is central to bounding the contribution of these events to aerosol radiative forcing. Consequently, this study is very myopic - only two fuels are examined and the experiments carried out are rBC-centric. This being said, the data is expected to find value in emission inventories and thus should be considered for publication after comments, listed below, are addressed.

The authors correctly indicate that the combustion process is the driving force that dictates variations in emission characteristics. It is clear that the authors characterized stages of the burn as either flaming or smoldering, yet offer no boundary conditions as to when a burn was flaming vs smoldering. If MCE was used, what value determined if the data points were from an active flaming condition or smoldering condition? And, if as the authors point out, both stages could occur at the same time. On page 9, line 13, the authors state that "when the combustion shifted from the flaming dominant to the smoldering-dominant state. . ." what is the criteria used to characterize one stage over the other?

This reviewer is rather surprised that the authors fit the rBC size/mass distribution data to a Gaussian (page 1, line 19; page 9, line 27) as opposed to a lognormal. It is well-known that most aerosol distributions are skewed (e.g., exhibit a long tail at larger sizes) and thus are better described with a lognormal function (Hinds 1999). On page 9, line 28, the authors make reference to figure 2a that presumably shows an example dM/dlogDp plot. Such a plot does not exist. The authors are strongly encouraged to add this figure along with a lognormal fit.

Central in their study is the use of MCE. The authors are encouraged to read the 2016 publication by Collier et al., (Regional Influence of Aerosol Emissions from Wildfires Driven by Combustion Efficiency: Insights from the BBOP Campaign; (2016) Environ. Sci. Technol.50, 8613−8622) with specific attention to Figure 4). While the Collier paper focuses on wildfires, the dependence of aerosol emissions on MCE the authors might might this study relevant to theirs.

Page 1, Line 15: The authors cite in their abstract that “A single particle soot photometer (SP2) was adopted to measure rBC-containing particles at high temporal resolution and with high accuracy,” yet do not explicitly discuss what was “adopted” to realize the high temporal resolution and high accuracy. If the authors altered some hardward/software aspect of the SP2 that improved upon its “out-of-the-box” capabilities, then they should explicitly discuss those changes. If nothing was not done, than eliminate this statement as it is misleading.

Page 4, line 19: The authors write “ All of the biomass was stored in sealed 20 plastic bags to preserve its original state.” Sealing a sample will not prevent loss of semi-volatile materials. Have the authors accounted for this or attempted to quantify this?

Page 4, line 22: “flexible rubber hose”. This reviewer assumes that the authors mean “conductive” tubing. If so, please state that.

Page 4, line 24/25: The authors indicate that four samples were placed in humid conditions for 30-minutes to absorb moisture. How was the moisture content quantified? Why only 30-minutes? What was the goal? To ‘coat’ the fuel with some moisture or increase the moisture content of the fuel? The moisture content would be expected to potentially impact the MCE and, in turn, the rBC/CO ratio and thus better quantification would be warranted.

Page 7, line 14/15: The authors assume that the non-refractory coating possesses a refractive index of 1.5 - 0i. While likely valid, the authors are encouraged to acknowledge that while BB events are a major source of brown carbon (BrC), it is highly unlikely that shortwave light absorbing OA will absorb at 1064 nm - the laser wavelength utilized by the SP2.

Page 9, line 28/29: As stated above, Figure 2a does not exist.

Page 10, line 25 - 28. While some trends appear to be present, the lack of water content quantification limits how much can be concluded with respect to comparing dry and wet wheat straw. The authors are encouraged to address this either by estimating the change in water content that a 30-minute exposure of a 99% RH environment could create or acknowledge that the lack of water content quantification limits the quantitative comparison of dry and wet wheat straw emission ratios.

Page 11/12 and Figure 4. The linearity between the incandescence delay time and shell/core ratio is somewhat surprising. In the application and comparison of the two methods of analyzing the rBC mixing state - incandescence delay and coating thickness - was the LEO method applied to the incandescence delay time analysis? Not only will the LEO method impact the scattering signal amplitude, it could impact the scattering peak location relative to the rBC incandescence peak. Therefore, as the shell/core ratio increases evaporative losses might be expected to exert a greater impact the location of the uncorrected and LEO corrected delay times.

Page 13, line 19/20: The authors state that "The coating thickness of freshly emitted rBC particles from OBB was relatively small ($\sim$ 20 nm), and this thickness was reported to increase to 65 $\pm$ 12 nm (Schwarz et al, 2008) and up to 100 nm (Taylor et al., 2014) when they experienced transport over hours or days." The authors are cautioned here. As Schwarz et al. state: "Although the sources of these emissions are unknown, their location and season of occurrence suggest that neither BB plume is from agricultural sources, but from brush fires." Similarly, Taylor et al., interrogated a boreal forest fires. The source fuel examined by the authors is agricultural in origin, not wilfires. Therefore caution must be exercised when extrapolating to expected aging behavior using two very different source fuels. As a matter of fact, this Reviewer is not convinced of the statement "We found that the aging of particles was more important than their sources in determining the coating thickness of rBC particles." More discussion is needed to buttress this statement.

Please insert error bars on Figure 6 if possible

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