Response to reviewer #2:
We thank the reviewer for their comments on the article. We've responded to the individual comments below.

This manuscript characterizes the physical and optical properties of biomass burning aerosols transported over the Mt. Bachelor Observatory during the summer 2015. This is an important dataset and deserves to be published. This being said, I feel the analysis of the measurement data could have been better processed with appropriate uncertainty values assigned. Hence, I would recommend publication of this manuscript after mandatory revision. Below are my major comments:

1) The fact that the authors observe a low single scattering albedo and Absorption Angstrom exponent implies majority of the aerosols were black carbon (BC) and not Brown Carbon. This is corroborated by higher MCE values indicating flaming phase of combustion. So, my question is: why are the authors surprised at lack of BrC aerosols? BrC aerosols are generated from smoldering fire phase, mostly associated with peat burning. Smoldering phase is associated with very low MCE, which was not observed in this study. What the authors observed were over crown forest fires (flaming phase). This concept has to be made clear in the text and the abstract. Otherwise, the confusion that only BC is generated from Siberian forest fires would propagate in the community.
Response:
We explain why we suspect there is a lack of BrC in the Siberian events in Section 3.3 (Lines 316-330). We explain that the lower AAE values could be due to a lack of BrC initially produced by the fires due to flaming conditions. We also suggest that the low AAEs could be due to the loss of BrC during transport through photobleaching, volatilization, and aerosol-phase reactions. We were not able to calculate MCE due to the long transport times, low CO2 enhancements and high background. We do state in the abstract (Line 23), results (Lines 279-283), and conclusions (Lines 422-427) that we suspect the Siberian events represent a selective portion of the fire plume that is more likely to represent flaming conditions.

2) The abstract and the text says "aerosol light scattering and absorption" were measured. Please specify what parameters were measured, scattering and absorption cross-sections or coefficients? I am assuming the authors measure coefficients.
Response: We measured scattering and absorption coefficients. We have clarified this in Line 21, 89, 106, 118, and 203.

3) The scattering and absorption coefficients were adjusted to desired wavelengths using Ansgtrom exponents calculated by other studies. Could the authors specify the values used to extrapolate? Reading Fisher et al (2010), it seems the SAE values ranged between 2-2.8? What's the rationale behind using this range? Why not use 4 instead? Since all particles are in Rayleigh regime (sub-micron), their scattering cross-sections decrease in power-law exponents of 4 with increasing wavelength. So, why did the authors adopt SAE of 2.4 and not 4?
Response: The SAE values were calculated for each 5-min average using the scattering coefficient measurements at 450 nm and 550 nm. We then used this SAE for each 5-min period to adjust the scattering coefficient measurement at 550 nm to 528 as per equation 1. This was

done so we could calculate single scattering albedo from scattering and absorption measurements at the same wavelength. This has been clarified in the manuscript in Lines 142-152.

4) Figure 5 doesn't make any sense to me. Could the authors provide any physical explanation behind the correlations? Scattering in the Rayleigh regime goes as square of particle volume, which probably explains the poor correlation. But what about the others. If one cannot explain or even hypothesize the reason behind a plot, why put it. I suggest the authors to remove this unnecessary plot from the main manuscript or move it to Supplementary Materials.
Response: Figure 5 was removed and replaced with Figure 7, which is a similar plot but with D_{pm} on the x-axis. This looks at the dependence of different variables on size distribution instead of MSE. For Figure 7 all the events were changed to the same color and not individually identified since D_{pm} was shown not to depend on transport time.
The previous plots showed a correlation between MSE and σ_{scat}, PM1, and CO, all of which can be thought of as surrogates for plume concentration. Since σ_{scat}, PM1, and CO are all correlated with size distribution (Figure 7), the correlations with MSE are likely just a function of particle size and cause no causal effect. In summary it seems the more concentrated the BB plume (higher σ_{scat}, PM1, CO) the larger the size distribution, which in turn increases the MSE.
Given this, we decided to highlight the correlation between σ_{scat}, PM, CO versus Dpm instead of MSE.
Language regarding the new figure is in Lines 350-357, and 390-398.

5) Please provide an error analysis of the techniques used to measure absorption and scattering coefficients. Uncertainties involved during calculation of SAE, AAE using previously published data should be mentioned. A paragraph on error analysis is a must for this kind of study. I would further suggest to propagate these values to the error bars in figure 4.
Response: An error analysis was completed. Precision and total uncertainties were calculated for all of the optical measurements and provided in Table S1. We added description of the error analysis in the Methods Section. Lines 212-128 for aerosol scattering, Lines 135-142 for aerosol absorption, Line 156 for AAE, Lines 185-198 for enhancement ratios.

6) The manuscript has grammatical and typographical errors. I suggest a thorough editing done to the contents during its revision.
Response: We went through the paper and corrected any grammatical and typographical errors. We also checked for consistency.