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Comment on acp-2022-167

Anonymous Referee #3

Referee comment on "Intensive aerosol properties of boreal and regional biomass burning aerosol at Mt. Bachelor Observatory: larger and black carbon (BC)-dominant particles transported from Siberian wildfires" by Nathaniel W. May et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-167-RC2, 2022

This paper summarizes the results of measurements made at Mt Bachelor over a roughly 2-month period. A variety of instruments were used to determine dry particle optical properties (absorption and scattering, each at 3 wavelengths), and particle number size distributions for particles in the diameter range of 30 to 600nm. Over this period, 13 biomass burning events were identified. The likely regions of wildfires producing the events were determined by back trajectory analysis, and properties of the events compared between sources (or ages of smoke). A possible dust event mixed with smoke is also identified. The paper is very well written, and care is taken in producing the data and evaluating uncertainties. However, sitting at a fixed site and observing periods of smoke blow by puts limitations on interpreting the smoke events. The authors spend considerable time trying to explain possible causes for the observed differences between events and those of other studies, which in most cases is largely speculation. I recommend the authors attempt a more robust analysis that would include more support (eg, statistical or theoretical calculations) for their many hypotheses to explain the differences. Overall, the methods nor the results are highly novel, but they do provide very useful data on smoke plumes of various approximate ages in the real atmosphere; possible causes for the variability discussed throughout the manuscript needs more attention.

Specific Comments:

Line 49-50, this definition of BrC (the aerosol overall AAE>2) is highly measurement specific, it reflects the fact that most methods, such as those deployed in this study, cannot detect low levels of BrC (ie, instruments that cannot isolate BrC, have very limited spectral resolution, and have lowest measurement wavelengths at rather high values where BrC contributions are not strong, such as in this study with lowest wavelength 450nm). The authors should clarify how this affects what they specify as BrC. For example, for a given wavelength, say 450 nm and 350 nm, for various AAEs, what fraction of the light absorption coefficient is due to BrC, and include an uncertainty in this ratio that considers the effect of the fit (ie, the assumption of a power law and the specific two wavelengths used, and the variability in pure BC AAE). Finally, does this method of determining contributions of BrC depend on the assumption that BrC falls on a continuum from weak to strongly absorbing BrC, see (Saleh, R. (2020), From Measurements to Models: Toward Accurate Representation of Brown Carbon in Climate Calculations, *Current Poll. Rep.*, *6*, 90-104). The point here is that there are subtleties and limitations in determining BrC based simply on AAEs – which should discuss.

Does the SAE and AAE calculated depend on the specific 2 wavelengths selected for the calculation. Eg, how much would they vary if the other two pairs were used in the calculation?

How much mass is missed by particles smaller than the OPC lower size limit? This could be determined by estimating the mass from the SMPS.

Line 178, at what wavelength was the single scattering albedo determined at?

Lines 275 to 295 is largely speculation since the NEMR's at the sources are not known and NEMRs from only 2 other studies are used as a contrast. I suggest this analysis be changed by providing an in-depth discussion of various dPM1/dCO recorded close to wildfires from as many data sets as can be found, and then compare that and the variability to the data from this study. The authors could then more quantitatively assess if they are observing differences in processes or if it is hard to say bases on variability in emissions that have been recorded. I would point out that dOA/dCO could likely be used as a surrogate for dPM1/dCO since most of the mass is OA, as the authors state in the Introduction. This will likely significantly expand the published data that can be used given all the recent aircraft missions studying wildfires. Over-interpretation of the data is common throughout this paper. Broad statistical comparisons, such as shown in Fig 2 are more convincing than speculation. Can a Fig similar to Fig 2 be made for dPM/dCO?

Lines 329 to 333, the interpretation here is that differences in observed BC/CO can be used to infer BC/CO at the sources (ie, flaming vs smoldering). Again, this is speculation. It may be one possible reason but there could be others, such as differential loss of BC relative to CO during smoke transport (contrast the typical lifetimes of BC and CO if precipitation is encountered).

Ling 346-347. The authors might want to note a contrasting paper, Dasari, S., A. Andersson, S. Bikkina, H. Holmstrand, K. Budhavant, S. Satheesh, E. Asmi, J. Kesti, J. Backman, A. Salam, D. Bisht, S. Tiwari, and Z. H. O. Gustafsson (2019), Photochemical degradation affects the light absorption of water-soluble brown carbon in the South Asian outflow, Sci. Advances, 5, eaau8066.

Fig 4 and associated text. The correlations and slopes shown depend on two points out of 8 or so. Can one infer from this that there is a general relationship here? Provide statistical proof.

Line 459-460. This last line sounds like the authors are making a generalization based solely on two observed events. Is that reasonable? It is rather unfortunate that no filters were collected that could be used to measure dust (Ca2+) and smoke tracers (K+, along with the measured BC) in the same event. This would provide proof that the plumes were indeed mixtures of smoke and mineral dust, the analysis presented is only suggestive. (This was noted on line 490, but I would point this out earlier in this discussion.

Section 3.6 title and text within, define exactly what type of size distribution is being discussed, ie number distributions.

Lines 480 and on, would not a calculated volume distribution provide better evidence for a possible dust influence in the SMPS measurement size range. Ie, one could compare the shape of volume distributions for the non-dust and speculated dust events.

Lines 491 to the end is mainly speculation. Why not estimate the lifetime of an UF particle based on the measured number distributions to support this discussion.

In conclusions, bullet 2. What is meant by little BrC. (See earlier comment). Given the very insensitive method the authors used to determine BrC, this is rather a subjective statement. Same applies to the term little remaining…. The point is, using the method for determining BrC in this paper, exactly what fraction of the light absorption at a given wavelength (the authors may choose) is due to BC vs BrC and include an uncertainty. Maybe use this instead of the term little.