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Comment on acp-2022-459

Anonymous Referee #1

Referee comment on "Examination of brown carbon absorption from wildfires in the western US during the WE-CAN study" by Amy P. Sullivan et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-459-RC1, 2022

This paper presents results on aerosol BrC and various other species measured in smoke plumes from the NCAR C130 research aircraft during the WE-Can study. Online measurements of only water-soluble BrC were compared to BrC inferred from a PAS that measured particle light absorption at 3 wavelengths. Comparisons to other smoke species, such as potassium and levoglucosan are also made. Many of the findings of this paper are similar to published results by other investigators, confirming the earlier work and providing a more expansive view. A common finding of this and other recent studies is the high level of variability in BrC evolution between different smoke plumes. These findings seem to contrast with a paper published (Palm et al) by some of the same co-authors based on data from the same study, which suggests that the evolution of OA and BrC is largely described by a balance between formation and loss resulting in little change over time. Why is there (or is there) a difference in the data interpretation within the same study? As noted below, the findings of this paper should be put in context with those of the Palm analysis. Overall, the results presented here are pertinent to the current interest in BrC from wildfires and suitable for publication in this journal.

Specific comments:

Line 265-268: Might want to discuss issues with ignoring lensing of BC, see Pokhrel. What about the other assumption of no BrC at 660 nm? Does this matter in the following analysis?

Give the particle size range measured by the UHSAS and what size range was used to calculate overall particle mass concentration.

What is the cause of the large difference in correlations in Figs 2a vs 2b? The poor correlation in 2b is somewhat surprising and suggests the AAE was highly variable. Is this because 2a is data from one fire and 2b is from several different fires? This difference in correlation between the two flight is seen throughout the analysis. This deserves more investigation. If it is caused by high variability due to smoke from different fires for RF11, then maybe a more uniform plume should be used, although this flight does provide a contrast.

Slopes could be included in Fig 4 plots, which is the MCE.

Why not write equation in line 348 as: UHSAS/(1.6 WSOC)?

Line 349, what properties of WSOC and WIOC are assumed to be same? It appears the assumption is that the MCE is assumed the same for both, and that the total mass = total OA mass.

Fig 5a and b are somewhat confusing. The x-axis is measured WS absorption and the y

axis is the Mie calculate for WS and total. What data was used as input to the Mie calculation in each case, for water soluble was it the MAE for soluble species and for total the MAE total calculated previously from the WS species?

What would happen if the same calculations were repeated for different wavelengths?

Lines 474 to 484, on the discussion of the evolution of smoke plumes. The results of this paper seem to contradict a study already published (Palm et al) based on these same (WE-CAN) data and which there are common co-authors to this manuscript. Palm concludes that although changes in the OA may be occurring, there tends to be a balance, so parameters like OA mass and BrC relative to CO remain steady as the smoke plume evolves. The authors should contrast their findings to this paper, since it seems to be two different interpretations from the same study with common co-authors. Additionally, the Palm paper should be cited and findings discussed in this papers Introduction. (Palm, B., Q. Peng, C. D. Fredrickson, B. H. Lee, L. A. Garofalo, M. A. Pothier, S. M. Kreidenweis, D. K. Farmer, R. P. Pokhrel, Y. Shen, S. M. Murphy, W. Permar, L. Hu, T. L. Capos, S. R. Hall, K. Ullmann, X. Zhang, F. Flocke, E. V. Fischer, and J. A. Thornton (2020), Quantification of organic aerosol and brown carbon evolution in fresh wildfire plumes, *P. Natl. Acad. Sci.*, *117*(47), 29469-29477.)

In Fig 10, trends for different flights can't be discerned, maybe add regression lines for each flight? Also, do a regression for PAS total BrC vs dCO to support the claim that there is a consistent drop in the first 2 hours (eg, on what bases was this conclusion reached)?

Maybe look at change in WSOC relative to CO to check specifically for production or loss of WSOC with plume age?

Conclusion 2, last line, give wavelength for the ratio of WS BrC to total.