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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influx of African biomass burning aerosol during the Amazonian dry season through layered transatlantic transport of black carbon-rich smoke" *by* Bruna A. Holanda et al.

Anonymous Referee #1

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Manuscript: Influx of African biomass burning aerosol during the Amazonian dry season through layered transatlantic transport of black carbon-rich smoke (Holanda et al.,)

Referee Review of manuscript

The current manuscript discusses observations during a single research flight of two biomass burning layers off- and on-shore of Brazil, where the upper layer was characterized by very high black carbon (BC) number concentrations while the lower layer by a much lower BC concentration. Using back trajectory calculations, the high BC layer was identified as originating from the African coast. Using this finding, the authors examined the long term data collected at the ATTO site to identify that the early

part of the Amazonian dry season tends to be influenced by the long-range transport of the African biomass burning emissions while the late dry season is dominated by local burning. One of the core findings of this paper is that the SSA (at 637 nm) during the early portion of the dry season (i.e., influenced by the African plume) is nominally 0.85 while local BB emissions are found to be nominally 0.9. This finding suggests that optical and microphysical changes with the African BB plume occur during transport. The information and analysis contained in this manuscript is certainly worthy of publication, but, as discussed below, additional analysis that could be done with available data needs to be done to help explain underscore these findings. Therefore, it is recommended that the manuscript be revised and resubmitted.

I read this manuscript with great excitement as long-range transport of biomass burning emission is a subject area that has not received much study. However, upon reading this manuscript I felt cheated. Combining the aircraft data with the long-term ATTO dataset, the authors make a very compelling argument that Amazonia is indeed influenced by African fires, but do present any further data analysis that might provide the community with a deeper understanding of what processes might be present. In the abstract, the authors correctly point out that the "microphysical properties, spatiotemporal distribution and long-range transport" BC aerosols are "not well constrained". But as highlighted below, the authors seemingly have the data to help probe this, but did not analyze that data and, in turn, are missing the opportunity to further strengthen their manuscript. A quick perusal of the Wendisch et al., BAMS article reveals that the HALO payload included the two SP2s, one three-wavelength PSAP, a single wavelength PSAP, UHSAS, and a C-ToF-AMS. (This reviewer is quite surprised to learn that a nephelometer was not part of the HALO payload.) These available datasets open up additional analysis that would greatly strengthen and help elucidate properties and processes. For example, the authors could very easily combine the PSAP absorption coefficients with the SP2 to derive the mass absorption cross-sections for both layers. This would inform us about how different the optical properties are between the plumes. Furthermore, using the SP2 the authors could probe the black carbon mixing

state (e.g., coating thickness). This reviewer suspects that this analysis will reveal that the coating thickness for the African plume will be thinner than that derived for the local BB plume - which would further explain why the ATTO-site derived SSA is 0.85 for the African plume and 0.90 for the local BB plumes. Also, sticking with the SP2, why not examine the BC size distributions? What do the differences - should be they exist - tell us about the two plumes? Continuing with this theme, the authors have the UHSAS which could be used with the SP2 to examine (and compare) the two size distributions. As referenced above, the authors cite the paucity of BC microphysical properties data, yet seemingly do not take advantage of the readily available data.

Sticking with the theme of missing an opportunity, the authors chose to center their discussion on number concentration: higher number fraction of BC particles in the African layer versus the measured number fraction in the local layer. The availability of the C-ToF-AMS, along with the SP2, enables the authors to derive a more meaningful mass fraction of BC in both layers. Additionally, the AMS provides a measurement datastream that can provide aerosol composition, yet is not taken advantage of. (It is interesting to note that the authors make a passing reference to the AMS on Page 13, line 424 where they cite the sulfate content concentration.) A plume advecting for 10 days from the African shore to the South American shore should exhibit differences with that observed for a localized ("fresh") plume. It is because of the available datasets outlined in this paragraph and the one before it, that this reviewer felt cheated in that so much more could be derived from the aircraft dataset and thus provide a much more complete story. It is recommended that the authors mine their data for more nuggets of gold that are surly there.

Other specific issues:

Page 2, line 46. The authors state that the BC particle number fraction is $\sim$ 40%. I think this is wrong and that the value is closer to 30%. Using number concentrations reported on page 12 (lines 371, 372, and 383, N_CN,20 = 970, N_acc = 850, and N_rBC = 280 particles/cc) the fractions I derive are 0.29 (using N_CN,20) and 0.33 (using N_acc).

As a rough check, derived values from figure 4 support the 30% fraction vs 40%.

Page 3, Line 73 -75. The authors are reminded that semi-transparent coating also alter BC optical properties.

Page 5, line 138-139. The authors refer to the African latter as pollution, yet their discussion centers on biomass burning. Pollution tends to suggest an anthropogenic contribution. It is suggested that the authors that consider changes "pollution" to biomass burning or something that describes the layer as containing BB emissions.

Page 7, 209- 211. The authors might want to examine the paper by Adachi et al., (AST, 52, 46-56, 2018) where these authors present data from a mid-latitude wildfire that exhibit thermal stability well above the 250 C employed in the TD. How would increased thermal stability alter conclusions derived in the present analysis?

Page 8, line 250-252. In deriving the EnRbc ratio, is the CO background corrected? I assume so, but this should be explicitly stated.

Page 11, line 337. Please be consistent with respect to referencing black carbon. Within the SP2 community, the "black carbon" that is detected by this instrument is referred to as rBC (refractory black carbon). Indeed, in many places the authors refer to "rBC". To help minimize confusion, please be consistent and use only one term.

Page 12, 389- 390. The authors write "The high frBC further agrees with the pronounced brownish color of the visually observable layer in Fig. 2." This is a very misleading sentence. The brownish color could easily be due to brown carbon (BrC). Which, given the fact that the HALO payload included a three-wavelength PSAP, could be readily calculated via the Angstrom absorption exponent (AAE).

Page 21, line 678-681. As highlighted above, the HALO 3-wavelength PSAP would provide insight into the presence of BrC.

Page 37, Table 2. Please explicitly cite the wavelength used to derive the SSA (637 nm).

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