Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-333-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Absorption closure in highly aged biomass burning smoke" *by* Jonathan W. Taylor et al.

Anonymous Referee #2

Received and published: 25 May 2020

This study presented black carbon (BC) microphysical properties and aerosol absorption over the southeast Atlantic Ocean during the CLARFY-2017 aircraft campaign. The authors showed that BC particles have high values of mass absorption coefficient (MAC) (∼20 and ∼15 m2 g-1 at the wavelength of 405 and 514 nm, respectively) and absorption enhancements (∼1.8) during the campaign, and these results suggest the importance of the lensing effect by coating species. The contribution of brown carbon (BrC) was estimated to be ∼10% from observed absorption Angstrom exponents (AAE) at three wavelengths. The authors also made an absorption closure analysis through the comparisons of the observed MAC and AAE values with the calculated values using the Mie theory and empirical parameterizations. The authors clearly showed that the calculations by the Mie theory (homogeneous grey mixture and core-shell as-

sumption) cannot reproduce all the observed features of MAC and AAE, while they are reproduced reasonably well by some empirical parameterizations.

The scope of this manuscript is will suited to ACP. The topic of this study is very interesting because the accurate understanding on the microphysical and optical properties of BC particles is key to improve our estimation of aerosol impacts on the global climate. The manuscript is written very well, and the uncertainties and implications of the data are discussed in detail. This manuscript should be published by ACP after revising some minor points.

Minor comments:

1) Page 1, Lines 3-4: Highly aged biomass burning plumes

The information of "4-8 days from sources" may be useful for readers.

2) Page 1, Line 12: MAC of BC

I suggest to add the values of BC MAC here (at least for the visible wavelength).

3) Page 2, Line 18:

I think 40% is too high. CMIP6 emissions (for the year 2010) are ∼10 Tg y-1 for total BC and ∼8 Tg y-1 for anthropogenic BC.

4) Page 4, Lines 9-13:

These sentences describe what the authors did in the manuscript. I think the authors can clarify the objectives of this study here (e.g., investigate the absorption closure between microphysical and optical properties of BC for highly-aged biomass burning plumes).

5) Page 4, Section 2.2:

Please provide the particle size ranges observed by the SP2 and PAS. The SP2 could measure most BC particles in the atmosphere? The size range of SP2 is consistent

with that of PAS? Their difference can affect the results and conclusions of this study?

6) Page 4, Lines 12-18:

Please clarify how AMS and CO data were used in this study.

7) Page 6, Line 7:

Delete “to”.

8) Page 7, Line 6:

Kondo et al. (2011), which showed MMD for biomass burning plumes, can be cited here.

9) Page 7, Line 25: The ratio of observed MAC to the values by Bond and Bergstrom (2006)

I think the uncertainty in the values by Bond and Bergstrom (2006) should be considered in the Eabs estimation (1.8 $\pm$ “uncertainty” is better).

10) Page 7, Lines 22-28:

Please add MAC values at the three wavelengths to this (or related) paragraph. I think MAC values themselves are important.

11) Page 8, Lines 6-7:

The particles size ranges are consistent between SP2 and AMS? This should be considered in the uncertainty in OA MAC.

12) Section 3:

The authors should note the importance of aerosol water to MAC and Eabs in this or discussion section. The values in this study are for dry conditions, but MAC and Eabs in the real atmosphere (ambient RH) are important in evaluating their climate impacts.

13) Page 8, Lines 31-32:

It is better to describe the optical models and parameterizations used in this study briefly in the main text.

14) Page 9, Lines 14-23:

This part (steps 1-6) is not easy to understand. How about adding a figure to explain this process?

a) Please clarify steps 1-2 are theoretical calculations and steps 3-6 use observed data.

b) Please explain what is the Liu et al. correction.

c) The spherical-equivalent core in step 4 is the same as the spherical-equivalent DBC in step 3? Step 4 is to calculate shell diameter only?

d) “Convert the single-particle data to equivalent MBC and MR”: I think MBC is calculated in step 3. So, step 5 is to calculate MR?

e) Step 6: this is not easy to understand unless readers read SI.

15) Page 10, Lines 9-12:

This part should be explained near the explanation of the 6 steps.

16) Page 11, Line 19:

The core/shell Mie model (green lines in Figs 6 and 7)

17) Page 11, Line 30:

Please explain briefly what is the “skin depth effect” here (though they are described in SI). Some papers should be cited.

18) Page 12, Lines 8-10:

I suggest the authors to add observed AAE for BC only to Fig 7a and 7c (like Fig 6a and 6d).

19) Page 12, Lines 11-12:

Is it not possible to show the kBC dependency of MAC and AAE for the parameterizations? Please explain why the results are shown at a kBC value for each parameterization.

20) Page 15, Line 32:

RI should be changed to refractive index.

21) Text S2, Line 3:

do not “correct”.

Kondo, Y., et al. (2011), Emissions of black carbon, organic, and inorganic aerosols from biomass burning in North America and Asia in 2008, J. Geophys. Res., 116, D08204, doi:10.1029/2010JD015152.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-333, 2020.