Title: Radiative Heating Rate Profiles over the Southeast Atlantic Ocean during the 2016 and 2017 Biomass Burning Seasons
Author(s): Allison B. Marquardt Collow, Mark A. Miller, Lynne C. Trabachino, Michael P. Jensen, and Meng Wang
MS No.: acp-2020-106
MS Type: Research article
Iteration: Initial Submission
Special Issue: New observations and related modelling studies of the aerosol–cloud–climate system in the Southeast Atlantic and southern Africa regions (ACP/AMT inter-journal SI)

Overall The authors present a topic of great interest and importance. The authors present simulations of heating rates due to aerosol layers under clear sky and cloudy conditions over Ascension Island and discuss whether these are representative for the entire South-East Atlantic domain.

We thank the reviewer for the time spent and helpful feedback.

Major comments
Overall, the analysis over Ascension Island seemed adequate, although the final assessment that BC is responsible for most of the SW absorption in this location is a bit of a stretch. This is due to the fact that the calculations heavily depend on SSA values, as shown in figure 6 of the manuscript and Table 1. For example, comparing the values contributed by BC only from Fig. 7 to the values in Fig. 6 depends on the SSA assumptions used, where no correction/RH corrected values indeed will give the impression that BC contributes the majority to the heating rate, while if using SSA adjusted for BrC absorption makes BC contribution about 25% (at least by comparing the color scales of the two plots). This is also stressed in their text (and contradicts their conclusions): in lines 15-20 page 9. There is no conclusion to which of the heating rates calculation in Fig. 6 is the closest one to reality, which might affect the final conclusions. Maybe there are some days where the British CLARIFY aircraft had valid profiles that can support one of these assumptions.
An effort has been made to clarify the conclusions on the contribution of biomass burning aerosol to the SW heating. Unfortunately, we were unable to find data from CLARIFY to validate the results over Ascension Island.

Moreover, the heating rate calculations along the 7-day trajectory from Ascension are not fully clear for some reasons:
(1) the calculation procedure is not clear, e.g. does the profiles were taken per each lat/lon along each of the 27 ensemble or whether there was one profile compiled per trajectory. If the latter is correct, then further explanations on the calculation and assumptions is needed.
The profiles were taken for each lat/lon along the 27 individual trajectories. This has been made clearer in the text.

(2) the SSA values selected/assumed over the SEA Ocean, in compared with the values assumed for AI need further elaboration. The authors first claim that MERRA-2 and AI SSA values do not match well, but thereafter claim that over the SEA Ocean they do match (following Shinozuka et al., 2019 analysis). Indeed, in the lower FT it seems that the GOES model (underlying MERRA-

2) is able to simulate SSA well (although the current paper talks about 0.92 for SSA over AI, where over the SEA GOES is withing 0.80-0.86 in the lower FT according to Shinozuka et al., 2019, however is underestimating in the mid-FT. The question is which SSA was used then for the vertical profile calculations? Also, it would be of great help to the reader to state the SSA values, both for AI and their MERRA-2 compared values and over the SEA Ocean, since trying to understand which MERRA-2 values compared well with which location was a bit difficult. I am not sure how the lower FT SSA values over the ocean are different than AI values for MERRA-2 and why.
As the aerosol plume is transported across the ocean, in reality, black carbon becomes coated with organic carbon, forming tar balls with optical properties that change the more the aerosol ages. This is not represented in MERRA-2, given the lack of brown carbon. Closer to the African coast, there is less time for the aerosol to age, and therefore, the optical properties are likely more similar to black carbon. By the time the aerosol reaches the island, physical properties of the aerosol have changed considerably. The results demonstrated by Shinozuka et al., 2019 demonstrate this. GEOS had excellent agreement with the ORACLES observations for SSA along the African coast, however, struggled closer to Ascension Island. A sentence has been added to the text reflecting this.

Also, the paper is a bit hard to follow and would benefit from additional editing.
The manuscript has undergone substantial changes that hopefully make it easier to follow.

Minor comments
Page 4, line 6, Cimel and not Cimen
This has been fixed.

Page 4, line 9, cloud effective radii (radii is missing)
This has been fixed.

Page 6, lines 10-12, why AMF1 and Aeronet Cimels are so different?
There are a few reasons the AOD from the AMF1 and Aeronet are not identical. 1) The AMF1 AOD is actually from an MFRSR, not a Cimel sun photometer. 2) The Aeronet site is located at the airport on the eastern side of the island, while the AMF1 was stationed at a higher elevation of the southwestern side of the island. 3) Given the different instruments and institutions (NASA for Aeronet, DOE for AMF1), there are different processing algorithms that were used to generate the AOD, though this is not likely the leading cause for any differences.

Fig. 2, reduce x-axis font
The font size has been reduced as suggested.

Page 6, line 24, are there evidence of volcanic dust (in the form of size distribution, AE etc.?) during some of the days?

We cannot say for sure. There is no instrument at the AMF1 site that can measure coarse mode aerosols or determine their chemical composition. There was a Proton Transfer Mass Spectrometer (PTR-MS) deployed at the AMF1 site during LASIC, but data from this PTR-MS have not been processed and there is no plan to do so in the near term according to the instrument operator. Absorption by volcanic aerosols would be present in the aerosol radiation measurements made by the PSAP (Particle Soot Absorption Photometer) at the AMF1, but there would be no way to separate the absorption from biomass burning aerosol from that volcanic aerosols. The second author of this study calibrated sun photometers on multiple occasions at the Mauna Loa Observatory in Hawaii, which lies at 3400 m in an extensive volcanic field subject to high winds, but no evidence of volcanic aerosol was found in these observations. While we cannot be completely sure that such aerosols are not present at the AMF1 site, we suspect that appreciable concentrations are unlikely given the windward location of the AMF1 and that there is active volcanic activity on Ascension Island.

Page 7, lines 8-10, August 2016 (Fig. 3a) shows some contribution from the west, over the ocean as well as from the continent.
With the exception of one day (which probably has an incorrect trajectory due to errors in the MERRA-2 wind field), the trajectories that originate over the Southeast Atlantic Ocean do pass through the interior of the African continent.

Page 8, line 5, please state which observations you are referring to.
A reference has been added to this line for the observations.

Page 8, line 20, aerosols in the (in is missing)
This sentence has been updated.

Fig. 6 and 7 might benefit from a similar colorbar (same max-min values) or maybe a plot that shows the accumulating percentages of BC and the other aerosol to the total might be clearer here?
We now have a figure that shows the percentage of heating due to black carbon instead.

Page 9, lines 8-9, it is unclear why the relative humidity scaled MERRA-2 values were chosen here and not the BrC scaled one?
The RH scaled SSA was chosen to present the middle of the road scenario, which is now noted in the text. Additionally, SSA observations are only available at the surface. Assumptions had to be made that the vertical profile of SSA was representative of the observations. By using the RH scaled scenario, we have a true observationally based correction.

Table 1, there are no italicized values in parenthesis?
The italicized values were in a previous version of the table however the caption was never updated. This is now fixed.

Page 10, lines 3-4, please rephrase
This sentence has been rewritten.

Page 11, lines 7 and onward: please elaborate on the heating rate calculations for the trajectory analysis; as stated above, this is unclear.
This section has been rewritten.

Page 12, line 20, the conclusion here contradicts the statement in page 9, lines 15-16.

The statements that were on page 9 have been adjusted following the new figure showing the percentage of heating due to black carbon.