Title: Radiative Heating Rate Profiles over the Southeast Atlantic Ocean during the 2016 and 2017 Biomass Burning Seasons
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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.

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.

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. (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.

Also, the paper is a bit hard to follow and would benefit from additional editing.

Minor comments

Page 4, line 6, Cimel and not Cimen

Page 4, line 9, cloud effective radii (radii is missing)

Page 6, lines 10-12, why AMF1 and Aeronet Cimels are so different?

Fig. 2, reduce x-axis font

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

Page 7, lines 8-10, August 2016 (Fig. 3a) shows some contribution from the west, over the ocean as well as from the continent.

Page 8, line 5, please state which observations you are referring to.

Page 8, line 20, aerosols in the (in is missing)

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?

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?

Table 1, there are no italicized values in parenthesis?

Page 10, lines 3-4, please rephrase

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

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