Review on "Radiative Heating Rate Profiles over the Southeast Atlantic Ocean during the 2016 and 2017 Biomass Burning Seasons" by Collow et al.

This study attempts to quantify the contribution of aerosols and clouds on radiative heating rates within the atmospheric column over Ascension Island in South-east (SE) Atlantic. The approach involves the use of thermodynamic profiles and low-cloud observations during LASIC field campaign and aerosol profiles from MERRA-2 reanalysis data as inputs to a Radiative Transfer Model (RRTM). The study finds that on average, the maximum local aerosol SW heating within the column over the course of the biomass burning season ranges from 2 to 4 K per day. In addition, on days biomass burning aerosol plumes are observed above clouds, shortwave heating within the aerosol plume is enhanced by about 0.5 K per day.

The quantification and assessment of the aerosol radiative heating rates utilizing the LASIC campaign data is novel, and is definitely of interest to the scientific community. However, reporting just the radiative heating rates appears to be an underutilization of the modeling tools and observational data that the authors currently use. The manuscript could improve from clearly stating the scientific questions authors want to address to better understand the aerosol-radiation-cloud interactions over SE Atlantic, elaborating on their current findings, and evaluating additional metrics to quantify the aerosol effects on radiation at the TOA and surface, such that these estimates can be easily compared to previous studies over SE Atlantic. I have following comments (both major and minor) and suggestions for edits.

Major comments:

1. P3, L9-22: a. The authors claim that "Aerosol impacts on cloud properties resulting in changes in the cloud radiative properties, i.e. aerosol indirect effects, will be captured through the observed cloud properties", yet there is not enough discussion on this topic later on in the results section, especially from the perspective of "indirect effects". One would expect some analysis of the observed cloud microphysical properties to assess the cloud adjustments due to the presence of aerosols. I suggest either removing this sentence from objectives or adding some analysis and discussions to address this topic.

   b. "heating rates are explored along a back trajectory originating at Ascension Island". Please elaborate on the motivation for this part of the study and what scientific questions will this analysis address within section 1.

2. a. Since MERRA-2 thermodynamic profiles are used as inputs to RRTM for heating rate calculations along back trajectories, it would be nice to see a comparison of these variables at least over Ascension Island, where observations are available, to get some sense on representativeness of MERRA-2 thermodynamic profiles compared to the observations from AMF1 or INTERPOSONDE profiles. This is important because at several places within the manuscript, authors bring up anomalous behavior of MERRA-2, with deeper boundary layer, deficiencies in RH profiles, without actually showing comparisons with the observations.

   b. Similarly, even though AOD from MERRA-2 are readily compared to AERONET and AMF1 observations in this study, which is a column integrated and assimilated property

within MERRA-2, some comparisons of aerosol vertical structure, probably using lidar observations from LASIC or other co-located campaigns during this time would be more insightful. P6, L31 mentions that "in agreement with Zuidema et al. (2018), the black and organic carbon in MERRA-2 is located above the cloud layer, but perhaps extends higher in the atmosphere than indicated by lidar observations." Can the authors please clarify which Figure within the specified reference are they alluding to?

3. The authors mention some recent modeling studies, e.g. Chang and Christopher (2017) that used similar techniques/modeling tools as the authors to estimate the aerosol radiative heating rates, as well as direct radiative effects (DREs) of absorbing aerosols at the TOA and surface over SE Atlantic. Therefore, this study could benefit from calculating these additional estimates of DREs at TOA and surface, such that they can compare and contrast the differences in estimates based on the differences in assumptions of aerosol properties, clouds and thermodynamic profiles, as well as the location within SE Atlantic of the current study versus the previous studies.

Minor/Editorial comments:

P2, L15: 'lofted to between 3.5 to 4.5 km': Please verify that these heights are above ground level. Also, use of the phrase 'lofted to between' seems inappropriate. Within the boundary layer smoke is well mixed, so to put it more appropriately, 'smoke aerosols extend up to 3.5-4.5 km above ground level'.

P2, L18: 'When compared to satellite observations, models commonly allow for the biomass burning aerosol to descend too rapidly once over the ocean': This applies more to the 'global models' rather than generalizing it to all models.

P13, L9: 'impact of clouds, aerosols, and black carbon': black carbon is part of aerosols, I suggest rewording to mean all aerosols except black carbon and black carbon.

P4, L 9: ice and liquid/ice cloud droplet effective 'radius'?

P4, L 22: vertical profile of aerosols and their 'column' integrated properties?

P5, L14: 'INTERPSONDE profiles were interpolated onto the MERRA-2 vertical profile'? Replace MERRA-2 vertical 'profile' with 'levels'.

P5, L23: The model experiments need elaborate description, may be also tabulation for quick remembering. The authors need to clarify how are clear and cloudy sky cases being simulated, using what classification criteria.

P7, L 5: 'Based on prior results for the height of the aerosol plume, the parcel originated at a height of 2 km.' Please clarify, what prior results are being referred to here? Moreover, this whole paragraph is hard to follow at times, I suggest overhauling and elaborating on how "determining the origin" of aerosol plumes impacts your findings of this study.

P7, L 25 onwards: This paragraph is describing the typical MBL and cloud structure over Ascension, but it appears like a commentary on general cloud features one would observe over this region, rather than depicting these features using the observation data. Moreover, references backing these statements about cloud structure and transitioning lack appropriate referencing.

P7, L 31: authors mention, "bottom panel of Figure 4, which exhibits a subtle, intermittent sub-layer at ~900 hPa". It is hard to make out any intermittent sub-layer at 900 hPa, probably color scale of the figure needs to be improved.

P9, L 1-7: This paragraph is really hard to follow. Authors mention, "heating due to clouds, generally located below 900 hPa, is underwhelming and of similar order of magnitude as the heating due to aerosol" and refer to Figure 6d. From my understanding of Fig. 6, these depict SW heating rates due to aerosols, so I don't understand how are "heating due to clouds" are being inferred.

P9, L 6-7: "in the presence of clouds, radiative heating within the aerosol layer is embellished". Suggest rewording "embellished", as well as clarification on what do the authors mean by this term?

P11, L 20: "It is known that the boundary layer is too deep over Ascension Island in MERRA-2". How is it known, please clarify or use an appropriate reference?

P11, L 22: "SW heating due to aerosol is no longer maximized within the aerosol layer but rather at the surface". Please elaborate why would that be, it is not clear from the current discussions.

Figures/Tables:

Table 1: caption says, "Italicized values in parentheses for all aerosols are results with the decreased SSA." I do not see any italicized values in parentheses within the table. Please clarify. Also, consider spelling out M2 to MERRA-2 or explain in caption.

In general, curtain/contour plots are okay, but some sort of mean vertical profiles as line plots are required for understanding the subtle features that the manuscript points to at various instances (e.g. discussions under section 3.2)

Figure 4: Color scale needs changing, as contours are hard to distinguish. Also, can the Y-axis be limited to 400-500 hPa, so that details of the lower troposphere can be highlighted, where the interests of this study lie?

Figure 5: Figure 5b is never discussed, while 5a is barely mentioned. Either remove the figures or include discussions within the main text.