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

This manuscript presents a quantitative assessment of the radiative heating rates due to aerosols over Ascension Island based on idealized calculations through a Radiative Transfer Model, using aerosol vertical profiles and optical properties from MERRA-2 reanalysis, thermodynamic profiles, and low-cloud properties from island-based observations during the LASIC field campaign. The authors find shortwave heating within the aerosol layer above Ascension can locally range between 2 and 8 K per day, and shortwave heating due to biomass burning aerosols is not balanced by additional longwave cooling.

The presented assessment of the aerosol radiative heating rates is novel and nicely conducted, which could be informative and insightful to the scientific community if the following concerns and questions are properly addressed and justified. In addition, I find myself having hard times understanding/digesting many of the discussions in the current form of the manuscript. These confusing discussions should be reconstructed and extended with additional details and elaborations before this manuscript can be accepted for publication.

**Major concerns/questions:**

1. The current introduction section seems a little weak in terms of scientific motivations. I recommend stating a stronger scientific motivation for the study, clarifying the scientific question you want to address with this study, and elaborating more on how is your study going to help us better understand the aerosol-radiation-cloud interactions within the SE Atlantic, rather than saying that the goal is to quantify and report the radiative heating rates over Ascension.
2. You compared vertically integrated aerosol properties from MERRA-2 to LASIC measurements, but not thermodynamical properties, especially thermodynamic profiles, which could have subtle impact on the vertical distribution of biomass burning aerosols.
    a. You mentioned in Line 20 on Page 11 that "boundary layer is too deep over Ascension Island in MERRA-2," comparing to LASIC measurements? Could you show the thermodynamic profiles comparison over Ascension?
    b. You mentioned potential deficiencies in RH profiles of MERRA-2, can this also be shown as a comparison with LASIC measurements?
3. You introduced six sets of experiments towards the end of Section 2. Please specify how are they defined/selected?
    a. Are you using cloud observations to screen for clear skies?
    b. Are you simply setting cloud parameter off in the model for clear sky case, even though the inputting thermodynamic profiles felt the existence of a cloud layer? If this is the case, please discuss how is this artifact affecting your results (i.e. is your heating rates under/over-estimating?).
    c. Not all of these experiments are presented in the following results part, e.g. the clean-cloudy case is not shown.
4. Section 3 is named as "Results," even though 3.1 and 3.2 are not actual "results" of the radiative transfer calculations. Using "Results" as a section name is vague, and I suggest reconsidering the section organization, one option could be making 3.1 as a new Section 4, and 3.2 as a new

Section 5. Currently, all your results are lumped into one section (3.3), which is poorly organized, I suggest making 3.3 as a new Section 6, and break each part into subsections, e.g. SSA sensitivity, SW heating due to black carbon, SW heating enhancement in the presence of clouds, LW cooling, and heating rates along back-trajectories.

5. I am a bit lost regarding the purpose of discussing the back-trajectories, how are they (or the origin of aerosol) related to (or affect?) your heating rate calculations? Besides, some of the details regarding the HYSPLIT runs should be introduced in the Data & Methodology section. P7 L15-18 seem abrupt at the end of Section 3.1, they also fit better in the Data & Methodology section. Perhaps these last two paragraphs of Section 3.1 could be moved to Section 2 together.
    a. P7 L2, "determine the origin of the aerosol" is a strong statement, as back-trajectories not necessarily indicate the exact pathways of biomass burning plumes, and one has to combine other sources, e.g. fire emission data, in order to determine the origin of the aerosol observed at Ascension. I suggest rewording.
    b. P7 L5-6, there were aerosol both in the free-troposphere and boundary layer, why 2 km is picked? What is the prior results you are referring to, please provide the reference.
    c. P7 L9-10, why the subtropical high over the southern Indian Ocean plays a role here? Monthly mean SLPs could be added to the background of Fig. 3 to base this statement.
    d. P7 L10-14, what are these observations implicating, how are they related to this work, or affecting the interpretation of your results?
6. Regarding Fig. 4, although one can identify an inversion layer from temperature profiles (distinguishing yellow and light orange from your plot), I would argue that potential temperature is a better choice to show the cloud top inversions. I think you could also extend the discussion to the potential role of the RH plot on indicating the biomass burning smoke plumes arriving at Ascension, as RH bursts in the free-troposphere tend to colocate well with the smoke episodes arriving at Ascension.
7. You did not discuss Figure 5b at all, and only mentioning Fig. 5a for clouds that are not visible in the plot. I wonder if this figure is necessary, it doesn't seem to add much useful information, and you barely discussed it in the main text.
8. The discussion in the first paragraph of Page 9 is very confusing to me. In the first couple of sentences, you mentioned comparing heating due to clouds with heating due to aerosols, and my interpretation of Figure 6d is that this is SW heating due to aerosol under a cloudy sky, and you did not show a case for heating due to clouds alone (no aerosol), so how did you make the comparison? whereas in the last sentence you talked about comparing aerosol heating under cloudy and clear skies (isn't this contradicting to the first couple of sentence? please clarify), and what do you mean by "embellished," I had trouble relating this word to the observations.
9. I also think the discussion for Figure 6d,e,f could be substantially extended. Currently, you barely discussed them ("embellished" is all you used to describe the comparison), and I will be curious to know why SW heating due to aerosol in the BL is enhanced under cloudy conditions? This is contradicting to my intuition, as the cloud layer reflects SW back to the space, I would expect the SW heating in the BL to decrease instead of increase.
10. It seems to me that MERRA-2 is not distributing enough BC/OC in the BL based on your Figure 2, 6 and 7 (clear sky condition). I wonder if you have compared MERRA-2 aerosol vertical profiles with extinction profiles from LASIC MPL (when available) or NASA ORACLES HSRL2 profiles or UK CLARIFY EXCALABAR profiles when they become available over Ascension during the 2017 season?

11. Regarding the LW cooling associated with the SW heating, the concern I have is that the observed temperature profiles (from LASIC radiosondes) had already felt that heating, in other words, the temperature increase was already taken into account in the observed profiles. By modifying the observed T profiles, you're artificially increasing the temperatures (artificially boosting the LW cooling). I would recommend just simply turn on the LW calculation using the same observed T profiles, and see if the net radiation budget produce a cooling or a warming.
12. You should state whether this LW experiment is calculated with cloud presence or not.
13. The discussion in lines 17-32 on page 10 is particularly hard for me to digest. As we know LW cooling is always happening no matter aerosol presents or not, and the "LW cooling" you are talking about in this paragraph is the additional LW cooling caused by the increase in the temperature profiles due to SW heating (since it is done by subtracting a control run). The following points should be addressed properly in order to make the discussion clear.
    a. When you say "LW radiational cooling never offsets the absorption due to aerosols," you should make this clear that you mean the additional LW cooling never offsets the absorption.
    b. L 25, "magnitude of the LW cooling never reaches …" same problem as above. LW cooling at inversions can easily reaches 10K/day at night. The LW cooling you are referring to is the difference between the T-modified run and the control run. Please make this clear.
    c. L 23, "radiational cooling still occurs…" As mentioned above, LW radiative cooling always occurs no matter the aerosol condition. Since you are showing the difference between the T-modified run and the control run, as long as there is SW heating due to aerosol (no matter how much), T profile will be modified, and difference in LW heating will exist. This cannot be used to demonstrate that additional heating due to aerosol remains in the column, you have to use the real LW heating profile to quantify that, not the difference between two runs.

I strongly recommend re-assessing this LW part (see Major comment 11), at least the way you interpret/discuss it.

14. Regarding your case study on the back trajectory, first, please specify reasons for originating at 2 km, second, why the meteorology for HYSPLIT runs are switched to GDAS instead of MERRA-2 as you did for the monthly back-trajectories, is MERRA-2 not capable to do ensemble runs? Please justify. Then, trajectories were forced by GDAS but radiative transfer calculations were using MERRA-2 thermodynamic profiles (why inconsistent)?
15. Why is the SW heating along the back trajectory limited to below the inversion? I would expect there to be aerosol in the FT along the 7-day back trajectories, and why not showing the aerosol and thermodynamic curtain plots along the trajectories from MERRA-2? In Lines 18-20, you're saying the aerosol layer is entirely above the inversion along the trajectory, and yet, no heating above the inversion? This is very confusing, please justify.
16. In the last paragraph, you mentioned that the ultimate goal is to study how the heating due to aerosols impacts the transition of marine stratocumulus to trade cumulus, I would really love to see more discussions added to the manuscript on how will this study help towards achieving this goal. For instance, how can this study contribute to the understanding of cloud adjustments to aerosols, and what insights can this study provide on the stratocumulus to cumulus transition in the southeast Atlantic. Such discussions will substantially strengthen the scientific importance of this study.

**Minor issues:**

*Abstract*

- Line 30, you mentioned "stabilization of the lower troposphere," but this is not discussed anywhere in the main text of manuscript. I suggest adding discussion regarding this point you raised in the abstract.

*Introduction*

- P2 L21, you haven't introduced Ascension Island yet, a general reader would have no idea where the island is, near coast? or in the remote ocean? I suggest introducing Ascension Island somewhere in the introduction.
- P3 L10-15, these information on datasets belong to the Data section, seems to me.
- P3 L16-18, these sentences seem to belong to the Methodology section. I would suggest adding more motivational statements, clarifying your scientific goals, here in the last paragraph of the introduction, replacing these details of datasets and approaches.

*Data and Methodology*

- P4 L7-, Because of the location of the AMF1 site, orographically generated clouds frequent present in LASIC AMF1 cloud measurements, please address how will this feature affect your assessment and the general representativeness of your results. Please also specify the temporal resolutions of ARSCL and MICROBASE.

*Results*

*3.1 Evaluation of Aerosols in MERRA-2*

- P6 L23, my understanding of the location of AMF1 site is that it is elevated and located at the upwind part of the island, which should be representative of the aerosol condition of a marine boundary layer (minimal island effect). Besides, if indeed there were dust (more scattering) mixed into the AMF1 sampling volume, shouldn't we expect a higher SSA? (Zuidema et al. 2018b's values are lower). Please correct me if this is not the case.
- P6 L28-29, could be helpful if AODs are overlaid on top of Fig. 2.
- P6 L34, the decrease in BL height is not very evident based on Fig. 2, overlaying some other forms of indication could be helpful.

*3.2 Thermodynamic Profiles over Ascension Island*

- P7 L21, "time-height" should be time-pressure, as you're showing pressure in the vertical.
- P7 L22, cloud top inversions at Ascension are not around 700 hPa (~3 km). Please double check the pressure axes in Figure 4.
- P7 L31, I do not see a "subtle, intermittent sub-layer at ~900 hPa" based on the RH curtain plot, perhaps this will be more visible in a single-profile presentation. Based on Fig. 13 of Zhang and Zuidema (2019), the intermittent layer seems to be located at ~700 m, which is lower than 900 hPa.

*3.3 Heating Rate Profiles over Ascension Island*

- P8 L21, it would be easier to visualize this co-variability between the heating rate and the AOD, if you could add MERRA-2 aerosol contours or AOD time series in the background of Fig. 6.
- P8 L29, if it is hard to tell with the color bar, could you provide some values to indicate the difference between heating rates calculated from MERRA-2 SSA and RH scaled SSA?

- P9 L18-19, I see heating due to black carbon ~0.5 K/day extending to around 600 hPa just as in Figure 6, please re-state your argument about this observation. Besides, how do you know it is absorption from dust (isn't dust more scattering)? Please justify.
- P9 L29, please define "enhancement of heating within the aerosol layer due to clouds" in the text or in the caption, i.e. how did you quantify that, is this cloudy-aerosol run minus clear-aerosol run (Fig. 6f – Fig. 6c = Fig. 8a?)?
- P9 L30-32, "a few K per day"? the color bar on Fig. 8 only goes to 0.6, how did you get a few K per day? "…but when all aerosols are considered the majority of the enhancement is located …" isn't this true for both 'All Aerosols' and 'Black Carbon'? Please check your logic here.
- P9 L32-34, could you please extend this discussion, especially on why this BL enhancement is not apparent for Black Carbon only case, even though BC is highly absorptive?
- P10 L1-2, I think you should be careful here and say "…due to the presence of clouds…"
- P10 L9 and thereafter, you used the phrase "radiational cooling," while I am more used to seeing "radiative cooling" being used in other literatures.
- P10 L28-29, "some heating occurs above and below the aerosol," we can't tell where the aerosol layer is based on this plot, one option is to put MERRA-2 aerosol contours in the background. Another option is to show a line plot highlighting a single heating profile along with the aerosol profile.
- P10 L29, please discuss how is this redistribution of heat, as you put it, going to modify the stability of the boundary layer, as you pointed out in the abstract.
- P11 L21, please elaborate more on how the depth of BL affects the SW heating. Why minimal SW heating occurs in the last few hours of the back trajectory?
- P11 L23, please explain or discuss your speculations on why SW heating maximized at the surface here.

*Summary and Conclusions*

- P12 L12, "…greater depth of the boundary layer…" comparing to what?
- P12 L16, "local heating rates are sensitive to the thickness of the aerosol plume" this is not discussed in the results section. Which figure supports this argument? Please mention this argument when you discuss that figure.
- P12 L20, "…most of the SW absorption" please be quantitative here.
- P12 L21, please be specific about this statement, i.e. which month? over Ascension or the whole SE Atlantic? Zuidema et al. 2018b states smoke often presents in the BL of Ascension Island, more frequent than "at times."
- P12 L23-24, I think what you want to express here is that adding a cloud layer will result in an enhancement of heating. Saying "interaction between SW radiation, clouds, and aerosols" is a bit misleading, as aerosols and clouds are not interacting in your calculations. I suggest a more careful rewording.
- P12 L29, I didn't think you were trying to represent the entire southeast Atlantic using Ascension observations until I saw this statement, and I don't think this study should be used to represent the entire region. I suggest stating this clearly in the introduction or data section, that this study only represents the remote SE Atlantic, and cannot be used to represent the entire region. Also, you could change the title to "…remote SE Atlantic…"
- P12 L31, "sensitive the heating … is," sensitive to what?

- P13 L15-20, the first and third sentences are the same sentence, please double check.

**Figure/Table issues**

1. Table 1, there are no italicized values in this table, please check.
2. Figure 2, please use $10^{-5}$ instead of e-05
3. Figure 4, please double check pressure axis, you are showing a 3 km BL.
4. Figure 5, in my opinion, this figure can be removed, and all the white space above 800 hPa can be minimized.
5. Figure 6, SSA instead of "SSA albedo." Again, the space above 600 hPa can be minimized, same for Fig. 7, 8 and 9.
6. I think for Figure 7 and after, you probably should remind the reader that we should compare these results only with the bottom panel of Fig. 6 (the RH scaled one), by making a note in the caption that SSA is the RH scaled one.
7. Most of the results are presented in curtain plots, they are nice in terms of showing the whole month, but rather poorly representing details in vertical. I recommend showing couple plots with single profiles when you discuss details in vertical, especially when you discuss the relative location of heating/cooling to the aerosol layer.