Thank you for your positive review of our manuscript. We appreciate the comments and questions you provided. Below we have provided responses (in boldface) to each of the main, other, and technical comments. Line numbers refer to the original manuscript.

In this study, the authors compute heating rates associated with smoke aerosols and water vapour overlying stratocumulus clouds from aircraft measurements for 9 cases over the southeastern Atlantic Ocean. They find large variability in heating rates, because of variability in aerosol properties and cloud albedo. Water vapour contributes a sizeable fraction of total heating rates, although aerosols are the main contributor. That variability collapses when heating rates are normalised to aerosol extinction and downward radiative flux, a quantity that the authors call heating rate efficiency (HRE).

The paper is short and to the point, and well written. The analysis relies on a very good characterization of the radiative environment of the cases studied. Figures illustrate the discussion well, except for Figure 5, which does not seem needed. The definition of HRE has some potential, but it is a demanding quantity to measure. That will probably limit its usefulness in the future. I only have a few comments on the papers, which should not require additional analysis. So I recommend publication after minor revisions.

Main comments:

Line 326: Is the (extinction) AOD really the main driver of heating rates for aerosols? Isn't the absorption AOD more important? That would reconcile that paragraph with the discussions on lines 369-378.

It is indeed correct that the absorption AOD (AAOD), in other words, the absorption coefficient is a more important parameter than the total AOD (or extinction coefficient). The reasons why we chose to normalize the heating rate by extinction instead of absorption factor are: (1) the extinction coefficient profile is (or will be) more readily available from remote sensing (HSRL) than the absorption coefficient profile. The latter would require knowledge of the SSA in addition to the extinction coefficient, and that is much less constrained by remote sensing than the extinction coefficient. If the extinction-based HRE is measured in a variety of regions or air masses, it obviates the need for SSA retrievals. (2) our concept of "efficiency" is derived from the radiative forcing efficiency (the radiative effect normalized by the optical thickness), which also uses the total (instead of the absorption) optical thickness.

Beginning at line 386, we have included the following text:

"Alternatively, if in the future the absorption coefficient were available at sufficient accuracy in addition to the extinction coefficient, the HRE could be redefined to normalize by the absorption coefficient, thereby accounting for the SSA vertical dependence."

Line 368: Figure 10 does support a factor of 2 enhancement of HRE at high albedo, but isn't that slightly counterintuitive? Granted, the layer is illuminated twice, but it also extinguishes radiation so the "second" illumination has less energy. So I would have expected a factor smaller than 2.

You are correct, the enhancement factor is less than 2. In the manuscript, we have updated the text more explicitly explain your point. The new text (starting at line 366) is as follows:

From the fit line, it is apparent that the HRE increases by approximately a factor of 2 across the full albedo range from 0 to 1. This can be understood intuitively, considering that the layer at an albedo of 1 is essentially illuminated "twice" – from the top and from the bottom. Of course, the heating of the layer is not exactly doubled since the illumination from the bottom (the upwelling) is less than from the top (the downwelling) due to the partial attenuation of radiation.

Lines 385-386: I do not understand the "a significant reduction in complexity." Words may be missing, as that statement does not follow from the preceding sentence. In any case, the main point is that the assumption of constant SSA in the aerosol layer made on line 175 directly translates into a vertically uniform HRE. So the small variability in HRE may be the result of a simplification, rather than an intrinsic property of that quantity.

The erroneous phrase has been removed, thank you for pointing this out. We chose to remove the original "reduction in complexity" phrase which was included to emphasize that to translate from extinction profiles into HR, one does not need an extensive set of additional input parameters (spectral SSA, asymmetry parameter, scene albedo etc.) If the SSA were variable with altitude, the layer-by-layer HRE variability would indeed increase. However, the variability that we see even for a constant SSA is mostly related to the vertical distribution of the extinction, combined with the variability of the underlying albedo, among other factors. However, those are relatively minor.

In addition, the HRE facilitates the intercomparison of different observations regardless of the geometric layer thickness, optical thickness, and other parameters. Previously, when comparing HRs across different papers, one had to consider over what thickness the layer was spread, for example. Now, this is no longer necessary.

Other comments:

Line 222: ECMWF is not a satellite instrument. Probably a reanalysis product, so it would be better to say which one.

We have updated the text at line 222 as follows:

"Compared to Deaconu et al. (2019), who derived instantaneous heating rates from MODIS/POLDER/CALIOP satellite instrumentation and the ECMWF ERA-Interim product from June-August 2008,"

Figure 5 does not bring much. It could be deleted.

We would prefer to keep this figure as it highlights the importance of water vapor heating in conjunction with aerosol heating.

Technical comments:

Line 329: Typo “very” -> “vary”
Updated
Lines 337-338: Only one of those two sentences is needed.
Updated
Line 352: Typo “and” -> “an”
Updated