



Review of Cochrane et al.

Anonymous Referee #3

Referee comment on "Biomass Burning Aerosol Heating Rates from the ORACLES 2016 and 2017 Experiments" by Sabrina P. Cochrane et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-169-RC3>, 2021

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.
- 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.
- 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.

Other comments:

- Line 222: ECMWF is not a satellite instrument. Probably a reanalysis product, so it would be better to say which one.
- Figure 5 does not bring much. It could be deleted.

Technical comments:

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