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Interactive comment

Interactive comment on "Impact of absorbing and non-absorbing aerosols on radiation and low-level clouds over the Southeast Atlantic from co-located satellite observations" *by* Alejandro Baró Pérez et al.

Anonymous Referee #1

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The paper written by Baró Pérez et al. is exploring situations with moist aerosol layers above stratocumulus clouds in the Southeast Atlantic, during the biomass burning season. The authors attempt to separate and quantify the impacts of aerosol loading and type and humidity on the radiative fluxes. They employed observations from CALIOP and CloudSat satellites and meteorological parameters from MERRA-2 and ERA5 reanalysis, in order to analyse the meteorological effect (in the pristine cases) and the aerosol effect (polluted cases of different types of aerosols – smoke and mixed type) on the atmospheric heating rates, i.e. cloud top cooling. The paper makes reference

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Discussion paper

to the paper by Deaconu et al, 2019 which found that separating meteorology from aerosol effects on below clouds it is not achievable from observations, as the meteorology is substantially different between polluted and less polluted (clean) cases in the Southeast Atlantic. The differences in approach are that the present paper considers four years of satellite datasets (2007-2010), the distance between the clouds and the above aerosol layer is minimum 0.4 km and aerosols are classified based on their type. The authors divide their data into two periods, June-July-August (JJA) and September-October (SO) due to seasonal meteorological differences. Most of their findings are in agreement with previous studies: e.g the shortwave (SW) heating of the aerosol layer increased with higher aerosol loading, the relative humidity (RH) of the aerosol layer had a negligible impact on the SW heating rate, no impact of smoke on the underlying cloud top radiative cooling, and enhanced levels of moisture are transported within the aerosol plumes. However, they haven't found indication of a semi-direct effect of aerosols, or a relationship between aerosol loading and increased RH.

The paper is well documented and generally well written. The methodology is different from Deaconu et al., 2019, but following a similar train of thought, and their findings are mostly complementary. This paper adds value and interesting discussions to the subject of aerosols above clouds and their radiative impact on clouds. I am recommending this paper for publication after the following remarks are addressed.

Deaconu et al., 2017 showed that CALIOP V3 underestimates the AOD above clouds with a factor of 2 to 4 when compared to other methods dedicated for above-cloud aerosol retrievals. While CALIOP V4 has improved the calibration at 532 nm compared to V3, the AOD retrieval is still underestimated over ocean compared with MODIS, showing little improvement over the Southeast Atlantic Ocean (i.e. Fig 16, Kim et al 2018). In other words, the extinction coefficient and/or the aerosol layer geometrical thickness are underestimated for thick aerosol layers. How do the authors justify using this extinction coefficient to compute the radiative heating rates, without any additional scaling? Following, how do the authors justify their choice of 0.4 km between the cloud top and the base of the aerosol layer (as detached cases), considering also that CATS system shows that the CALIOP V3 algorithm probably overestimates the base of the aerosol layer by 500 m (Rajapakshe et al., 2017) – which probably stands true for V4 as well.

The authors should mention that the sole difference between polluted continental/smoke and elevated smoke is their altitude separation and that pollution lofted by convective processes or other vertical transport mechanisms can be misclassified as elevated smoke (especially that the V4 lidar ratios used in the CALIOP retrieval algorithm are identical for both situations (70 ± 16 sr at 532 nm and 30 ± 14 sr at 1064 nm)).

I am not entirely convinced by the choice of the three AOD intervals. I believe that using different thresholds for the AOD intervals for different regions and time periods is confusing. For example what you consider 'high' AOD interval for the mixed cases JJA is covering mostly the 'middle' AOD for the smoke cases SO. While a comparable number of profiles for each bin is fairly important for a statistical analysis, I find it more important to compare same ranges of values. I would suggest either reducing the number of intervals (above a threshold and below a threshold) or choosing values that are applicable to all categories (e.g. in the case of mixed type, have only 2 intervals – as it is clear the AOD values are lower than for smoke).

Is it possible that the different RH profiles between JJA and SO are associated to different meteorological conditions (e.g. wind direction from the ocean, instead from land)? Deaconu et al, 2019 looked at high and low AOD above clouds cases and found that easterlies (winds predominantly from the NE) are associated with larger AODs and larger humidity values, while the wind coming from the open ocean is characterized by low values of AOD and humidity. The author also mentions that the average monthly horizontal winds show a significant difference between the months of SO compared to JJA at 700hPa. In this paper the authors also separate AOD in different intervals and analyze the RH, but it is not clear if these cases have similar underlying meteorology, and if for all these cases the aerosol and humidity layers are in fact together transported from the land. Considering that the CALIOP Aerosol Profile Data product includes the RH extracted along the track from MERRA-2, but no information on the winds, I suggest mentioning the caveats associated to this approach when comparing these cases and the different resulting heating rates.

The results showing mean profiles of relative humidity for the three aerosol intervals for SO period are surprising. Adebiyi et al., 2015 use radiosondes measurements from St. Helen for different aerosol loading intervals (e.g. Fig 11, Fig 14) and their results show both the humidity and temperature boundary layer-top inversions strengthen as the aerosol loading above increases. Flight campaign measurements that took place in the Southeast Atlantic during the biomass burning season (ORACLES, CLARIFY and AEROCLO-sA) have also found increased moisture with increased aerosol loading. Would the authors like to comment on this?

Fig.6: Could the authors also plot specific humidity profiles? Even in a supplementary figure.

The authors mention 'no indication of a semi-direct effect'. However, there is no additional study on the cloud liquid water path and cloud optical thickness that would support this comment. Their statement is based on the results on the LW cooling rates due to RH, which, as they mentioned, shows confounding impacts due to the variability of the cloud top cooling rates. This study would be enhanced with a sensitivity test, in which the cloud top altitude would be fixed for the different aerosol types, AOD and RH intervals and time periods.

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