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Atmospheric Chemistry and Physics Discussions

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 #2

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The study examines the impacts of smoke and other aerosols species on the low-level clouds over the southeast Atlantic. The paper's main goal is to characterize the effects of aerosol loading, aerosol types, and the mid-tropospheric relative humidity on the thermodynamical profile, radiative heating rates, and the cloud-top radiative cooling. To do so, the study used aerosol and cloud retrievals from CALIPSO and CloudSat as well as MERRA-2-based meteorological variables obtained as part of CALIPSO and ERA5 reanalysis dataset (see Table 1). The study focused on the area between 10 to 18°S and 2 to 10°E, and between June and October 2007 and 2010. According to

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Discussion paper

the study, the justification of the area and study period is because the low-level cloud and above-cloud aerosol maximize sometimes between June and October. In addition, the authors also added that the months of study were chosen in other to compare their results with previous studies of (Deaconu et al., 2019) and (Adebiyi et al., 2015), who used June-August and July-October period, respectively. Furthermore, the study divided their dataset into three cases: (1) the smoke cases – based on the aerosol retrieval classified as “elevated smoke" in CALIPSO. These are cases where the cloud layers (between 0.75 km and 2.5 km) are separated from the “elevated smoke" layers by at least 0.4 to 6 km. (2) The mixed cases are the same as the smoke cases, except that they are for other CALIPSO aerosol retrievals NOT classified as “elevated smoke". (3) The pristine cases with cloud layers between 0.75km and 2.5km but no aerosol above.

The study finds that the smoke and mixed cases have similar meteorological conditions (winds direction, thermodynamical profile), although with different magnitudes. The study also suggests that no monotonous increase in the mid-tropospheric relative humidity with increasing aerosol optical depth exists. In addition, they find that while the vertical distribution of the SW heating rates corresponds to the above-cloud aerosol extinction for both the smoke and mixed cases, they do not correlate with the moisture distribution.

Given the importance of the southeast Atlantic’s aerosol-cloud interaction to the global radiative budget, this article will add to the growing body of knowledge about the complicated aerosol-cloud-meteorology system over the southeast Atlantic. While the article is generally well written, I have some comments that I hope the authors will address.

1. The authors should change the title. While smoke is undoubtedly an absorbing aerosol, I don’t think the authors have shown sufficiently argued that the “mixed cases” are indeed “non-absorbing” aerosols. To my understanding, these mixed cases also contain smoke and dust, which are both absorbing aerosols (Samset et al., 2018).

2. That said, I think calling other CALIPSO-derived aerosol types that do not fall within the "elevated smoke" classification "mixed cases" might be somewhat inaccurate. The reason is that the term "mixed" gives the impression there are always more than one aerosol species within the aerosols layer at all times, different from the elevated smoke. Although Fig. 1 shows cases of polluted/continental smoke, dust, and dusty marine aerosols, it is unclear whether those cases represent the entire atmospheric column or only the mid-troposphere. Moreover, the lidar ratio used to separate the elevated smoke in CALIPSO V4 is identical to that of polluted smoke, except that elevated smoke is defined as smoke in the mid-troposphere. Hence, if indeed the authors focused on aerosols above clouds, then part of the polluted smoke included with the mixed cases is in-fact misclassified elevated smoke in the mid-troposphere, which is expected to have similar optical properties, and therefore similar impacts on meteorology and low-level clouds as the elevated smoke. Hence, in addition to changing the classification name, the authors need to make a stronger case that the optical properties of the “mixed cases" are in fact, different from those of "smoke cases". The author should also show that a significant amount of dust and smoke are represented at all height levels of the mid-troposphere to justify their "mixed cases" argument. Perhaps Fig. 3 can be done for the different aerosol species identified in Fig. 1.

3. The author should discuss the differences between this study and that of (Adebiyi et al., 2015) and (Deaconu et al., 2019), given that the conclusions in this study are sufficiently different from the other two studies (to some degree). Although some comparisons were made in some parts of the texts, it is mostly incoherent and unclear to the reader looking to know how exactly this study stands out from the previous papers. I will suggest that the authors have a separate sub-section, where they discuss specifically the differences between this study and previous ones.

4. The authors did not explore the caveat that CALIPSO bottom layers have substantial uncertainty, as have been shown in previous studies. For example, CALIPSO has been shown to have a significant bias when compared to aircraft-based observation from

HSRL (e.g. Kacenelenbogen et al., 2011), and satellite-based observation from CATS (e.g. Rajapakshe et al., 2017)). The authors should account for this uncertainty in their analysis. In addition, the authors should also discuss how this caveat could affect their conclusions.

5. Surprisingly, the authors find decreasing RH as a function of AOD. First, as indicated below, I think the same AOD and RH intervals should be used for all cases highlighted in Fig.6-8). That puts the results on equal footing and allow for better comparison. Secondly, I wonder if something is inherently mistaken in the dataset that the authors used, since it is wildly different from the previous study. Adebiyi et al. 2015 used radiosonde collected at St. Helena Island and found a reasonable relationship between AOD and moisture in the mid-troposphere. Other studies that used reanalysis datasets have found a similar positive relationship. Here, the authors used MERRA temperature and relative humidity values packaged with CALIPSO retrievals, not those directly obtained from the MERRA website. While I do not suggest that these datasets may be different, I think the author should validate their results using other reanalysis datasets. Specifically, the authors should use the ERA (and possibly NCEP) reanalysis and show that the same result holds as those found in this study. Those results/plots should be included in the supplementary document and cited in the paper. It is worth noting that these reanalysis datasets are often significantly different, especially over the ocean where limited ground-based constraints are assimilated. Nevertheless, as Adebiyi et al. 2015 (see their Fig. 14) showed, the reanalysis datasets are expected to be broadly consistent. Third, I wonder if other criteria set in the analysis also do play a role in the differences? The authors could use their datasets and test their results over the same regions and periods defined in other studies (Adebiyi et al., 2015; Deaconu et al., 2019). Finally, given that this part of the study is in sharp contrast from other studies, the authors must spend a sufficient amount of time explaining to the reader that these results are accurate, and also discuss in detail the difference between this study and others that found different results (see comment above).

6. Lastly, the details of all dataset should be discussed in the methodology. In particular, a lot of this study's results rely on the CloudSat-derived radiative fluxes. While those radiative fluxes may have used CALIPSO aerosol extinction profiles, they make other important assumptions about the aerosol's optical properties that can have important implications on this paper's conclusion. The authors must discuss the limitation of those assumptions in their results. In addition, there is a potential difference in the thermodynamical profiles used to obtain those CloudSat-derived fluxes (ERA) and those used for general characterization of meteorology in this study. As suggested above, the author must show that both reanalysis datasets support their overall conclusion.

Other Comments

1. Section 2.1: Given that the study relies on elevated smoke versus other pollution, some more details should be provided on how CALIPSO made those classifications.

2. Line 169: I am finding it difficult to reconcile why the number of profiles shown in Table 2, for smoke cases, for example, is higher than the total number of cases of elevated smoke shown in Fig. 1. I understand that the "smoke cases" should be a subset of the CALIPSO's "elevated smoke" classification since it has some additional criteria. The same discrepancy appears to be present in mixed cases as well. Are they strictly cases where the aerosol layer is above the low-level cloud for both the smoke and mixed cases? That is what Section 2.3 appears to suggest. The difference between Fig. 1 and Table 2 should be further clarified.

3. Line 175-178: Based on previous studies, like some of those cited by the authors, I think it is unlikely to have more cases of aerosols above clouds between 2-6E than 6-10E. I will suggest that the authors make the same plot as Fig. 3 but separated as a function of longitude or latitude. Such a figure can be included in the supplementary document.

4. Figure: While it may already be mentioned in the text, Fig. 2 should include the latitude range considered for the longitude distribution. Similar thing for the latitude

distribution. Also, for Fig. 3.

5. Line 208: Change "...southerly component compared to the two aerosol cases" to "...southerly winds compared to the two aerosol cases."

6. Line 227: Please re-write the sentence.

7. Line 214 should be Figure 5a-c

8. Line 231 should be Figure 5d–?

9. Section 2.1 There should be a brief description of how the radiative heating rate of Cloudsat is obtained to give the reader the complete picture and the ingredients to interpret the result adequately. For example, while Cloudsat may have used the vertical extinction profile from CALIPSO, what spectral distribution of single scattering albedo and asymmetric factor are assumed? What surface parameters that could affect radiative heating is assumed? How do they treat other wavelengths that are not provided by CALIPSO in longwave and shortwave? The author needs to provide a complete context to interpret the result.

10. Fig. 5 (as on others, see comments above) – always provide complete information for each figure. In this case, information of what data is plotted should be mentioned in the comment section.

11. Fig 6: Again, information needs to be complete. Where is the AOD coming from?

12. Like 249-254: The two paragraphs are repeated. The author should carefully go through the manuscript before any future re-submission.

13. Line 256-58: Any comparison between different cases will likely not be "fair" – that is, it will result in comparing "oranges" against "apples". I will suggest that the authors make the tercile range the same for the AOD classes.

14. Section 3.5: If AOD will be used to make inference about RH with the aerosol plume, it must be AOD of the aerosols above the cloud. It is difficult to expect a clear

linear relationship between RH averaged within a layer and column integrated AOD.

15. Line 275: Is this RH averaged within the aerosol layer or the entire atmospheric column? Please clarify this within the text and in the figure caption.

16. Fig A1: Check the caption. AOD or RH?

References Adebiyi, A. A., Zuidema, P. and Abel, S. J.: The Convolution of Dynamics and Moisture with the Presence of Shortwave Absorbing Aerosols over the Southeast Atlantic, J. Clim., 28(5), 1997–2024, doi:10.1175/JCLI-D-14-00352.1, 2015. Deaconu, L. T., Ferlay, N., Waquet, F., Peers, F., Thieuleux, F. and Goloub, P.: Satellite inference of water vapour and above-cloud aerosol combined effect on radiative budget and cloud-top processes in the southeastern Atlantic Ocean, Atmos. Chem. Phys., 19(17), 11613–11634, doi:10.5194/acp-19-11613-2019, 2019. Kacenelenbogen, M., Vaughan, M. A., Redemann, J., Hoff, R. M., Rogers, R. R., Ferrare, R. A., Russell, P. B., Hostetler, C. A., Hair, J. W. and Holben, B. N.: An accuracy assessment of the CALIOP/CALIPSO version 2/version 3 daytime aerosol extinction product based on a detailed multi-sensor, multi-platform case study, Atmos. Chem. Phys., 11(8), 3981–4000, doi:10.5194/acp-11-3981-2011, 2011. Rajapakshe, C., Zhang, Z., Yorks, J. E., Yu, H., Tan, Q., Meyer, K., Platnick, S. and Winker, D. M.: Seasonally Transported Aerosol Layers over Southeast Atlantic are Closer to Underlying Clouds than Previously Reported, Geophys. Res. Lett., (410), doi:10.1002/2017GL073559, 2017. Samset, B. H., Stjern, C. W., Andrews, E., Kahn, R. A., Myhre, G., Schulz, M. and Schuster, G. L.: Aerosol Absorption: Progress Towards Global and Regional Constraints, Curr. Clim. Chang. Reports, 4(2), 65–83, doi:10.1007/s40641-018-0091-4, 2018.

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