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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Satellite inference of water vapor and aerosol-above-cloud combined effect on radiative budget and cloud top processes in the Southeast Atlantic Ocean" *by* Lucia T. Deaconu et al.

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The authors use a combination of POLDER, MODIS, CALIOP and modeled meteorological profiles to evaluate the changes in met. parameters (e.g., Temperature, RH, Specific Humidity, Winds), cloud properties (droplet effective radius, top height, liquid water path), and heating rates as a function of more or less overlying AAOD. This paper is of good quality, well written and structured. It will be worthy of publication, once the issues below are addressed.

Overall comments: âĂć Section 2.1. could benefit from a Table listing all the products and corresponding satellites/ models used in their method. âĂć The authors base their study near the coast because this is where "aerosols are mainly detached from clouds" using CALIOP (by the way, CALIOP will likely miss the base of the aerosols). But then, further in their study, they analyze potential aerosol-cloud interactions. It would be worth adding some information on aerosol-cloud contact frequency over the region âĂć The reader could benefit from an explanation of their AAOD thresholds (i.e., >0.01 and <0.04); If AAOD is the threshold, and it says "high" or "low" aerosol loading, does this mean that the authors assume a constant SSA value? If not, shouldn't they say "higher loading and higher absorption" instead?

Detailed comments: . I had to read the title multiple times to understand it. "Combined effects of water vapor and aerosols on underlying cloud top processes and radiative budget from satellites over the South East Atlantic Ocean" or something along those lines would make it clearer. . P1, line 14: "it is a prerequisite" . P1, line 21: "sensing techniques" . P2, Line 10: "negligible wet scavenging" needs more references . P3, line 11, line 30 (and other places): should read "cloud properties", "particle size", "droplet effective radius" etc.. . P3, line 20: I suggest briefly describing the "assumptions" (i.e., mostly the CALIOP lidar ratio) . P3, line 21: when introducing the depolarization method, I suggest saying "first introduced by Hu et al., 2007 and further implemented by e.g., Liu et al., 2015, Kacenelenbogen et al., 2019 Deaconu et al., 2017" . P3, L22: "AAC properties" . P3, L31: I suggest mentioning SSA from Peers et al.(2015) is retrieved above clouds. . P4, L3: please consider referring to Table 1 or 2 of Kacenelenbogen et al. [2019] . P4, L15: "high loadings of smoke" . P5, L14: I suggest describing the "semi-direct effect" . P5, L30: available at 490 and 865 nm . P5, L31: I suggest briefly describing the Angstrom exponent . P5, L32: "the aerosol model prescribed in the POLDER (?) satellite algorithm" . P6, L6: Are MODIS cloud properties corrected for AAC? . P6, L7: "cloud altitude derived from POLDER (ZO2)" . P6, L7: "ZO2 is calculated using. . ." . P6, Section 2.1: As said in the overall comments, this section could benefit from a Table listing all the products and corresponding satellites/

models used in the method. . P7, L3: “The GAME model” . P7, L4: Instead of “for this”, I suggest “inputs to GAME are. . .” . P7, L10: Instead of “we”, I suggest “GAME uses” . P7, L12: Instead of “the CALIOP method”, I suggest “the standard CALIOP product can underestimate. . .” And this is happening also when aerosols are below a certain detection threshold. You could reference Kacenelenbogen et al. [2014]. . P7, L24: “using the POLDER method” . P7, L25: I suggest mentioning that, although this might not affect your study, the aerosol base height might still very likely be biased high after scaling the extinction profile as seen on Fig. 1 . P8, L7: Consider replacing easterlies by easterly winds . P8, L27: I suggest “decrease” instead of “go down” . P8, L28: I suggest “value prescribed for the dust model in the POLDER algorithm” if I understand this correctly. . P9, L1: For SSA values during ORACLES-2016, I suggest referencing Pistone et al., [2019] . P9, L13: I suggest describing Fig. 2g before Fig. 3 . P9, L17: “lower than” . P9, L19: “Nevertheless, the stratocumuli are low-level clouds, so, an underestimation of around 300 m by the POLDER product is more likely”: this is not clear to me. . P9, L21: “stratocumulus become more fractioned” would it be worth showing the cloud fraction as well? . P10, L9: I suggest explaining the thresholds of 0.01 and 0.04 for AAOD. Could it be an AOD of 0.2 at 865nm with an SSA of 0.8? . P10, L17: “aerosols are mainly detached from low level clouds”: I suggest to be more quantitative. Again, CALIOP aerosol base height is biased high. There is likely more contact than what is seen from CALIOP. Maybe use results from Rajapakshe et al. [2017]? . P10, L31: “June to August (JJA) 2008” . P11, L7: “from 7.5 to 10 g.kg-1” . P11, L8: “smoke plume level (i.e., between 850 and 700 hPa)” . P11, L18: “plumes resulting from” . P11, L30: “Figure 7b and 7c” . P12, L1: I suggest “smoke” instead of “polluted” (urban pollution and smoke being two different aerosol types when using satellite remote sensing) . P12, L25: the authors choose the sampling area so that “aerosols are mainly detached” from clouds. Are you implying more aerosol-cloud contact here? . P 13, L1: This is not clear. I would rephrase. . P13, L14: I would quantify how low the difference is. . P13, L20: “wind speed (see figure 10)” . P17, L12: “South East Atlantic Ocean” . P17, L13: “increase in size, decrease in absorption” . P17, L20: why

“advanced”?; replace “forcing” by “effects” . P17, L21: I would quantify “lower” . Fig. 4: I suggest adding that these results use CALIOP data in the legend; First row could say MJJ and second row could say ASO . Fig. 5, 6, 7: An illustration of the mean aerosol and cloud layer heights for the sampling domain and period would help the reader . Fig. 7d: Legend is confusing: is it now blue for august and red for June-July? . Fig. 9: It would not hurt to remind the reader that this is about clouds only and add “at 925hPa” to the y-axis of Fig. 9d . Fig. 7, 9, 10: I find it non-intuitive to color the “low” aerosol loading conditions in red and the “high” aerosol loading conditions in blue. I would have done the opposite

Deaconu, L. T., Waquet, F., Josset, D., Ferlay, N., Peers, F., Thieuleux, F., Ducos, F., Pascal, N., Tanré, D., Pelon, J., and Goloub, P.: Consistency of aerosols above clouds characterization from A-Train active and passive measurements, Atmos. Meas. Tech., 10, 3499–3523, https://doi.org/10.5194/amt-10-3499-2017, 2017. Kacenelenbogen, M. S., Vaughan, M. A., Redemann, J., Young, S. A., Liu, Z., Hu, Y., Omar, A. H., LeBlanc, S., Shinozuka, Y., Livingston, J., Zhang, Q., and Powell, K. A.: Estimations of global shortwave direct aerosol radiative effects above opaque water clouds using a combination of A-Train satellite sensors, Atmos. Chem. Phys., 19, 4933-4962, https://doi.org/10.5194/acp-19-4933-2019, 2019. Kacenelenbogen, M., et al. "An evaluation of CALIOP/CALIPSO's aerosolâĂŘaboveâĂŘcloud detection and retrieval capability over North America." Journal of Geophysical Research: Atmospheres 119.1 (2014): 230-244. Liu, Z., Winker, D., Omar, A., Vaughan, M., Kar, J., Trepte, C., Hu, Y., and Schuster, G.: Evaluation of CALIOP 532 nm aerosol optical depth over opaque water clouds, Atmos. Chem. Phys., 15, 1265–1288, https://doi.org/10.5194/acp-15-1265-2015, 2015. Pistone, Kristina, et al. "Intercomparison of biomass burning aerosol optical properties from in-situ and remote-sensing instruments in ORACLES-2016."

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