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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.

Anonymous Referee #3

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This paper provides a study of aerosols and clouds over the southeast Atlantic Ocean during the northern hemisphere summer season when smoke aerosols are transported over low-level stratocumulus clouds. The study is largely complementary to prior studies of the area. The unique contributions are the presentation of POLDER cloud properties in relation to the POLDER-retrieved aerosol optical thickness above the clouds and a radiative transfer model analysis constrained by observations and model reanalysis products to separate the contributions of aerosols and water vapor to changes in the radiative flux profiles during periods of high smoke concentration over the ocean.

The study largely confirms characteristics of the region previously described in the literature and, for the most part, supports prior hypotheses for how clouds respond to periods of high smoke transport in the layer above the clouds. The authors mount a hypothesis that variations in tropospheric humidity impact clouds through a weakening of cloud-top cooling. The paper may be suitable for publication in ACP, however I feel some the physical reasoning offered to support the authors' hypothesis for the impact on clouds of humidity variations requires a bit more rigor in its description, and I am concerned that it may rest on variations in a model-derived humidity profile that does not adequately resolve the vertical distribution of moisture to support the argument. Further comments on these and some other minor matters follows.

Major comments:

Page 13, lines 29-30 the authors claim that a difference in humidity at 925 hPa can explain the differences in LWP between the high and low AOT cases, but they do not explicitly describe the mechanism. Is the 925 hPa layer within the cloud layer or boundary layer where greater humidity is therefore able to condense, or is the 925 hPa level above the clouds and the authors are referring to a different mechanism? The physics behind this conclusion needs to be explained here.

Related to the previous comment, the physical reasoning described in the first two paragraphs of section 4.4 is difficult to follow. As mentioned above, the interpretation would seem to depend on whether the greater humidity at 925 hPa is considered in the cloud layer or not. Is it possible that the cloud-top pressure is sometimes below and sometimes above the 925 hPa level? If that is the case then there could be an artifact that appears as a difference between the high and low AOT cases, especially if the cloud-top height and cloud thickness is different between the two groups.

Also related to this is a concern about whether the ERA reanalysis is capturing the altitude and narrow thickness of the inversion layer at the top of the boundary layer. Is there some confidence that the inversion height is properly located in the vertical and that the 925 hPa humidity in ERA corresponds well with observed humidity?

Minor comments:

Some of the imager-based cloud products from satellite sensors assume that clouds are plane-parallel and homogenous within the field of view of the instrument. Are the retrievals shown in figure 2 and discussed on page 9 lines 7-22 based on a similar assumption? Often the clouds over the southeast Atlantic Ocean are broken or otherwise horizontally heterogeneous at scales smaller than satellite footprints. If this is a source of uncertainty for the POLDER retrievals, it should be discussed here.

In sections 3.1 and 3.2 it talks about justification for the sampling area and time period, but arbitrarily sets the AOT thresholds that define "low" and "high". How are these values selected? And how many samples reside in the space in between where AOT is between 0.01 and 0.04?

Section 3.3 is titled "covariance between humidity and aerosol loading", but in the discussion the word "correlation" is used several times. On line 9 of page 11 it is even described as a "strong correlation". Nevertheless, there is no correlation analysis shown in this paper. Certainly, the word "strong" should not be used without actually evaluating a correlation coefficient and presenting it as such. I would recommend avoiding the word "correlation" here unless the correlation coefficient is evaluated and reported in the paper. A high/low grouping analysis can show statistically significant differences in a property even if the correlation coefficient between the grouping property (AOT in this case) and the other observed property (humidity) is low.

Page 12 lines 3-5 discusses changes in subsidence that are expected with smoke aerosol loading, but it is not explained why they are expected. What is the physical reasoning for a relationship between the smoke loading the environmental subsidence?

In section 4.2 there is discussion of the radiative fluxes and the it appears to me that the values are instantaneous values for the afternoon overpass time of the satellite. I think

it is important to clarify if the radiative fluxes correspond to mid-day values because in other papers values are reported as estimates of diurnal mean radiative fluxes.

Page 4, line 16: The Sakaeda et al. study used the global atmospheric model (CAM) coupled to a slab ocean. This is a coarse resolution model, not a large-eddy model, which usually refers to models that resolve some cloud-scale dynamics, which CAM does not.

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