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Interactive comment on "Daytime aerosol optical depth above low-level clouds is similar to that in adjacent clear skies at the same heights: airborne observation above the southeast Atlantic" *by* Yohei Shinozuka et al.

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This paper provides a valuable check on a frequent assumption in aerosol cloud studies: that aerosol properties, including optical thickness and intrinsic properties, such as particle optical properties, in cloudy skies are similar to those same properties in the clear-sky areas between clouds. This assumption is made in a large segment of the literature using passive remote sensing data of aerosols and clouds to investigate possible effects of aerosols on clouds, such as microphysical or radiative effects. In particular, the paper focuses on the southeast Atlantic Ocean case where light-

absorbing smoke resides above stratocumulus clouds. In doing so, they also provide a more detailed commentary on our previous paper (Chung et al. 2016), which found a curious difference in aerosol optical thickness (AOT) above cloud compared to clear air near clouds in CALIOP data that is different for daytime overpasses than for nighttime overpasses. This new paper is a useful application of recent high-quality airborne measurements to revisit these issues and is a valuable contribution. I recommend some revisions based on the following comments.

While the authors find no statistically significant difference in AOT above cloud compared to the adjacent cloud-free region, they do find a significant difference in the particle number concentration under the mesoscale averaging method, which combines observations from within 2 degree lat/lon boxes. This result is at odds with the finer scale analysis of the same quantity. To explain, the authors refer to a criterion on "outliers", but I think there is room for more discussion. In addition to clarifying what the outlier criterion is, I wonder if the extreme variability of this quantity in airborne measurements of the free troposphere due to layering of aerosol plumes, for example, make it likely that the sampling is insufficient to even address this metric. One way to evaluate this for particle concentration, as well as the other metrics, is to ask the question: given the observed variability is it likely that the number of samples present is sufficient to adequately sample clear and cloudy air within the 2 deg. box at all? Given that analysis of cloudy air to adjacent clear air shows no significant difference, I suspect that the real problem here is that you have not adequately measured the average particle concentration in the box. This question should probably be evaluated for particle concentration and the other metrics under the meso-scale sampling method.

On the matter of reconciling these new results with the previous Chung et al. (2016) paper, I recommend clarifying a couple of points:

(1) One of the pieces of evidence that the differences between above cloud and clear-air AOT might be an artifact of the CALIOP data is the contrast between the daytime and nighttime difference. This is emphasized by Chung et al. where they note that "The

global average AODct above low clouds is much larger during the nighttime than during the daytime for all seasons (Table 1), which is most likely related to a higher S/N [signal-to-noise ratio] for nighttime retrievals compared to those during the daytime" (p. 5788) and again on p. 5790. While the submitted manuscript mentions that detecting aerosol layers is sensitive to the signal-to-noise ratio which is impacted by the background lighting conditions (lines 67 and 304), it does not clearly make the point that this leads to a strong day/night contrast in the detectability limit. This is, of course, well known in the remote sensing community, but I think that the paper could make this part of the work more accessible to a broader readership by expressing this simple point more directly.

(2) The paper uses data from HSRL and finds no evidence of the differences between above-cloud and clear-air AOT reported in the Chung et al (2016) paper, and builds further support that the differences reported in Chung et al. (2016) are an artifact of the CALIOP data. However, on line 73 the authors also note that Kacenelenbogen et al. (2014) "saw no clear bias" in above cloud AOD between CALIOP and HSRL. How are we to reconcile these to points that seem contradictory?

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