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Interactive comment

Interactive comment on "Radiative effects of long-range-transported Saharan air layers as determined from airborne lidar measurements" *by* Manuel Gutleben et al.

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Review of Gutleben et al. This paper presents radiative transfer calculations of Saharan Air Layer (SAL) conditions relative to more background marine demonstrating that dust radiative effects are second order relative to that of water vapor co-transported with the dust. In short, for more background marine conditions large-scale subsidence of dry air is used as a baseline in comparison to SAL dominated environments. Indeed, while the "SAL" is considered dry, it is not as dry as the downward Hadley cell. In concept I think this is a fine exercise and demonstrates the need for more holistic consideration the background aerosol environment. Their finding that differential heating can lead

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to SAL destabilization is also interesting, and may explain previous observation that large dust particles are transported further than they theoretically should under laminar conditions (e.g., Maring et al., 2003; something perhaps they may want to mention in their abstract).

In general, I found the paper well written. While there have been simulations done elsewhere to this effect, but I don't think they have been published, certainly not as neatly as this. They also made some changes in response to my "quick" preproposal comments. There are a few things thought at require some correction in a final review. Hope this helps. Jeffrey S. Reid, US Naval Research Laboratory. Introduction

Page 2 line 1: "SALs remain relatively undisturbed and can be transported over thousands of kilometers towards the Caribbean or Americas (Carlson and Prospero, 1972; Karyampudi and Carlson, 1988; Karyampudi et al., 1999)." However, in the PRIDE campaign we showed this is not the case (e.g., Reid et al. 2002,2003; Maring et al 2003) there is considerable variability in dust heights by the time dust reaches the Caribbean. People like the Karyampudi model because it is simple, but it is an idealized situation. Further, the SAL is not always well defined in association with dust transport, especially in January through May. June is a transition month, late July-August is when the Karyampudi SAL model is most appropriate. Indeed, dust transported across the Atlantic in lots of different ways, and the authors language convolutes these mechanisms. During the PRIDE campaign (e.g., Reid et al., 2002), the highest dust concentrations were actually in the marine boundary layer, NOT IN THE SAL. The reasons for this are open to debate (Reid et al., 2003 versus Colarco et al., 2003).

Page 2 Line 13. "We found enhanced water vapor mixing ratios within the SAL compared to the surrounding dry free troposphere." Again, this is true in the context of a well-defined SAL relative to large scale subsidence in the Hadley cell. Under the influence of an easterly wave, there can be quite a lot of moisture around. I have no concern with this study looking at more idealized situations, but it should be mentioned that this analysis is just that, idealized. There is much more variability in the region.

I don't expect the authors to handle this full range of complexity, as their point is well made. But they should discuss it.

Page 3, line 11. "...(NARVAL-II) took place in August 2016..." my point exactly. This is a limited time period in the middle of the most representative of Karyampudi.

Page 5, lien 28. I am not sure why you don't make full use of the HSRL here. We know the mass extinction efficiency for dust is around 0.5 m2 g-1 and you have an extinction measurement. Or if you have noise issues, aerosol backscatter with a mean lidar ratio at least provides linear error propagation (e.g., Reid et al., 2017). Using AERONET retrievals would be a last resort in my mind. In fact, I have my doubts as in mixed environments the retrievals have to apply a mean index of refraction, which does not fit anything.

Page 7, Line 5. As mentioned in my pre review, I think the Hess models have serious problems with dust, right down to incorrect spectral dependence of extinction and large uncertainties in spectral absorption. I think the authors should look hard at the results of Hansel et al. 2009 and Sokolik and Toon 1998. Again, I don't expect the authors to resolve this, and using OPAC is ok for a baseline study. But the authors should be clear on this point.

Page 8, line 8 "hygroscopy" should be hygroscopicity

Page 9, Figure 2. I am not sure based on these lidar profiles that one can say that the MBL goes to 1.6 km. It really depends on where the cloud tops are on whether or not there is detrianmetn there or if it is a residual layer form somewhere else. Mixed layer is easier to define, but MBL top is a bit amorphous.

Page 10/Page 123 line 10/Page 15 line 30: The authors use potential temperature to define mixing, whereas it really should be equivalent potential temperature. Water vapor profiles for case (b) are well mixed in the middle of the SAL, (c) is distinctly not, with multiple water vapor layers visible corresponding with dust concentration. Mixing ratio should be constant in the presence of mixing. So with the difference in vertical heat shown, why is there stratification? You may want to look at wind shear.

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