

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

## **Anonymous Referee #2**

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The authors present vertically resolved measurements of aerosol optical properties and water vapor mixing ratios at the same time combining different lidar techniques. The data were collected with an airborne lidar instrument in the long-range transport regime of Saharan dust during the NARVAL-II campaign. Using the profile measurements in the Saharan Air Layer (SAL) as input, they perform radiative transfer calculations. They found that the water vapor inside the SAL is the driving force for the radiative cooling at top of the SAL. The dust particles have only a minor contribution to the net heating rates. The heating rate inside the SAL is decreasing with height to a maximum cooling at the top of the SAL. With these findings the authors gave sound explanation for the vertical mixing within the SAL which counteracts the gravitational

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settling of the large dust particles during long-range transport from Africa towards the Caribbean.

The findings presented in the manuscript contribute significantly to the scientific understanding of the long-range transport of the Saharan air layer over the Atlantic Ocean and publication is highly recommended. Although the main message of the manuscript was already given in a publication by the same authors (Gutleben et al., GRL 2019), the current manuscript contributes to a deeper understanding of the process by presenting 3 well-analyzed case studies and the calculation of the radiative effect of the SAL at surface and top of atmosphere. The authors highlight the importance of the correct water vapor profile in radiative transfer simulations in case of the SAL. Important for the quality of the study are the simultaneous measurements of the vertical profiles of aerosol optical properties and water vapor replacing assumptions which are often made by studies relying on passive sensors.

The manuscript is well written and could be published after minor revisions which are addressed in the following.

The main points to be considered are:

I. A clear definition of the term “marine boundary layer” (MBL) is missing. In the presented case studies (Fig. 4) the so-called MBL reaches up to the trade wind inversion at about 1.4 – 1.8 km height. Later, it is stated that the SAL is confined at the bottom by the trade wind inversion which is not in line with the indicated SAL in Fig. 4. Please explain in a more quantitative way how the upper limit of the MBL and the lower limit of the SAL are determined in your study. Various attempts to describe the vertical stratification in the Caribbean are found in literature (“convective marine boundary layer (CMBL)” in Groß et al., 2016, “sub-cloud layer (SCL)” and “intermediate layer (IL)” in Jung et al., 2013, “marine aerosol layer (MAL)” in Rittmeister et al., 2017). The convective part (CMBL or SCL) of the MBL or MAL is clearly visible in Fig. 2 and reaches up to around 600 m height. A definition of the MBL as used in the present study would

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improve the manuscript.

II. Some comments concerning the aerosol classification (Sec. 2.4.1):

You discuss only the contribution of pure mineral dust and pure marine aerosol and its mixture. What about contributions of other aerosol types like pollution from the African continent? At this time of the year, it is not very likely, but should be mentioned.

The effect of dry marine aerosol on the depolarization ratio was even studied at Barbados and is described in Haarig et al., 2017a.

III. Some comments concerning Section 2.4.2:

The lidar ratios for dust (47 sr) and marine aerosol (18 sr) are based on literature from different locations around the globe. However, Groß et al., 2015, reported higher dust lidar ratios of  $56 \pm 7$  sr for Barbados. Could you derive with your high spectral resolution lidar (HSRL) actual lidar ratios of the pure types during your campaign to judge which lidar ratios are more appropriate for your situation?

What values for the particle linear depolarization ratio have you used in Eq. 4 and 5? 0.26 and 0.04?

From the text it is not clear whether you have calculated the conversion factors (extinction to volume) using the method described in Mamouri and Ansmann (2016) or whether you have taken the values from the literature. Looking into the cited paper, I can't find your conversion factors.

IV. For your radiative transfer calculations, it is important that no cirrus cloud is present above the scene. The upper panel of Fig. 1 (not yet used in the text) supports the absence of cirrus clouds.

Minor comments:

1. Pii, L12 + L26: Using the term “we” for studies by the same authors is a matter of taste.

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2. Pii, L21: Gutleben et al. (2019x),  $x = a$  or  $b$ ?
3. Pij, L 6: Haarig et al., 2018, describes smoke, you probably mean Haarig et al., 2017b.
4. Piv, Eq (1) What is  $\delta T/\delta t$ ? And two lines later a  $\delta$  is missing ( $\delta F$ ).
5. Pv, L8: Saharan dust \_and\_ marine aerosols
6. Pv, L11: Burton et al., 2012, is not a good reference for the unchanged dust properties. It describes the aerosol classification and gives just a case study showing mineral dust. Look for other references.
7. Pvi, Eq (2) – (5) and the whole page: Indices should not be written in italic.
8. Pvii, Table 2: The percentages for the mixing ratio refer to which quantity (volume, number, ...)?
9. Pix, Fig. 2, caption: The profile are not shown in red and blue, but green and black.
10. Fig. 2 and Pxi, case (a): There is still a thin layer of some depolarizing particles between 1.7 and 2.5 km height. You are right, there are no dust signatures above 3 km height. But what about the particles above the MBL top? Please discuss this issue shortly.
11. Fig. 3, caption: light-green -> light-blue
12. Pxi, L27/28: “Case (a) however, indicates that no distinct correlation of enhanced  $rm$  and  $R(532)$  could be observed in a SAL-free troposphere.” What do you mean by “no distinct correlation”? Both curves are decreasing with height.
13. Pxii, L5: 24 h-averaged – The explanation follows some pages later: “It is assumed that the observed profiles do not change and remain stationary within a 24 h time frame.” (Pxiv)
14. Pxii, L11: 2–5 g/kg, one page before you stated 3–5 g/kg

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15. Fig. 4: DIAL measurements are shown in black not blue.
16. Fig. 4 right: Black and dark blue are difficult to distinguish. Please take a different color.
17. Sect. 3.3: According to the text, the radiative effects are calculated for the Saharan dust particles disregarding the water vapor. Is this the case? Please state it clearly.
18. P<sub>xv</sub>, L8-16: This paragraph should be carefully rewritten. “Calculations of RETOA show similar results for case (c). (L10)” – Similar to what? In case of negative numbers, what is considered a minimum? (L13-15)
19. P<sub>xvi</sub>, L20: Reference in brackets.
20. P<sub>xvi</sub>, L34: January/February 2020
21. P<sub>xvii</sub>, L17: “and therefore counteracts the development of convection \_in the MBL\_”
22. P<sub>xvii</sub>, L20: “. . . were found at intermediate zenith angles \_for the presented case study\_” However, case study (c) represents a rather thick SAL and so the reported maximum values should hold for long-range transported dust at Barbados.
23. Sometimes your sentences tend to be very long. It would be easier for the reader to split them into two sentences, e.g., P<sub>vii</sub>, L21-24 or P<sub>xv</sub>, L21-23

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