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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Saharan dust contribution to the Caribbean summertime boundary layer – A lidar study during SALTRACE" *by* Silke Groß et al.

Anonymous Referee #3

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General comments

The paper presents a case study related to SALTRACE campaign addressed to the characterization of the boundary layer with the presence of a mix of aerosol (dust and maritime) in the Caribbean area during a Saharan dust transport. It deals with a very interesting topic for the scientific community involved in atmospheric research because it provides information that can be merged with other results coming from other papers produced for the same campaign, obtaining a large and exhaustive overview and interpretation of the atmospheric observations in a particular site and in several kinds of conditions. This gives the paper a value even if it is not particularly original.

The paper seems to be written with no sufficient detail in the discussions and justifi-

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Discussion paper

cations. Improvement and more care should be requested in English language, being present several English grammar typos errors.

Specific comments

Some general considerations

1) In order to give the paper more completeness and to allow a better understanding of the observations, the authors should link (and cite) a previous paper: *"S. Groß, V. Freudenthaler, K. Schepanski, C. Toledano, A. Schäfler, A. Ansmann, and B. Weinzierl, Optical properties of long-range transported Saharan dust over Barbados as measured by dual-wavelength depolarization Raman lidar measurements, Atmos. Chem. Phys., 15, 11067–11080, 2015, www.atmos-chem-phys.net/15/11067/2015/, doi:10.5194/acp-15-11067-2015"*, where most of the authors are the same, the lidar system is the same and also the measurement period is the same. In the present paper the authors address the study to a different day, characterized by a dominance of marine aerosols.

2) There are several references to papers in preparation for the same special issue. In my opinion, this is possible if some aspects presented will be furtherly analyzed and discussed in those, but this is strange if the results of those papers are used (e.g. Groß et al 2015, Haarig et al., Marinou et al.) before the corresponding peer review processes. In principle, the results or conclusions of those papers could also be rejected. This paper should have its own self-consistence and therefore the results of those other papers should be introduced in a different way, otherwise the paper should be accepted after the others will be accepted for publication.

In detail

Page 2, Lines 11-12: The authors write: “This strong increase at the top of the cloud-topped or cloud-less CMBL is to our opinion a clearly sign for an efficient...” The conclusion should be better discussed by the authors.

Page 3, Line 6: The authors justify the assumption about the two component mixture of marine aerosols and mineral dust with “coordinated in-situ measurements”. Which kind of measurements?

Section 2.1. The authors assume several values for linear depolarization ratio for dust and marine aerosols, lidar ratio. Did they try to have support from direct measurements to these assumptions? For example, in my opinion, with reference to the paper I cited before (Point 1), why in this paper the authors do not use a similar optical characterization?

Section 2.2: It is not clear to me how, from the reference ensemble of Gesteiger et al (2011) at 532 nm, the value 0.68x10-6m is obtained. But, in general, this is applicable also to the several values of v/alpha. The assumptions are introduced in a very fast way, without justifications. I think, a discussion, even if minimum, should be given to give the paper a self-consistence.

Page 4, Line 15: Did the authors tried a comparison using Raman measurements? According to the paper I cited at the beginning (Point 1), POLIS is also equipped with Raman channels. How the backscatter in fig 2a are calculated? Why Raman measurements have been not used to characterize the layers like in the previous paper (see Point 1)?

Page 9, Table 1: It is not clear to me the case 24 June – 10 July. What does this indication mean: dust and marine (marine dominated), but without marine cases.

Page 10, Line 2: Which is the distance between the measurement site and the Ragged Point? Is this comparison significant?

Page 10, Line 8: Which is the meaning of the factor 1.25? How is it obtained?

Page 10, Lines 9 and 10: What does it mean "we assume an uncertainty of. . .". How this estimation has been obtained?

Page 10: The comment to the results of Fig. 6 is really very short. In general, these should be better discussed.

Page 11, Summary: I image that the conclusions are referred to the 10 July case study. The authors does not report this. Moreover, I do not see correspondence between the values reported for PLDR in the Summary and those reported in table 1. Again, in the last line, which is the distance from the eastern part of the island? In general, the summary should be more clear and should give the idea of the importance of the reached goals.

Technical corrections

Page 1, Line 5: change "information of the CMBL" into "information on the CMBL"

Page 2, Line 21: change "information of the boundary layer" into "information on the boundary layer"

Page 2, Line 24: change "ground-base" into "ground-based"

Page 2, Line 26: change “located at the area” into “located in the area”.

Page 3 Lines 6-8: Specify that the content of the sentence has been demonstrated when aerosols are transported across the Atlantic in summertime, otherwise it seems valid in general.

Page 7, Lines 3 and 5: change “on top the CMBL” into “on top of the CMBL”.

Page 7, Line 3: change “found, that” into “found that”. Remove the comma.

Page 8, Line 9: the authors write “AOD >= 0.4 nm”. They missed the wavelength between “0.4” and “nm”

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