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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Triple-wavelength depolarization-ratio profiling of Saharan dust over Barbados during SALTRACE in 2013 and 2014" *by* Moritz Haarig et al.

Anonymous Referee #1

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The paper of Haarig et al. presents a study on the spectral dependency of the lidar-derived depolarization for dust aerosols. This is a very interesting aspect that has already been covered in other publications in the same group, but which deserves to be published under the SALTRACE program. It also provides additional information on the evolution of dust depolarization during long range transport over the tropical Atlantic. Whereas the tools used are well presented and referenced, there are some important aspects of the article that need to be clarified before publication. It is also necessary to review some parts of the results presentation, especially in section 3.

Major remarks:

Abstract

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SALTRACE-2 does not have to appear in the abstract because it is not used.

Introduction

The introduction is well constructed and it clearly shows the interest of lidar systems with several polarization channels. The democratization of lidar systems of this type has an interest in the broader atmospheric physics domain, but this can only be done if manpower and money are available, which is far to be the case everywhere. Moreover, there are limitations related to eye safety, to obtain authorizations from air navigation authorities, regardless of location in the world. It would be nice to write a few words about it. About the dust study, please expand the reference list to other groups than the team of the authors. Many others have carried out and published scientific works on the subject of dust.

Section 2

The SALTRACE project is very interesting and has been conducted with a relevant approach.

Line 34p6: Why “is not optimized”? Hopefully, there have been many uncertainty studies on BERTHA and it must have a better SNR than POLIS.

This section refers to Appendix A.

Appendix A

This appendix is quite long, but the results are coming from another article. The calibration process presented is fairly standard in optics. It is laborious and indeed necessary when broad interferential filters (some nm) are used, which integrate temperature sensitive of highly depolarized inelastic scattering lines. But it has been published several times before.

Conversely, it is necessary to give clear information on the stability over time of the calibration. It can evolve with different environmental factors (for example the cleanli-

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ness on the optics, the temperature, etc.) and with the aging of certain elements of the detection chain.

Remark: the Mueller matrix computation usually requires precise information on materials and optics in general. That does not seem so easy to obtain. Nevertheless, even if the values are not perfectly accurate, this does not hamper the cited sensitivity study.

Section 3

This section should be reorganized in order to make it easier for the reader to understand its aim. Between 3 and 3.1, explain that there are 3 case studies and how these different cases are interesting.

Sub-section 3.1 Rename the title to 1st case study: ...

Lines 14-15p8: What parameters are homogeneous? If you are talking about the AOT, this is not the case (see MODIS).

Lines 18-19p8: This is high for a MBL and actually there can be local effects. The MBL is rather blue in Fig. 2 and the layer above is in the free troposphere. The last one can be created by forcing at the MBL top, linked to the presence of Sc for example. It may be necessary to look at CALIPSO measurements to confirm or disprove this.

Lines 24-25p8 (fig. 3): With 10-day backtrajectories, it is more relevant to use the ensemble mode of Hysplit.

Line 27p8: Provide a reference or explanation for what is called the Âń classical Raman lidar technique Âż.

Line 1p9: There are previous references to marine aerosols, for example Flamant et al., JGR, 1998.

Lines 4-5p9: Why not use what has been found in Fig. 4 to inverse POLIS? BERTHA could also be inverted with Gross et al. (2015) and compared to the previous inversion without using POLIS in order to minimize the polarization calibration effect of both

lidars. POLIS seems redundant in this section.

Lines 20-24p9: The error bars overlap and it seems difficult to conclude with certainty.

In Fig.4, are the error bars including atmospheric variability during the averaging time?

Sub-section 3.2 (instead of 3.1.1) (remove)

This section is more of a discussion and should not be included here. Moreover, it is based only on previous work. It is more a report of the state of the art that has its place in an introduction. The set of values presented comes from Mamouri and Ansmann (2017). It is not clear how this applies to the data used in the article.

Lines 1-5p10: The aerosol mode ratio changes a lot if you look at AERONET and the values given here can significantly change.

Lines 20-25p10: This is a conclusion, but not for this article.

Sub-section 3.2 (stay section 3.2) The title is not in the topic of the paper. Change the title to 2nd case study: . . .

Lines 27p10-7p11: This is an important point to check the consistency of the depolarization data between the 3 wavelengths of the BERTHA lidar. This should be placed in a dedicated paragraph, before presenting the case studies. Why did not you evaluate the LRs on your measurements themselves?

Line 25p11: Could you explain why?

Sub-section 3.3 Change the title of sub-section 3.3 to: “3rd case study: dust transport from Africa to Arkansas over 12000 km”

Section 4

In all the text, the meteorological descriptions encountered during the campaigns must be given earlier in the article, for instance after sub-section 2.1.

Lines2-3p14: The sentence is not clear.

Line 21p14: The differences between these values are in the error bar, thus, the differences are not significant. For the comparison between SALTRACE, SAMUM1 and SAMUM2, are you certain that the dust sources are the same and that the aerosol nature does not change?

Lines 1-5p15: there is no change between 532 and 1064 nm from the simulations, why?

Minor remarks:

Line 17p3: lidarstudies to lidar studies Line 18p3: assuarance to assurance Line 30p6: remove V before et al. Line 21p8: 532 nm instead of 1064 nm? Line 33p9: 50-60% is it the fine mode fraction? Line 12p10: What observations? Line 17p10: diameter in the... Line 17p11: replace equal by very close Lines 32p10-2p12: Not useful for the paper. Lines 5 and 9p12: Cite the figures in order of appearance Line 7p13: remove "see sect. 2.4", not useful Lines 18-19p13: already said Line 20p17: amtmospheric to atmospheric Table 3: define each variable in the table caption.

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