General Comments:

This paper deals with triple-wavelength depolarization ratio measurements performed in aged Saharan dust layers, highlighting the importance of multi wavelength polarization observations for dust modeling improvements and dust characterization. The authors rightly acknowledges previously reported studies, related to field/laboratory observations and to modeling simulations.

The manuscript is well written, discussing interesting measurements, and worth being published in Atmospheric Chemistry and Physics, under the SALTRACE project. However, in order to be improved, I would suggest to the authors to take into consideration the following comments.

Major remarks:

1. The introduction is well written providing a good overview. In my opinion in this section should be also mentioned that airborne triple-wavelength depolarization measurements of Saharan dust in Caribbean exist in the literature by Burton et al., 2015. However, this is something that is mentioned later in section 2 (Page 4, Line 33). Moreover, in this section, should be made clearer to the reader which is the focus of this manuscript.
2. Page 6, Line 34: Could you please comment briefly in which sense BERTHA was not optimized for depolarization observations?
3. Page 7, Line 15: To my understanding, for characterizing CALIOP observations as dust or not, a threshold value has been set regarding the particle depolarization ratio. Two lines later is mentioned that “only the observations characterized as dust or polluted dust from the CALIPSO…”. In case that are used also scenes characterized as polluted dust, then lower values of particle depolarization ratios are introduced to the statistics. Could you please comment more on this and make it clearer?
4. The focus of the manuscript is to demonstrate triple-wavelength depolarization measurements of dust particles, through three case studies. These case studies have to be well organized and demonstrated in the manuscript, in order to be easier for the reader to follow up. Thus, I suggest to the authors to rename the sub-section headers accordingly. For example the 3.1 could be something like: “Case study I: 11 July 2013”.
5. Page 8, Line 29: The authors are mentioning that the backscatter coefficient at 1064 nm is significantly lower than the corresponding values at 532 nm. This seems to be more intense during the first case study, compared to the next two. How could this be further explained? The temporal evolution of columnar Angstrom exponent (500/1020 nm) as obtained from the corresponding AERONET site, indicate a variability around 0.1-0.2. This seems to be in contradiction with the high backscatter related Angstrom exponent derived from 532 and 1064 nm (Fig. 4; around 0.5 in the SAL). Higher backscatter coefficient at 1064 nm, would lead to different spectral dependence of particle linear depolarization ratio at the wavelength pair of 1064 to 532 nm (Fig. 5c). By the way, I

think that it would be beneficial for the manuscript if for each case study, the corresponding columnar AERONET observations, are also presented.

6. The sub-section 3.1.1 is really interesting and provides valuable information for the findings presented in this study. However, in my opinion should be moved either in a separated paragraph in section 2 as a generic methodology followed during SALTRACE project to provide complementary dust related information, or has to be shorten in order to be directly included in the sub-section 3.1 which describes the Case I. Moreover, I think that the last paragraph (Page 10, Lines 20-25) of this sub-section is more related to the Conclusions section, than here.
7. As already mentioned (see major remark No. 4) you are kindly suggested to change the header of sub-section 3.2 to something like: "Case study II: 20-21 June 2014". Moreover, in my opinion a dedicated paragraph should be constructed earlier than section 3, in order to demonstrate the consistency of the depolarization data between the 3 wavelengths of the BERTHA lidar. This paragraph could include the comparison with POLIS and the cirrus case presented in Figures 5 and 6.
8. Why in Fig. 6b the volume depolarization profiles at 532 and 1064 nm are not plotted above the cirrus? Is this due to signal to noise issues of the corresponding cross-polarized channels? Moreover, if the volume depolarization ratio is equal to particle depolarization ratio at 1064 nm, this is something that should be mentioned in the caption of Fig. 6b.
9. In Fig. 14 it would be nice also to show the number of case studies used for the corresponding statistical values obtained with BERTHA, MULIS and POLIS during SALTRACE and SAMUM 1-2.
10. In my opinion Fig. 15 is an important figure summarizing all the scientific results and open questions of the manuscript. The scientific answers which are related to the less sharp (compared to observations) simulated dust depolarization spectral slopes, along with the discrepancies found in HSRL-2 and BERTHA observations (which are inverted when going to higher wavelengths) are scattered in the manuscript, but not well summarized when describing Fig. 15.

Minor Remarks:

1. Page 3, Line 17: Change "lidastudies" to "lidar studies".
2. Page 5, Lines 8-9: It is clear from Fig. 1 that the linear polarizers are behind the beam expanders however, you could mention this also in the manuscript. Moreover, it would be helpful if you could describe if there is a specific need for using two lasers in BERTHA setup? Is this due to the implemented HSRL channel at 532 nm, or is related to the plane of polarization of each laser generated harmonic?
3. Page 6, Line 30: "Freudenthaler, V. et al., 2016" delete the "V.".
4. Page 7, Line 31: I kindly suggest changing the AERONET site name with the one that can be found in AERONET website, namely Barbados_Saltrace.

5. Page 8, Line 21: In the manuscript, the wavelength of 1064 nm is mentioned while in Fig. 2 and its caption the 532 nm is shown. Please consider correcting where appropriately.
6. Page 8, Line 27: I would suggest replacing the text “classical Raman” with “conventional Raman”.
7. Page 8, Line 28: Since there are many studies of Saharan dust observations with conventional Raman systems, I would suggest to the authors to provide some more references than “Tesche et al., 2011a”.
8. Page 9, Line 18: Please consider providing same number of significant digits for common parameters. This is a general comment and I would suggest go over the entire manuscript and tables, in order to correct it where appropriately.
9. Page 9, Line 29: Delete the word “here” due to redundancy.
10. Page 11, Line 3: “BERTHA was operated at…this measurement”. This information has been already provided earlier (page 5, line 10). I suggest you to delete it from here.
11. Page 13, Line 31: Change “give” to “given”.
12. Page 17, Line 12: A definite article “the” is missing.