Answer to comment of S. Gross

on "Retrieving volcanic, desert dust, and sea-salt particle properties from two/three-component particle mixtures after long-range transport using UV-VIS polarization Lidar and T-matrix" by G. David et al., *ACPD **13**, 1891-1947, (2013).*

We thank S. Gross for spending time writing a comment on our ACPD-paper. We here provide answers to her questions and remarks, and to ease the reading, as for designated reviewers #1 and #2, each comment is first recalled in italics, then the corresponding author reply is given. Our answer is short because most of your issues and comments were already raised by anonymous reviewers #1 and #2, and by M. Tesche, as you pointed out. We refer Dr. Gross to our answer to other reviewers.

S. Gross

"The paper 'Retrieving volcanic, desert dust, and sea-salt particle properties from two/three-component particle mixtures after long-range transport using UV-VIS polarization Lidar and T-matrix' contains local ground-based polarization sensitive Lidar measurements at two wavelengths, performed at Lyon, France. This manuscript and former papers of this group show the good performance and characterization of the instrument and analysis. However, I agree with the comment of M. Tesche that the manuscript and the study do not fulfill the requirements for publication in ACP in the current state. The paper is not well structured, hard to read and hard to understand."

Authors reply

Other Referees also noted that the clarity of the manuscript could be improved by a rearrangement, which we have done. Hopefully the revised version will be found easier to read and understand.

S. Gross

"The figures and the information within are sometimes hardly visible."

Authors reply

We have enlarged the font sizes in the figures, as also suggested by Referee #2.

S. Gross

"Many assumptions are made without critical discussion, and without sufficient references and justification. Therefore no reliable atmospheric implication can be drawn from this manuscript. Concerning the method, literature/references, the use of intensive and extensive parameters for aerosol type separation, inconsistency of the manuscript, transformation and mixing of aerosol during transport I refer to the comments by M. Tesche. There is no need for repetition."

Authors reply

Many of these issues have been addressed in the revised manuscript, based on the detailed comments from the other Referees. We refer Dr. Gross to our answer to M. Tesche. There is no need for repetition.

S. Gross

"The concept of using lidar measurements to separate the contribution of different aerosol components to a two / three component mixture is not in itself new as is stated in the introduction. However, the discussion of former literature on this topic in the introduction is insufficient, e.g. the reference of 'Marenco and Hogan 2011' (Marenco and Hogan, Determining the contribution of volcanic ash and boundary layer aerosol in backscatter lidar returns: A three-component atmosphere approach, J. Geophys. Res., 116, doi: 10.1029/2010JD015415, 2011) for aerosol type separation in a threecomponent mixture is missing."

Authors reply
The same point was raised by M. Tesche. See our response there.
We point out that the three-component mixture in Marenco and Hogan (2011) actually includes air molecules as one component. In this respect, our cases would be three and four-component mixtures. We have added reference to Marenco and Hogan (2011) but keep our original two and three-component mixture terminology. The conceptual difference between our and their mixtures is shortly mentioned in the revised manuscript (see highlight in cyan color).

S. Gross
"Furthermore key references concerning lidar measurements of volcanic ash, Saharan dust and sea salt aerosols are missing."

Authors reply
Among the about 80 references cited, there are many related to each of the topics mentioned by the Referee. We defined in the introduction the frame within which our new methodology applies (see page 1896, lines 26-29). In the interest to keep the study focused and the reference list from getting unreasonably long, we have chosen not to cite works related to passive remote sensing or Raman lidars, which are not necessary to apply our new methodology. We believe that this is reasonable. This is not a review paper.

S. Gross
"In the first case study shown in this manuscript the authors deal with measurements in the aftermath of the Eyjafjallajökull eruption assuming a mixture of spherical (ash) and non-spherical particles. The results of this case study are already published in Miffre et al. (2012) and therewith not new. But nonetheless there are some open points and questions concerning this case study. Which references and explanations do the authors give for the choice of the lidar ratio of S=55 ± 5sr? Several papers report about lidar ratio measurements in the volcanic aerosol layer. Authors should refer to these papers or give an explanation for their choice if it differs from these measurements."

Authors reply
See our response to Reviewer #2 about the same topic. We added reference to Ansmann et al. (2012), already quoted elsewhere in our discussion paper, also here.

S. Gross
"Furthermore, comparing the profiles of the particle depolarization ratio on 19 April 2010 at 19:00 UTC shown in Figure 7 (not Fig. 8 as referred in the manuscript) in this manuscript with the profile of the same time period shown in Figure 6c by Miffre etal. (2012) differences in both profiles are obvious. Where do these differences result from?"

Authors reply
In Miffre et al. (2012a), figure 6c has been acquired at 18h40 UTC while the one shown here has been acquired at 19h00 UTC. This time difference is sufficient to explain the observed differences in the two profiles (see also Figure 4 of Miffre et al. (2012a) describing the spreading of the volcanic ash plume in the Lyon troposphere in the form of a time-altitude map).

S. Gross
"In the second case study the authors assume a three-component mixture of Saharan dust, dry sea salt aerosols and water-soluble aerosols. These assumptions are highly speculative. The authors give backward trajectories as a proof for this assumption. This procedure is incorrect and misleading as trajectories cannot be attributed in a direct way to the aerosol type within an air parcel. Here further discussions are mandatory concerning e.g. dust outbreak in the area of influence of the trajectories, comparisons with dust dispersion models (no dust forecast for Lyon from BSC-DREAM model runs),

and detailed discussions why the authors assume sea salt particles in the measurement area (about 450 km away from the west-coast). Furthermore information on relative humidity along the trajectories would be highly informative with respect to the phase of the sea salt particles. This point is also addressed in detail in the comment of M. Tesche."

Authors reply

− The same point was raised by M. Tesche. See our response there.

− We point out that, having information on the phase of sea-salt particles along the trajectory, is not necessary, because, as we pointed out in old section 2.1 and at page 1921, line 10, sea-salt particles are solid for RH-values at the remote site below the sea-salt crystallization point (40 %).

− We point out that, in our introduction, we clearly stated the frame in which our work applies: (see page 1896, line 29 up to page 1897, line 4): "*A precise chemical analysis has not been performed during the experiments carried out, because it is not the scope of this contribution. We hence used 7-days air mass back-trajectories to identify the origin of the nonspherical particles at the remote site. Otherwise, the combined use of our Lidar depolarization experiment with laboratory chemical studies has been already published in Dupart et al. (2012).*" It is hence not the scope of this contribution to the Special Issue on Light Depolarization by Atmospheric Particles to study that point in particular. It is surprising that the comparison with dispersion models be asked for only for case study 2, which states the main novelty of our work (in case study 1, backward trajectories appear to be sufficient).

Specific comments

S. Gross

"Page 1921, line 20: When choosing S(UV)=50 ± 5sr and S(VIS)=60 ± 5sr as in between the computes values for S(dust) and S(SS), what are the computed values for S(dust) and S(SS)?"

Authors reply

These values are given in Table 3 of our manuscript. For the sake of clarity, we added this information (see text highlighted in cyan color).

S. Gross

"Page 1922, line28: What are the justifications for delta(SS)=33%,10%,0%? Give references".

Authors reply

These are somewhat arbitrary values used to test the robustness of our methodology. We have tried to clarify this point in the revised manuscript by replacing "exaggerated values" by "somewhat arbitrary, test values" (see text highlighted in cyan color).