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Atmospheric Chemistry and Physics Discussions

Interactive comment 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.

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

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 three-component mixture is missing. Furthermore key references concerning lidar measurements of volcanic ash, Saharan dust and sea salt aerosols are missing.

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$\pm$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. 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 et

al. (2012) differences in both profiles are obvious. Where do these differences result from?

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.

Specific comments:

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 1891, 2013.