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Atmospheric Measurement Techniques
Discussions
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Interactive comment on "Study of aerosol microphysical properties profiles retrieved from ground-based remote sensing and aircraft in-situ measurements during a Saharan dust event" *by* M. J. Granados-Mu noz et al.

M. J. Granados-Mu noz et al.

mamunoz@jpl.nasa.gov

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The authors would like to thank both reviewers for their thoughtful and helpful comments and suggestions. Their reviews have made a significant contribution to the improvement of the paper. The line numbering in the reviewers' comments refers to the manuscript published in AMTD whereas the line numbering in the responses refers to the new version of the manuscript.

Comment: General

Although the topic is of high interest, the paper is not in a good shape and acceptable form. I was and I am still close to the point of voting for rejection. At least, major revisions are required. Clear and precise answers to the questions are expected as well as improvements regarding all points mentioned below. The first part up to section 4.2 is ok. Referee #1 provides a long list of comments, so that I do not have to add all these points here. However, you may add two references to provide a better link to other AMT EARLINET special issue papers:

On page 9293, line 28 (l28), one could provide the reference: Binietoglou, I., et al., Atmos. Meas. Tech., 8, 3577-3600, doi:10.5194/amt-8-3577-2015, 2015.

Answer: Reference has been included in line 232.

Comment: On page 9303, l14, the following citation could be made: Mamouri, R. E. and Ansmann, A.: Fine and coarse dust separation with polarization lidar, Atmos. Meas. Tech., 7, 3717-3735, doi:10.5194/amt-7-3717-2014, 2014.

Answer: Reference has been included in line 373.

Comment: Here is the list of my main points which all deal with the CAS-POL part. As mentioned, I expect careful, precise answers to all questions. This will hopefully trigger a better internal discussion with the co-authors (especially with Darrel Baumgardner). Without fully satisfactory answers, the paper cannot be accepted for publication. Major issues:

* p9308 l6: Where do the 50% for the refractive index assumption come from? Appendix says 20% sizing uncertainty due to refractive index.

Answer:

The 50% is incorrect and should have been 20% from the analysis that is presented in the Appendix. This has been corrected.

Comment: * Fig. 6a: The errors plotted for CAS-POL volume concentration are on the order of 50%, but 90% are reported on p9315 l12. Where does this difference come from?

Answer: The horizontal bars shown on the CAS-POL concentrations are not error bars but the standard deviation about the mean values that are smaller than the uncertainties estimated using error propagation; hence, the measured variability is within the expected uncertainties. The text has been modified to reflect this.

Comment: * Fig. 6b: What is shown in Fig. 6b? Is the particle linear depolarization ratio plotted, as used for example by Freudenthaler et al., 2009?

Answer: The lidar depolarization ratio is the same as Freudenthaler et al., 2009. For the aircraft profile, is the plot is of the ratio S/P where S is the polarization perpendicular to the plane of incident polarization and P is the polarization in the same plane as the incident polarization. This has been clarified in the text.

Comment: * Fig. 6b, figure caption: Nomenclature is not consistent. Sometimes "polarization ratio" is used, sometimes the same quantity is called "depolarization ratio"?

Answer: For consistency with the lidar literature, we have normalized to “depolarization ratio” throughout the whole manuscript.

Comment: * Fig. 7: 1) As far as I understand, PCASP-100X detects the size range between 0.1 and 3 μm and CAS-POL detects 0.5 - 50 μm. Why are the data in the overlap region of both instruments not shown? Please also include the CAS-POL data below 4 μm. How good is the agreement between the PCASP-100X and the CAS-POL in the overlapping size range?

Answer: We have now included the CAS data below 4 μm. We had excluded it previously to avoid complicating the figure.

Comment: 2) What is shown in Fig. 7? Is it a volume size distribution (dV/dlog Dp) or is it something else (volume concentration per bin)? Are the data in the individual bins normalized to the bin-width?

Answer: Yes, the PSDs are normalized by the logarithm of the size interval. This is now clarified on the legend and the axis.

Comment: 3) Please include error bars in Fig. 7.

Answer: We have added standard deviations but only at one altitude level as adding to all five curves would be an unnecessary cluttering of the figure.

Comment: * p9309 l6: Why is ice and not dust used to explain that a larger depolarization ratio is measured by the CAS-POL in comparison to the lidar?

Answer: We have suppressed this figure and added a new one (see new Figure 9) derived from laboratory measurements of dust, ash, water and ice with the CAS-POL to illustrate the polarization response of this instrument to different types of ambient particles. We also have revised this discussion so that we are no longer talking about just water and ice. We have also emphasized that we do not expect the CASPOL depolarization ratio to match the depolarization ratio of the lidar, not only because of slightly different collection angles (a minor difference as we now describe in the Appendix) but more importantly because there is not currently a reference standard to calibrate the two channels of the CAS-POL.

Comment: * p9309 l9-11: P22/P11 is not the inverse of what is reported from lidar and CAS-POL! See Freudenthaler et al., Sassen et al. etc. for definition of the linear depolarization ratio.

Answer: See previous response

Comment: * Fig. 8: P22/P11 should be in the range 0.0-1.0, and not >1.0 like plotted in Fig. 8

Answer: See previous response

Comment: * p9309 l4-15: Besides that, the explanation is certainly wrong. According to scattering calculations (e.g. Dubovik et al., 2006 or Wiegner et al., 2009), P22/P11

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for dust is larger at 168-176 than at 180. Thus, the scattering matrix shows that the depolarization ratio for dust reported by CAS-POL should be smaller than the value from lidar.

Answer: See previous response

Comment: * p9313 l10-14: Unclear. Can you rephrase what is meant with this section?

Answer: We have replaced the sentence with the following (lines 667-669): "For the current study, FS is used to derive the EOD, and BS and POL are used to calculate the average depolarization ratios defined in the main body of the manuscript".

Comment: * p9314 l28: How were the depolarization ratio signals averaged? A) Sum up signals from both channels and divide or B) Divide and average? For comparison of CAS-POL data with lidar measurements, option "A" should be used.

Answer: In the original analysis, the depolarization ratio was determined particle by particle. The reviewer is correct and we have reanalyzed using the ratio of the summed S and P signals at each altitude.

Comment: * p9315 l1: How was this uncertainty of 30% derived? Which uncertainties are considered? Variation of orientation leads to a much larger uncertainties...

Answer: This has been replace with an estimate of 30-50% based on preliminary laboratory tests that are illustrated in Fig. 8 showing the ratios with standard deviations.

Comment: * Fig. 10: This figure is a copy of Fig. 7 in Baumgardner et al., 2014, except that the label writes "Particle size = 2 μm" instead of "Particle size = 1 μm". In addition, this calculation seems to be done for the CPSPD instrument which has a different instrument geometry ("off-axis laser" in the CPSPD in contrast to a "on-axis laser" in the CAS-POL).

Answer: We have removed Fig. 10 since the new Fig. 9 illustrates the variability adequately with no need for model simulations.

Comment: * p9315 l6-8: The link between this sentence and the previous sentence is unclear.

Answer: This section has been rewritten

Comment: * p9315 l8: How was the uncertainty of 50% estimated?

Answer: This is based on the laboratory measurements that show this amount of variability when measuring dust and ash particles.

Comment: * p9315 l12: It remains unclear were the uncertainties for volume concentration and median volume diameter come from.

Answer: The root sum square RSS of error propagation that we describe is a well-accepted method to derive uncertainties. We have just used the estimated uncertainties in sizing and counting to derive the uncertainties in volume concentration and median volume diameter.

Comment: * How were the backscatter channels calibrated?

Answer: We have added to the appendix that polystyrene and crown glass beads were used to calibrate the backscatter channels with an additional clarification that the polarization channel is not calibrated since there is currently no reference particle that produces a known intensity of S polarization

Comment: * How do the presented "polarization ratio" values from CAS-POL (with values around 0.25) compare to the "polarization ratio" values (0.30-2.50) presented by Glen and Brooks (ACP, 2013)?

Answer: The polarization ratios of Glen and Brooks ranged from 0.3 to 2.5. As with the CAS-POL ratios reported in our study, the S signals are not calibrated to a reference but were adjusted to produce approximately the same amplitude as the P signals when measuring PSL and crown glass beads. This means that the ratio is only a relative measure of asphericity. We have emphasized throughout the manuscript that the

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comparison of the vertical profiles between the lidar and CAS-POL is only meant to determine if the trends are similar. To this end, we have normalized the values from the lidar and CAS-POL to the maximum values found in the dust layer and added this figure to highlight that the trends are in good agreement except for the dip in the profile of the lidar seen at 3700 m. The disagreement in this part is mainly related to the variability of the lidar depolarization profiles at this altitude during the morning as observed in Figure 3 and to differences in the temporal sampling (lidar profiles are 30-min averaged whereas CAS-POL data are instantaneous). We further emphasize that co-located measurement in space was not possible so that much of the difference is likely due to the inhomogeneous nature of the dust layer.

Comment: Some minor issues: * p9294 l11-14: "Most comparisons, such as those reported ..." That is not correct. Weinzierl et al. used the size distribution and refractive index derived from multiple instruments together with an optical model to calculate extinction coefficients which were then compared to directly measured extinction coefficients from lidar. Therefore, quantitative intercomparison not only qualitative intercomparisons were done.

Answer: We have rewritten the introduction according to the comments of Reviewer 1 and this part is no longer included.

Comment: * p9296 l16: Nomenclature is not consistent. Alpha is already defined as extinction coefficient, but here it is also used as Angström exponent.

Answer: Nomenclature has been revised throughout the document and the symbol AE is used for the Angstrom exponent.

Comment: * p9306 l25-26: Not quite correct, as AERONET covers up to a radius of r=15μm

Answer: We now compute volume concentrations up to 30 μm and have removed the red curve from Fig. 6b

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Comment: * p9307 l3: Is the imaginary part of refractive index set to zero?

Answer: Yes, and we clarify this by restating that the refractive index is set to 1.54 –i0.0

Comment: * p9308 l14: 5d should be 5e

Answer: Figure 5 has been modified.

Comment: * p9308 l16: EOD not defined (it is defined later in the appendix)

Answer: EOD stands for Equivalent Optical Diameter. It is now included in the manuscript.

Comment: * p9308 l21: 680 nm does not fit to 658 nm in sect 2.3

Answer: The 680 nm is correct. We have change 658 to 680 in Sect. 2.3

Comment: * p9314 l2: There are no dashed lines in Fig. 9a.

Answer: Former figure 9 is now figure 8. This sentence has been rewritten (lines 686-687):" We can see that particles with different sizes have the same scattering cross section."

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C4306/2015/amtd-8-C4306-2015-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., 8, 9289, 2015.