Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-380-RC4, 2016

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "HSRL-2 aerosol optical measurements and microphysical retrievals vs. airborne in situ measurements during DISCOVER-AQ 2013: an intercomparison study" *by* Patricia Sawamura et al.

Anonymous Referee #4

Received and published: 6 July 2016

General comments: Sawamura and coauthors present a comparison of aerosol microphysical and optical properties obtained from airborne in-situ and lidar measurements. The dataset is unique, with over 700 vertically resolved profiles of aerosol microphysical properties, which makes this comparison of techniques more robust than those previously published in the literature. The topic is sound and of interest for the scientific community. However, there are many clarifications and modifications that have to be adequately addressed before publication in ACP. The overall manuscript structure is not well defined, which makes the manuscript difficult to read and the focus is often lost. All the retrievals explanations should be included in the methods section (avoid

methodological aspects in the results section). In addition, small and individual sentences make up a paragraph. These topic-related sentences could be combined in a single paragraph to make the reading more fluent. There are too many tables and figures, I would suggest to keep just the important ones.

Specific comments

Page 2, line 20-34: Since the main objective of the paper is the evaluation of lidar retrievals with airborne insitu data, some discussion should be included here about what have been done in the past. For example, Granados-Muñoz et al. (2016) compared aerosol volume concentrations retrieved from Lidar measurements with airborne in-situ size-distributions. The paragraph in page 3, line 10 about Müller et al. (2014) could be moved here for consistency. Granados-Muñoz et al., A comparative study of aerosol microphysical properties retrieved from ground-based remote sensing and aircraft in situ measurements during a Saharan dust event, Atmos. Meas. Tech., 9, 1113–1133, doi:10.5194/amt-9-1113-2016, 2016.

Page 3, line 14: Why hygroscopic adjustments were not necessary?

Page 3: sections 2, 3 and 4 headings could be improved. . .

Page 5: aerosol properties are measured in situ onboard the P-3B aircraft. . .

Page 5, line 5: replace "mum" by "μm".

Page 6, line 1: How is the RH<40% achieved? nafion dryer?

Page 6, line 1: State the actual RH inside the dry nephelometer (mean and std)

Page 6: How well do the two nephelometers compare when measuring at RH<40%?

Page 6, line 2: I see one major problem here, and it is the calculation of the gamma parameter with only two RHs. Additional discussion about the errors and uncertainties introduced with this approach should be included here. This will be very important for deliquescent aerosols.

Page 6, line 13: The authors refer to aerosol backscattering instead of scattering, right?

Page 6, Line 18: Are the PSAP wavelengths correct?

Page 6, line 21: A reference supporting the non-hygroscopic enhancement in the absorption coefficient should be included here.

Page 6, line 24-Page 7, line 8: The UHSAS and LAS instruments overlap in the range 0.09-1 μm, have the authors compared the size distributions in the overlapping region? Have these instruments been intercompared with more robust instrument like SMPS?

Page 7, line 10: only at 550 nm?

Page 7, lines 14-15: This is a repetition...

Page 7, lines 23-28: These two paragraphs are confusing; the first one ends with "... mdry values are unknown." and the second one starts with "Once mdry is determined...". How is mdry determined? It is not said until section 5.2 in page 8! This section is jumpy and messy...

Page 8, line 30: nephelometer

Section 5.2: why the size distribution measured with LAS is not used? The nephelometers are sampling up to 5 μm (inlet cut-off) but the size distribution used is restricted to diameters < 1 μm.

Section 5.2: The UHSAS is calibrated using AS (m = 1.53) as stated previously, and then this AS-calibrated size distribution is used to retrieve mdry. Thus, from the reviewer's point of view, the mdry obtained should be seen as an "effective" refractive index able to reproduce your optical measurements. Therefore, the consistency of the insitu measurements (as stated in page 9, line 10) is not demonstrated.

Figure 3: it would be nice to see the ambient RH profile as well.

Section 5.2 and Figure 3: The authors should keep in mind that the scattering coeffi-

cient at ambient conditions is not a measured variable, is a calculated one (and likely with a high uncertainty due to the gamma fit).

Page 9, last paragraph: the symbols for the extinction and backscatter coefficient should be introduced earlier. The computed backscatter coefficient, refer to 180° or to integrated hemispheric backscatter?

Page 10, 3rd paragraph: repetition

Page 10, line 17: why the coarse mode is not studied? The notation in graphs and in the text should make clear that the surface and volume concentrations refer only to the fine mode.

Figure 4: Why these profiles have been shown?

Figure 5: In-situ also refers to fine mode only

Page 10, line 25: do the authors have any explanation about the different agreement found for the surface and volume concentrations?

Page 10, line 27: Any explanations about why the bias was larger in California than in Texas? I would suggest presenting the results combined with the discussion.

Section 6.2: It would make more sense to compare the measured in-situ extinction coefficient (scattering + absorption) with the HSRL-2 extinction coefficient, rather than the retrieved insitu extinction coefficient...

Figure 8 can be omitted.

Section 7: As mentioned before, it would be better to present the results together with their discussion.

Page 13, line 23: initially?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-380, 2016.