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Atmospheric Chemistry and Physics Discussions

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 #2

Received and published: 22 June 2016

The title and the abstract describe very well what the intent of the manuscript is. The abstract is very well written. The work overall is very thorough and should definitely be published.

This reviewer believes the authors' intent is to ultimately validate the HSRL-2 retrievals so they can be applied globally to a similar spaceborne lidar system which is being considered for the ACE (Aerosol-Cloud-Ecosystem). The strong point being that these measurements will be representing ambient conditions (i.e. the conditions relevant for radiative transfer) and can be global, whereas the in situ measurements (while more

detailed) are neither global nor taken at ambient conditions. This reviewer feels it would be important to make this point in the introduction of the manuscript.

Many corrections that are well described in the manuscript need to be applied to the in-situ data before they can be compared to the in-situ data. The quantitative comparisons and trends are very impressive and revealing. It would be helpful to mention how well extinction can actually be measured with in-situ data. Schmid et al. (2006) some years ago have arrived at such an estimate by looking at a large number of campaigns.

This reviewer agrees with what seems to be one of the main conclusions of the manuscript. "Regardless of the number of uncertainties that can affect studies like this one, we have demonstrated that the HSRL-2 retrievals of fine mode aerosol size parameters are well correlated to in situ measurements. Further work is still necessary in order to more effectively quantify the net effects of such uncertainties in comparison studies of this kind." In other words, are more comparisons needed to arrive at a definitive validation of the HSRL-2 aerosol microphysical retrievals? AOD comparisons with ground based sunphotometry have also been shown and look impressive. Should this be emphasized more?

Specific Comments:

Page 1, Line 19: This reviewer believes the McFarquhar reference is missing. These references are just examples so should be prefaced with "e.g."

Page 2, Line 20: This reviewer suggests using "focused on" instead of "in"

Page 3, Line 8: This reviewer suggests using "non-representative" instead of "unrepresentative"

Page 3, Lines 10-14: Text makes it sound like only 2 spirals were flown in TCAP. Which sounds incorrect. Please also explain that HSRL-2 was only deployed in phase 1 of TCAP. It would be nice to mention the in situ aircraft by name (DOE G-1) and perhaps use the corresponding reference (Schmid et al., 2014). Definitely must explain

why hygroscopic adjustments were not necessary. Also please quantify the level of agreement instead of just saying “good agreement”. It might also be worth mentioning comparisons done in TCAP Phase I between HSRL-2 and another remote sensing method (4STAR, Shinozuka et al., 2013a, b).

Page 4, Line 4: Missing “the” before NASA

Page 5: Line 5: micron symbol got lost

Page 6, Line 25: Please specify that this is the airborne version of UHSAS mounted in the free airstream. Who good is sizing of UHSAS near 1 micron?

Page 8, Line 22: "most" leads me to ask the question "why not all" data are publicly available.

Page 9, Line 8: The size range of UHSAS is a subset of the size range the Neph and PSAP sees unless a size cut-off of 1 micron was used which is not mentioned. You need the LAS data for the larger sizes. Have you done a cumulative scattering calculation as in Kassianov et al. to see what percentage of scattering the UHSAS is missing?

Page 11, Line 25: At this point in the manuscript this reviewer was very surprised to learn that only the fine mode had been considered. Finding this out on page 11 is too late and a bit frustrating.

Page 12, Line 4: At this point you need to say that the cut-off is likely not sharp at exactly 5 microns. Rather it probably has an S-shape. Some particles larger than the cut-off will still make it through the inlet, whereas some that are smaller than the cut-off won’t. So is 5 micron the 50% efficiency point?

Page 12, Line 4: Very late in the manuscript (p.12) to say that supermicron data from LAS were not available for DAQ TX.

Page 12, Line 11. Good to see this improvement!

Page 12, Line 14. Unclear. Increase of what?

Page 20, Line 25. Some strange symbols

Figure 2: Should use a different symbol than alpha as this is used already for extinction. In 3 beta +2 alpha.

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