

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 #3

Received and published: 1 April 2017

This manuscript describes a very important and valuable study which compares the HSRL-2 retrieval algorithm to in situ airborne profiling measurements. This type of study is essential for the validation of the HSRL-2 algorithm and the understanding of its abilities/limitation. Also pointing out the need for in situ optical aerosol measurements at ambient conditions (RH and coarse mode aerosols). The paper is very well written and the plots are clear. The work is carefully done, including stuffiest statistics. I recommend publishing after addressing these comments:

General comments:

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*P8 Line22-25: The authors provide an explanation for the discrepancy between the HSRL-2 and the calculated in situ, addressing the presents of course mode aerosols. The reviewer agrees this could be a possible explanation and the authors supports their explanation nicely using the AERONET data. However, it is not clear what type of aerosols do the authors suggest could explain this discrepancy, given that depolarization ratio at 532 nm wavelength greater than 5%,was screened (meaning the authors have removed dust particles). Also why the Texas data has accentuated deviation compared to California? What should be the size of these course mode aerosols? If incorporating the 5 μ m LAS data was not stufiest to explain the discrepancy. Could there be any other explanation for this discrepancy?

*P6 Line 18 "We Assume that the aerosol RI is wavelength independent" (450-700nm) , this assumption requires a short justification, especially in the lower wavelength range, close to UV. It seems like a reasonable assumption for mostly scattering aerosols (as shown in this study), but needs to be mentioned. Were there any absorbing aerosols present in one of the sites? (e.g. BB aerosols)

*Was there any chemical information of the aerosols on the airplane platform or ground base that could support the conclusions?

*The authors report that the HSRL-2 vertical profile are products within 30 min matched to the spiral data, was the aerosol population well mixed, for this comparison to meaningful?

*P6 line 25: "The entire size distribution shifts towered larger diameters", it seems like reasonable assumption, depending on the type of the aerosol. What is the error associated with this assumption?

Specific comments:

*Suggest to make figure 6 more clear by moving the headline of DAQ-CA and the DAX TX outside (same comment for Figure 8)

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*P8 Line 7: Please delete brackets from Brock et al., 2016

*Figure 7: suggest to modify Reff to: R_{eff} (subscript the eff)

*P9 line 14: “LAS (0.09 - 5 μm diameter) instead of the UHSAS (0.09 - 5 μm diameter)”.
There is a typo: the same range is reported for both instruments.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1164, 2017.

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