Author comments to Referee #4 (RF4)

Response to general comments:

The authors would like to thank RF4 for his/her thorough review of our manuscript. All his/her comments and suggestions were carefully considered and addressed.

Response to specific comments:

1) 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.
   Fixed. Reference was included.

2) Page 3, line 14: Why hygroscopic adjustments were not necessary?
   Please refer to item 8 of the authors' comments to RF1.

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

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

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

6) Page 6, line 1: How is the RH< 40% achieved? nafion dryer?
   Correct. A nafion dryer is used to dry the aerosol sample stream.

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

8) Page 6: How well do the two nephelometers compare when measuring at RH<40%?
   In general, the raw data from the two nephelometers usually agree within 2-5/Mm. The nephelometers were corrected for any offset measured when running at the same RH, which was usually less than 2/Mm.

9) 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.
   A discussion on that topic has been added to the revised version of the manuscript.

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

11) Page 6, Line 18: Are the PSAP wavelengths correct?
    The wavelengths have been corrected.

12) Page 6, line 21: A reference supporting the non-hygroscopic enhancement in the absorption coefficient should be included here.
    Fixed. Also, by suggestion of RF1, we included the values of the dry single scattering albedo measured during the campaign.

13) 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?
    The UHSAS measurements have been compared to the LAS measurements and good agreement has been observed in the overlap region. However, a systematic comparison among the three instruments has not been performed.

14) Page 7, line 10: only at 550 nm?
    Correct. The wet nephelometer only measures at one wavelength: 550 nm.

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

16) 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. . .
    Fixed.

17) Page 8, line 30: nephelometer
    Fixed.

18) 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.
    The LAS size distributions were available only during DAQ CA. The results obtained with the LAS data are currently discussed in Section 6.2. Per RF2 suggestion, the authors will make sure to mention the LAS measurements earlier in the paper.

19) 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.
A note has been added on that and the sentence has been removed.

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

21) Section 5.2 and Figure 3: The authors should keep in mind that the scattering coefficient at ambient conditions is not a measured variable, is a calculated one (and likely with a high uncertainty due to the gamma fit).
That is correct. A discussion on the way gamma is calculated has been added to the manuscript.

22) 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?
Fixed. The computed backscatter coefficient refers to 180°.

23) Page 10, 3rd paragraph: repetition
Fixed.

24) 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.
The coarse mode could not be studied in detail due to experimental limitations. We note that in the manuscript.

25) Figure 4: Why these profiles have been shown?
The comparison of profiles has been shown to demonstrate the sensitivity of the lidar retrievals to small changes in the vertical distribution of aerosols.

26) Figure 5: In-situ also refers to fine mode only
Correct. A note has been added.

27) Page 10, line 25: do the authors have any explanation about the different agreement found for the surface and volume concentrations?
At this point we can only speculate. In simulation studies the surface-area usually is the variable which is better reproduced. In our study since the in situ measurements also have uncertainties due to the method used to adjust them for hygroscopicity, it becomes difficult to establish the cause for the difference in agreement that is observed.

28) 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.
That is one of the points which we investigate and report in the manuscript.

29) 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. . .
    Due to the strict agreement threshold imposed when retrieving the ambient refractive index, the measured and computed extinction at 532 nm agreed within 1%. Since the only in situ ambient measurement available is the extinction coefficient at 532 nm, we chose to show the computed properties in all plots, instead, for consistency.

30) Figure 8 can be omitted.
    Figure 8 has been moved to the Supplemental Material.

31) Section 7: As mentioned before, it would be better to present the results together with their discussion.
    The Summary and Discussion have been combined into one section.

32) Page 13, line 23: initially?
    That section has been restructured.