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Interactive Comment

Interactive comment on "Comparison of surface and column measurements of aerosol scattering properties over the western North Atlantic Ocean at Bermuda" *by* R. P. Aryal et al.

Anonymous Referee #1

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Aryal et al. compare optical measurements of aerosol particles performed at the surface and in the overlaying column at Bermuda. Sun and sky radiometer measurements of AERONET and its retrievals are used as well as MPL lidar measurements of the atmospheric column, while a ground-based nephelometer is used to measure the aerosol scattering coefficient at one wavelength alternating between PM_1 and PM_{10}. A time period of approx. 6 months from January to June 2009 (with interruptions) was analysed. The authors find a "high" correlation between surface and columnar measurements, while the remaining differences were explained by vertical structures of aerosol concentration and composition. A decrease in submicron scattering (columnar and surface) with increasing wind speed was attributed to an increase in coarse mode dominated sea spray aerosol, which should be expected at this oceanic site.

Aryal et al. present in their manuscript very little new scientific results or insights. Most of what the authors present is a series of correlations of one data set at a specific site, which alone cannot be regarded as scientifically significant. Most of the results conform to our current understanding and can be found in standard text books (e.g. the dependence of sea spray production on wind speed) or existing scientific literature. With regards to the **scientific significance**, as defined by Atmospheric Chemistry and Physics (ACP), the manuscript offers no substantial new concepts, ideas, methods or even new data to the community. The discussion of the results is also conducted in a rather cursory way. For example, the authors mention the effect of aerosol hygroscopicity and stratification when discussing the observed differences between columnar or in-situ measurements, however, the discussion is very short and incomplete, omits recent findings and also fails to discuss other possibilities like aerosol partitioning effects. Therefore, the **scientific quality** can only be rated as poor. In contrast to the very short discussion on the results omits current literature findings, the paper is quite excessive with the amount of figures, which are often repetitive and/or unnecessary. Thus, the **presentation quality** as defined by this journal can also only be regarded as fair. The work could be improved by adding a substantial amount of further analysis (see comments below), however this would result in a completely new manuscript.

In conclusion, the manuscript as presented at its current stage, brings no new and significant findings to the community and together with the comments made above, I have strong doubts that this paper fulfils the quality requirements of ACP. Therefore, I have to recommend that this manuscript is rejected for publication in ACP.

Major comments

- Section 2.1: Please precisely describe what kind of common inlet (any size cut?) was used at the station and if driers were used before the aerosol was measured in the nephelometer. The losses within this inlet have to be determined using particle loss calculations and this information should be included in the paper. I have doubts that you will measure PM_{10} particles if for example you are using 1/4-inch tubing in front of the nephelometer even with just a few bends.

- Section 2.2.3. and Equation 2: It should be clarified if the AOD is calculated by using the number size distribution taken from the same AERONET retrieval, as described here (also I can't find the actual discussion in the results section). From my point of view, it does not make sense to calculate an extensive parameter using the columnar size distribution which was retrieved from the same (extensive) measurement to which the result will be compared to. This is a circular path and is therefore not valid.

- Page 1796, Line 10: As stated here, the absorption coefficient was also measured at the site, but is neglected when comparison is made to the ambient extinction coefficient. This is not justified and the absorption should be accounted for when comparing the in-situ value to the ambient extinction coefficient. Within the Mie calculations, I also assume that you have included the imaginary part of the complex refractive index. Please clarify.

- Sect 2.3.2.: A discussion on the uncertainty of the MPL system and the Klett-inversion is missing and should be added.

- Please precisely describe what kind of linear regressions were performed within this work. The presented regressions do not appear to account for the uncertainty in the x-direction or include any weights/uncertainties of the different data points. I have the impression that the negative slope of the linear fit in Fig. 6 is mainly

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driven by a few outliers, which is underlined by the low correlation coefficients. In general, it would be better to show squared correlation coefficients within the entire paper. The correct regression-technique is especially important for the fit presented in Fig. 1b. Here, it seems that the slope is as well biased by a few large outliers. A bivariate weighted fit has to be performed here as well.

- The conclusion demonstrates in its shortness the moderate scientific gain of this manuscript. It is not clear to me what we can actually learn from this work and what the real take-home message is. Quotes like "highly correlated" are not justified if you consider that the best R^2 observed in the work is about 0.42.

- The lidar data and radiosonde data could be used more intensively. For example by systematically looking for elevated layers or by investigating the effects of hygroscopic growth at elevated layers. Thus, the radiosonde data should be used during the analysis of the lidar data.

- As mentioned above, the amount of figures is quite excessive, especially the lidar profiles (Fig. 2 and 3). Further, the many unneeded time series (Fig. 5a, Fig 7a and especially Fig. 8) can be combined and/or removed. They are often just discussed with a single sentence.

- Figure 9: Could the differences between MPL and in-situ be explained by systematic errors in in-situ (particle losses) or the lidar (Klett inversion)? The rather constant ratio for the low RH below 70% is striking and would point towards this direction. As mentioned in the manuscript, one would expect an increase of the shown ratio with increasing RH, however, a decrease is seen from 80 to 90% RH. I would guess that this is caused by the low amount of comparable cases (as given in the Figure). At high RH, the MPL retrieval could also be influenced by small and thin clouds.

The work could be improved by adding a substantial amount of further analysis. A few

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ideas:

- Further and more intensively using the radiosonde and lidar data to investigate the effect of hygroscopic growth within the entire planetary boundary layer (PBL) and not just the lowest point at 400 m.
- The trajectory analysis could be used to investigate the differences found in Sect. 3.4 when the lidar extinction coefficient is being compared to the in-situ value.
- A marine hygroscopicity of f(RH) could just be assumed or calculated (if chemical proxy data is available) and included in the analysis.

Minor comments

- The section heading 2.2.1 is not needed and can be deleted, since no 2.2.2 is following.
- Section 2.2.: Please mention the mean RH inside the nephelometer.
- Page 1804, Line 25: Please state the slope and intercept.
- Equation 1: Using the absolute value when calculating the correct scattering coefficient can bias the true scattering coefficient towards larger values. For example, if the nephelometer will result in negative scattering coefficients (which can happen at very low concentrations due to the uncertainty of the internal nephelometer calibration (dark current, offset, etc)). In addition, it should be $b_{size-samp}$ in the Equation and not only $b_{size-surf}$.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 1791, 2014.

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