Response to the comments provided by M. Tesche

We the authors thank Dr. Tesche for his attention to our study and his insightful critique of the manuscript. We have done our best to address each of his points as detailed below.

Note: All comments by M. Tesche are in *italics*. All author responses are in normal format.

#1. *To avoid confusion, I suggest the use of the term marine aerosol instead of sea spray aerosol (SSA). If not defined otherwise, the latter refers to a production process (primary marine aerosol) while the former is associated with a location of origin and is commonly used by people that work with aerosol optical parameters. It should be borne in mind that a lidar will detect all aerosols within your layer of interest and not just the ones that are actual sea spray (i.e., of primary origin). I am aware that SSA is the term commonly used by the sea spray aerosol community so at the very least this should be clarified when the SSA acronym is introduced in the manuscript.*

We have decided to retain the acronym SSA as it is now widely used in the sea spray remote sensing/modeling community. Concerning the point that "a lidar will detect all aerosols", we believe there is a misunderstanding. Description of the scene selection algorithm (in section 2.4) clearly states that "we start with clean marine aerosol…" Although we acknowledge that some natural continental aerosols and human-induced pollution can be miss-classified by CALIOP as clean marine, and caution readers when interpreting data near coastlines, suggestion that *all* aerosols within the layer of interest are included in the calculations is incorrect.

#2. *Following up on the previous comment, Figure 2 shows that SODA retrieves increased AOD for marine aerosol in the Yellow sea, around the Indian subcontinent, and to the west of the South American and African landmasses. This seems to be an artifact that is related either to a strong contribution of non-marine aerosols or an effect of clouds. In the same figure, there appears to be at best a weak relationship between wind speed and AOD; those regions generally expected to have higher wind speeds do not show markedly increased AOD.*

We believe the reviewer means Fig. 1, as Fig. 2 is reporting the lidar ratio, not the AOD. The increased AOD around the Indian subcontinent was identified as a probable artifact that was mentioned in the manuscript (section: Results, paragraph 1) and interpreted as contamination of continental pollution. The manuscript text has been revised to more clearly state this point: "The region around the Indian subcontinent and over the Bay of Bengal is believed to be just a retrieval artifact."

#3. *This seems to be an artifact that is related either to a strong contribution of non-marine aerosols or an effect of clouds.*

Clouds always pose a challenge for satellite retrievals of aerosols. We have to the best of our ability, removed clouds by our described layer screening technique (see the

manuscript section 2.4). We have used the information from the CALIOP vertical feature mask to only analyze columns containing one aerosol layer (classified as clean marine) and no identified clouds. Nevertheless, hydrated aerosols near the cloud edges may lead to biases in CALIOP retrieved AOD. We attempt to mitigate this by requiring SODA aerosol retrievals to make up 70% or more of the 5 km CALIPSO aerosol layer product. In other words, there must be more than 10 retrievals in the 5 km averaging swath. This means that for any 5 km aerosol products that we use in our analysis, there are at least 11 CALIPSO shots (out of a possible 15) that make up the reported values. This ensures that aerosols near large clouds will not be included in the retrieval.

#4. *Generally, a quantitative discussion and critical assessment of the manuscripts findings is missing. The AOD obtained with the SODA algorithm must also be compared to direct measurements of AOD. Therefore I strongly suggest to validate the SODA AODs with actual measurements at suitable AERONET stations.*

The SODA method has been extensively evaluated against High Spectral Resolution Lidar (HSRL) retrievals as well as MODIS observations (Josset et al., 2011; 2010; 2008). We strongly believe that the evaluation of the SODA algorithm against AERONET data is outside the scope of the current paper.

#5. *In Section 2.4, the text from page 221, line 8 to page 222, line 14 simply describes the same procedure as the one from page 222, line 15 to page 223, line 11. I suggest the authors harmonize these descriptions omitting one of them.*

We believe that, for the sake of clarity, the text should remain as is. The first section referred to in the comment describes the scene selection component of the quality control algorithm (i.e., layer type and conditions of layer selection), whereas the text referred to in the second part of the comment explains the rest. We think the current state of the text helps readers easily understand what steps have been taken in the quality control algorithm.

#6. *The description of the CALIPSO data retrieval lacks critical references regarding the instrument (Winker et al., 2009), the feature-finding algorithm (Vaughan et al., 2009), the lidar-ratio selection algorithm (Omar et al., 2009), and the extinction-coefficient retrieval (Young and Vaughan, 2009). These references should be given in Section 2.1 and not in the discussion of the findings.*

The references have been added to the revised manuscript.

#7. *I found three different time periods for which data were considered: 2007 to 2010 (Introduction), Dec 2007 to Dec 2009 (Abstract, Conclusions), and Dec 2007 to Feb 2011 (Section 2.4). Please clarify which one is correct.*

The text has been fixed; the proper dates are from December 2007 to February 2010.

#8. *I suggest the authors move the supplementary material into the actual paper or at*

least add the occurrence rates of the different wind speed regimes to Table 2.

We have added the number of retrievals and the percentages to Table 2.

#9. *In order to strengthen the authors' findings, it seems worthwhile to use these different size distributions to investigate by means of scattering calculations whether they would lead to different lidar ratios.*

These calculations were done with measured size distributions near Hawaii by Sayer et al. (2012). We have inserted the reference where appropriate.

References:

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