

Interactive comment on “A new study of sea spray optical properties from multi-sensor spaceborne observations” by K. W. Dawson et al.

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The manuscript presents a method to determine extinction-to-backscatter (lidar) ratios of marine aerosols from combined measurements of CALIPSO and CloudSat over the oceans during selected atmospheric conditions. While I think the general idea of retrieving lidar ratios is interesting and potentially useful, the authors fail to convince me that their findings are of consequence due to a number of significant issues with the method used. For example, the manuscript lacks a critical assessment of how representative SODA derived AOD is as well as a thorough comparison of the retrieved lidar ratios to actual measurements of this parameter (which are available in the literature). I suspect that the authors are not experts in lidar measurements and I therefore feel they

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might appreciate some input from the lidar community to strengthen the basis and findings of the manuscript. The following comments follow the structure of the manuscript and are not ordered according to any priority.

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.

The authors should extend the literature review on the lidar ratio of marine aerosols. Currently, most references are of a rather theoretical nature (i.e., modeling studies that obtain lidar ratios from measurements of the particle size distribution or lidar ratios obtained from measurements with passive sensors). Only one reference is given that presents a direct measurement: *Ansmann et al.* (2001). However, this study did not focus on measurements of marine aerosol. While the authors state that direct measurements of the lidar ratio of marine aerosols are possible with HSRL (page 217, line 25), they fail to mention that this is also possible with Raman lidar (RL). More critically, the authors have omitted a number of key references from the literature which present direct measurements of the lidar ratio of marine aerosols using well-calibrated HSRL or RL. Common values at the CALIOP wavelength of 532 nm are 23 ± 3 sr (RL, North Atlantic, *Müller et al.* 2007), 23 ± 5 sr (RL, Indian Ocean, *Müller et al.* 2007), 18 ± 2 sr (RL, Equatorial Atlantic, *Groß et al.* 2011), 20 ± 5 sr (HSRL, North Atlantic, *Burton et al.* 2012), and 18 ± 5 sr (HSRL, North Atlantic, *Groß et al.* 2013). I suggest that the authors revise the introduction to include a thorough review of **directly measured** lidar ratios of marine aerosol. Furthermore, the findings of the manuscript should be discussed in the context of these observed values.

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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. I would assume that 500-nm AOD for conditions considered in this paper (only marine aerosols present in a single layer close to the surface, no elevated layers, no clouds) varies between 0.05 (*Kaufman et al.*, 2001) and 0.10 (*Smirnov et al.*, 2009, 2011) with a tendency towards the smaller values. For instance, *Smirnov et al.* (2002) report a mean AOD of 0.07 at 500 nm for marine aerosols observed at sites in the tropical Pacific. *Smirnov et al.* (2002) have also presented a review of AOD measurements over the oceans; however, it should be noted that measurements at coastal sites are not synonymous with measurements of marine aerosols. I recommend that the authors take some time to look at Sun photometer observations from remote islands or ships (*Smirnov et al.*, 2002, 2009, 2011) given that these will provide an idea of the values they can expect in pristine marine environments. It is noteworthy that these values are much smaller than those presented in Figure 1 and Tables 1 and 2 for SODA or given as a common value for marine aerosol in the introduction to this manuscript. In this context, the CALIPSO-derived AOD seems more realistic. That the representativeness of SODA AOD is completely overlooked by the authors is the strongest weakness of the manuscript given that it is at the root of the retrieved lidar ratios presented. Therefore I strongly suggest to validate the SODA AODs with actual measurements at suitable AERONET stations. Without an assessment of the accuracy of the SODA AOD used (or at least some form of sensitivity analysis) there is absolutely no value in the retrieved lidar ratios. Further, it is very likely that a decrease in the AOD used in the retrieval would lead to decreases in the lidar ratio observed, moving them towards the values obtained directly with HSRL and RL. This issue should be discussed with the same depth as the manuscripts current discussion on the influence of the integrated attenuated backscatter coefficient on the accuracy of the retrieval.

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

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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.

The authors should be aware that CALIOP AOD is not considered as a reliable operational output. This also needs to be stated in the manuscript. A comprehensive overview of CALIOP-derived AOD can be found in *Winker et al.* (2013). CALIOP AOD can only be used for cloud-free profiles for which a surface signal is detected. This information is provided in the 5-km aerosol profile product. I assume that using CALIOP observations according to the availability of SODA AOD intrinsically accounts for cloud-free conditions and for a surface signal being detected. Is that correct? If so that should be mentioned somewhere in Section 2.4 (Data selection method). If not the presence of clouds above the marine aerosol layer would increase the value of the total integrated backscatter coefficient.

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.

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 authors use the terms integrated backscatter and integrated attenuated backscatter. My guess is that this is the parameter `Column_Integrated_Attenuated_Backscatter_532` provided in the CALIOP aerosol product. It is not clear from the text if that is the case or if the authors obtain this

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parameter themselves. It is also not stated in Section 2.1 which CALIPSO products are included in the data analysis.

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.

Please elaborate why there would be more misclassification in the low wind speed regime (page 225, line 23-26 and page 227, line 15-19). A lot of these cases occur over the southern oceans where the influence of anthropogenic pollution is almost negligible. I find it more convincing that the decreased signal-to-noise ratio of the CALIOP measurements during situations with low aerosol load introduces noise that leads to misclassification? Misclassification along the coastlines is more likely due to the effect of surface type on the aerosols detection algorithm (see, e.g., *Omar et al. 2009* or *Kanitz et al. 2014*).

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. If I understand this information correctly, 90% of all cases (wind speeds between 4 and 15 m/s) show lidar ratios of $S = 25 - 27$ sr. These values are slightly higher than the ones measured with RL or HSRL (20 ± 3 sr) but show a comparable variation of only a few steradian. While it is conceptually comprehensible, I find it somewhat odd to deduce a wind-speed dependence of the lidar ratio from the remaining 10% of observations. Also, not much of a wind-speed dependence is left if the value for the lowest wind speed (that shows the highest standard deviation) is omitted. It would also be of interest for the readers to see the absolute number of cases in the different wind-speed regimes in the tables together with the mean and median values of the retrieved lidar ratios.

I understand that wind-speed dependence is a major issue for the sea spray community. As a lidar expert, I will add that the presentation of more or less direct and systematic measurements of the lidar ratio of selected aerosol types is already an

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achievement that warrants publication—if it is done in a convincing way. Consequently (and as a reply to a comment by one of the official reviewers), I have to say that from a lidarist's point of view there is absolutely no value in a parametrization of the lidar ratio with wind speed!

Based on the values obtained from direct measurements of the lidar ratio of marine aerosols given above, it is my personal opinion that changing the value used in the CALIPSO retrieval by a few steradian would not lead to any improvements. I am afraid to say that there are many stronger factors (clouds in a profile, signal-to-noise ratio, solar background, internal calibration, ...) that have an influence on the quality of CALIPSO products to seriously consider such a minor change. Since the extinction coefficient of marine aerosol is generally small, a slightly incorrect (if so!) choice of the lidar ratio will only have a small effect. The authors have to keep in mind that the lidar ratio can also show a dependence on relative humidity—especially for strongly hygroscopic aerosols. It would be complicated to disentangle such a dependence from the one that is due to wind speed.

The authors state that the size distribution of marine aerosol may change with wind speed. 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.

The authors state in the conclusions that the obtained lidar ratio of 26 sr leads to a better agreement of the AOD obtained through SODA and CALIPSO measurements alone. Here the results obtained with SODA should also be challenged since this is also of great relevance. Following up on an earlier point, I suggest that the authors add a comprehensive assessment of the SODA AOD for the aerosol situation considered in this paper. This could be done in the form of a sensitivity study or a comparison to AERONET observations. It is no surprise that the authors obtain higher lidar ratios if SODA overestimates the AOD of marine aerosols. The authors spend the entirety of Section 4 discussing only one of the two input parameters used in their retrieval. Why

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is the AOD as the second input parameter not assessed in the same way? This is an obvious flaw that should be corrected.

Assuming that the conclusions of the paper still hold after the authors check for the issues raised above, I also suggest that the authors consider changing the title of the manuscript. For example, *Spaceborne observations of the lidar ratio of marine aerosols* would raise much more interest in the lidar community. From a lidar perspective, it would still be a strong and innovative paper if the authors omit the focus on the wind speed dependence of the lidar ratio. Especially as I feel that the current evidence is not nearly convincing enough to draw this conclusion.

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