

## ***Interactive comment on “Reconciling aerosol light extinction measurements from spaceborne lidar observations and in-situ measurements in the Arctic” by M. Tesche et al.***

**M. Tesche et al.**

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We thank Referee 3 for his/her comments. We have incorporated them into the revised manuscript. Please find our point-by-point answers below.

### **Overview:**

**This study presents a comparison of extinction coefficients as determined from spaceborne lidar measurements and from ground-based in-situ measurements at Zeppelin station during the year 2008. For this, the authors present here a complex procedure to match CALIPSO and ground-based observations based on**

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**HYSPLIT back trajectories to ensure the comparison of the same air mass. This procedure leads to only 57 overpasses during 2008 (from over 2000 overpasses in that year). The results obtained by the authors show how difficult is to obtain good results in such comparison.**

**I would recommend the authors to focus more on the screening and matching of the CALIOP data, analyzing further the associated uncertainties (averaging height range, intervals along the CALIPSO ground track, time, etc.). Although the number of cases analyzed is very low, it can be presented as the first attempt to compare extinction coefficients from spaceborne lidar and ground-based measurements using this approach. However, the authors need to analyze in depth the uncertainty of their approach and the results obtained.**

We agree with the reviewer that there are many sources of uncertainties in our approach. We also realized from this and the other reviewer comments that we forgot to mention the history of how we came up with our comparison approach. It is in fact the outcome of a continuous refinement of the simple closest approach method which we found to fail in providing physically meaningful comparison cases. Several orders of magnitude of differences were found between the extinction coefficients from in-situ measurements and CALIOP observations when using the closest approach method. Discrepancies were reduced by trajectory matching, considering time delays, cloud screening, etc. This history of refinement is the reason why we consider a factor of about 2 as a very good comparison result. In the manuscript we name all the sources of uncertainty that we identified along the way. However, it is virtually impossible to quantify the individual errors as they all have the potential to make any meaningful comparison impossible. Accounting for the individual effects as best as possible will not ensure a flawless comparison. However, it will be closer to the truth than using rigid schemes like the closest approach. We added the following text to Section 3 to inform the readers about the background of our comparison approach:

*We started our investigation by applying the closest approach method to link CALIPSO*

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observations in the region of interest to coincident dry in-situ measurements at Zeppelin station. While this course of action led to a high number of matches, it did not enable reasonable case-by-case reconciliation of in-situ and remote-sensing data. Differences in the compared aerosol optical properties ranged between two and three orders of magnitude. Perpetual refinement of the comparison procedure as described below showed that the failure in reconciling the different observations in the initial comparison is due to:

1. Physically meaningless comparison scenarios in which no connection can be established between the locations of the ground site and the satellite track during heterogeneous aerosol conditions
2. The inclusion of apparently unrealistic signal spikes into the CALIOP extinction coefficient in case of fixed or inappropriately selected along-track averaging intervals
3. Humidification effects
4. The temporal delay in the observations

The first two points make reasonable comparisons impossible. The latter two can still introduce uncertainties of up to 100%.

**General comments:**

**Page 5695, lines 4 - 8: This paragraph repeats the information on Page 5691, lines 28 - 29 and Page 5692, lines 1 - 4. The Zieger et al. (2013) reference is missing here though.**

We removed this paragraph from the introduction and left it in the description of the instrumental setup at Zeppelin (Section 2.1).

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**Page 5696, lines 6 - 13: The hygroscopicity model was validated with data from the period July – October 2008. Can the authors explain further how this is valid for the whole year 2008? How would the annual variation of the aerosol concentration and properties affect this?**

The model uses hourly size distribution measurements together with daily or monthly chemical composition data collected during the entire year of 2008 to account for the annual variation in the aerosol conditions at Zeppelin. The model performs satisfactory during the evaluation period. Hence, we assume that it will also do so during the rest of the year 2008 given that the required input parameters are adapted to measurements performed during this time.

Based on the suggestions made by the reviewers we investigated if wet scattering coefficients can also be obtained reliably by using the dry nephelometer and PSAP measurements together with scattering enhancement factors derived for a lower, median, and upper estimate of  $\gamma$ -values. We restructured Section 2.2 accordingly to describe this procedure in Section 2.2.2. The results were added to Figures 1, 3, 4, and 5. The description and discussion of these figures has been revised accordingly. We now used the ambient extinction coefficients obtained from applying the scattering enhancement to dry nephelometer measurements for the comparison to CALIPSO findings presented in Figures 4 and 5.

**Page 5696, lines 14 - 15: “Values of  $f(\text{RH}) = 4.30 \pm 2.26$  with a range from 1.5 to 12.5 were found for the year 2008.” To get these values, the hygroscopicity model by Rastak et al. (2014) was used with measurements of dry aerosol size distribution and aerosol composition. How frequent were these measurements? What is the uncertainty of this model? How would this affect the aerosol extinction coefficient for ambient conditions? And the comparison with CALIPSO?**

The details on data availability are provided in Section 2.1:

*The aerosol in-situ instruments at Zeppelin station include a differential mobility par-*

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ticle sizer (DMPS) for measuring the particle size distribution in the diameter range from 10 to 790 nm (time resolution of 20 min), a particle soot absorption photometer (PSAP) for measurements of particle light absorption coefficients at 525 nm (time resolution of 60 min) on a filter, and an integrating nephelometer (TSI model 3563) for measurements of particle light scattering coefficients at the wavelengths of 450, 550, and 700 nm (time resolution of 10 min) (Ström et al., 2003; Tunved et al., 2013).

A high-volume sampler with a PM10 inlet was used to obtain the chemical composition of the Arctic aerosol with time resolutions of one day for sulfate and sea salt and one month for OC/EC during 2008.

Details on the model can be found in Rastak et al. (2014), which has been accepted for publication in ACP. A brief summary of the model performance including the validation of the ambient extinction coefficients is provided in Section 2.2.1 of our revised manuscript. However, using the size distribution to only 800 nm is likely to neglect the contribution of the coarse mode. Previous studies showed that large particles can be responsible for up to 30% of the observed extinction coefficients in the Arctic. This would add an uncertainty of a factor of two to the extinction coefficients obtained with the model of Rastak et al. (2014). To assess the actual underestimation of the extinction coefficient due to not accounting for the coarse-mode contribution we now also derived the ambient extinction coefficient from the dry nephelometer measurements as proposed by Zieger et al. (2010). See also answer to previous comment.

**Pages 5700 - 5702: "Comparison approach" The authors should include information about the uncertainties associated to this approach, e.g.,**

We refined our comparison procedure from the simple closest approach method to increase the likelihood for meaningful comparison cases. We missed to state that our comparison approach was actually the result of several steps of refinement. A description of this evolution has been added to the beginning of Section 3. See also first answer to this review.

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It is futile to quantify the uncertainty associated with the comparison approach as there are too many possibilities that can render a comparison case physically meaningless. We constrained comparisons on a case-by-case basis to the best of our knowledge to ensure the highest possible quality in the reconciliation of the different observation. Simpler comparison scenarios will come with a much higher share of "apples and oranges" comparisons caused by insufficiently accounting for, e.g. atmospheric variability or noisy data. In our case, it is unlikely that a less restrictive comparison approach with the resulting higher number of comparison cases ("better statistics") will be of any advantage as most of these additional cases will consist of physically meaningless comparisons scenarios.

**"We believe that time rather than distance is a better parameter to assess changes in the aerosol properties in the atmosphere." Why?**

Using range as a constraint is the prime limitation of the closest approach method. This method assumes horizontal homogeneity, and thus, limits the number of comparison cases to a certain distance from a site. However, even for the considered cases the method cannot assure that the resulting comparisons are indeed meaningful. For instance, stagnant conditions with low wind speed or atmospheric flow that does not connect the ground site to the spaceborne observation (i.e. along rather than crossing the ground track) would complicate such a procedure. Accounting for such conditions requires the use of backward trajectories as a means of connecting the different locations of observations. Once the connection is established it is the time scale that determines if we can expect conditions for a meaningful comparison of the different observations. See also first answer to this review.

**"A change in the along-track average of the CALIOP extinction profile (i.e., from a range related to crossing trajectories with different starting time at the location of the ground site to a fixed interval) can result in large differences of the resulting mean extinction profile." By how much?**

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To consistently apply the trajectory matching, we used the along-track averaging criterion described in the paper. Accounting for the spread of trajectories is more physically meaningful than using a fixed part of the ground track. Our analysis showed that the spread of the trajectories along the satellite track varies on a case-by-case basis and that using too long track segments increases the risk of incorporating unrealistic or noisy signals. This could be a feature that is typical for the Arctic. Again, it is impossible to quantify the effect of deviating from an approach that is considered to be as physically meaningful as possible.

**“Better agreement with the in-situ observation may be obtained for an average over a smaller height range. However, we chose a conservative range that is likely to be suitable for most cases.” Please provide level of uncertainty.**

The level of uncertainty depends on the individual extinction profile which can change by an order of magnitude over time or with altitude. Instead of speaking of likelihood we changed the statement and now refer to what has been found during this study:

*For particular cases, better agreement with the in-situ observation may be obtained for an average over a smaller height range. However, we chose a conservative range that was found to be suitable for the cases considered in this study.*

**Page 5704, lines 1 - 2: “Using the in-situ measurements at the time of the satellite overpass decreases the agreement of the observations.” How much?**

The impact of using in situ observations at the time of the CALIPSO overpass depends on the time delay between the satellite observation and the ground-based measurements as determined from the length of the trajectories. Not accounting for the time delay increases the difference between the extinction coefficients. Here is the shift of the ratio in extinction coefficients for the example cases in Figure 2:

Case 1: 7 h delay, 1.08 changed to 1.94

Case 2: 13 h delay, 1.09 changed to 1.41

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Case 3: 9 h delay, 1.31 changed to 1.87

Case 6: 1 h delay, 4.79 changed to 5.72

Case 8: 15 h delay, 1.31 changed to 2.25

Case 9: 12 h delay, 1.29 changed to 1.77

We changed the statement to include a quantification of using improper averages of the in-situ measurements to:

*Using the in-situ measurements at the time of the satellite overpass increases the ratio of the ambient extinction coefficients from in-situ and CALIOP observations by 30% for the example cases in Fig. 2.*

**Page 5704, lines 26 - 28: “There is no indication that a closer distance between satellite ground track and in-situ ground site (or a smaller time lag, not shown) would give a better agreement.” Please specify or provide examples, references, etc.**

This is a conclusion of our investigation for the cases presented in Figure 4. The color coding of the points in this figure refer to the distance of the CALIPSO observation to the ground station. Points coded with cold colors (closer distances) do not accumulate closer to the 1:1 line than those with warm colors (further distances). We changed the statement to clarify that we are still discussion Figure 4:

*According to the color coding of the points in Fig. 4, there is no indication that a closer distance between satellite ground track and in-situ ground site (or a smaller time lag, not shown) would lead to a better outcome of the reconciliation procedure.*

**Page 5705, lines 20 - 21: “These aerosol types are rather uncommon at 78N and suggest misclassification in the CALIPSO retrieval.” Has this been proved? What is CALIPSO’s ratio of misclassifications/classifications?**

Given the structure of the CALIPSO aerosol classification scheme described in Omar

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et al. (2009), most CALIOP observations in the Svalbard region should be classified as clean continental (weakly depolarizing, not over desert, integrated attenuated backscatter coefficient  $\gamma' < 0.0005$  over land or  $\gamma' < 0.0015$  over snow/ice) and clean marine (weakly depolarizing, over ocean,  $\gamma' < 0.0015$ , not in elevated layer). The discrimination between these two aerosol types is influenced by the location of the observation (over land/snow/ice or water) and the threshold in the total attenuated backscatter coefficient. The other aerosol types require elevated aerosol layers (smoke, not observed), increased depolarization ratios (dust and polluted dust), or increased integrated attenuated backscatter coefficients of  $\gamma' > 0.0015$  (polluted continental). The latter two can result from improper cloud screening or the presence of diamond dust. Consequently, we conclude that dust, polluted dust, and polluted continental are the result of misclassification. A closer look at the individual cases reveals that they were either observation with a coinciding presence of clouds in the profile (the two dust cases, two polluted dust cases, one polluted continental case) or that several aerosol types were classified in almost equal parts within the respective layers (two polluted dust cases, three polluted continental cases).

It is hard to give a ratio of misclassification for the CALIPSO retrieval as this would require a reliable benchmark that is not available for observation in the Svalbard region. However, one can assess which aerosol types are more prone to misclassification. Dust, polluted dust, and polluted continental are classified according to the exceedance of certain threshold values of the attenuated backscatter coefficient or the approximate depolarization ratio. Improper cloud screening or noisy signals therefore have a stronger effect on these aerosol types than on clean marine or clean continental — especially in our Arctic cases with generally low signal to noise ratio. The latter two are only separated depending on the location of the observation (i.e. over water or not). To elaborate on this background of the misclassification issue, we now write:

*On the other hand, the CALIOP aerosol classification scheme can choose from a larger pool of lidar ratios for observations over ocean and land compared to those over snow*

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*and ice (Omar et al., 2009).*

*Misclassification can occur as a result of signal noise, improper cloud screening, or due to surface effects. Given the structure of the CALIPSO aerosol classification scheme described in Omar et al. (2009), CALIOP observations in the Svalbard region during background conditions (weakly depolarizing and integrated attenuated backscatter coefficient not exceeding the threshold value of 0.0015 at 532 nm) should be classified as clean continental (over land and snow/ice) or clean marine (over ocean).*

**Page 5705, lines 25 - 26: "It remains unclear, why half of the clean marine cases are within the set of outliers." Why the authors not consider this as misclassifications?**

This has been a conclusion of the nature of the CALIPSO aerosol type classification. If a case of clean marine was misclassified, it could only be clean continental instead (as no threshold values are exceeded). The difference in selecting either type is due to the observation being performed over water rather than land or snow/ice. Hence, clean marine should be properly classified.

We now also present ambient extinction coefficients that are obtained from the nephelometer measurements (revised Section 2.2.2 and revised Figures 4 and 5). These values show better agreement with the CALIOP observations and also enable an estimate of an error range (as a result of using a minimum and maximum estimate of the  $\gamma$ -value). Using these new values improves the comparison for cases classified as clean marine in a way that they no longer stick out. Consequently, we dropped the statement in the revised manuscript.

**Page 5706, lines 14 - 16: "The RH at the location of the CALIOP observation is taken from the meteorological data provided with the trajectory analysis and thus highly uncertain." Please quantify.**

We want to remind the reader that the value is taken from a model field and that relative

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humidity is one of the most variable atmospheric parameters. The GDAS fields used by HYSPLIT have a horizontal and temporal resolution of  $1^\circ$  by  $1^\circ$  and 6 h, respectively. In addition, lower tropospheric data have a vertical resolution of 25 hPa and 50 hPa below and above 900 hPa, respectively. We believe no error bar is necessary to realize that these data are highly uncertain.

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