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

Anonymous Referee #2

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Overview

The manuscript deals with a comparison of extinction coefficients at the Arctic which were derived in situ from nephelometer measurements with subsequent corrections at Zeppelin station and which were taken from remote sensing data (corrected backscatter data measured by CALIOP) in the wider vicinity of Zeppelin station. This is a very challenging task. The problem that the measurements were not taken at the same time and place was tackled by using trajectories in order to assure that at least the same air mass was probed. The authors applied a correction method regarding relative humidity which links the dry nephelometer measurements with the ambient CALIOP measure-

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ments. Finding measurements which are suitable for comparisons required an extensive screening of the data. Besides this huge amount of work, the manuscript lacks an analysis of data quality, especially determination of measurement errors including error propagation through application of the correction method. Without tackling errors, it is difficult interpreting and understanding the results.

Detailed comments

The line and page numbers are taken from the printed version not from the online display.

General

* I have difficulties in understanding phrases such as "agreement of a factor of 1.85" (page 1), "agreement of a factor of ca. two" (page 4), "factor of five in agreement" (page 12). Does it mean that the data agree with each other or they disagree? I think, data can agree within their error bars and in case there is a factor of something it points towards disagreement.

* The whole issue of errors, error propagation, error bars is almost ignored. I am also missing comments about calibration of the nephelometer (how often, how old was the latest calibration, the data were reprocessed in order to account for a shift in the calibration constant between the day of calibration), about the detection limit.

* It is not clear how the authors tackled the problem of the particle absorption which is not measured by the nephelometer. It seems from the comments on page 6 (lines 193-195) that particle absorption is only taken into account through the refractive index of particles omitting the issue of the mixing state (external/internal mixture). The analyzed data contain biomass burning episodes? The approach of measuring dry aerosol particle scattering coefficient and calculating ambient aerosol extinction coefficients is valid for such episodes? Furthermore the time resolution for OC/EC ratio is one month (page 6). I doubt that monthly values can be reasonably used for single

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hourly measurements and correct for absorbing aerosols.

* The authors investigated data for the whole year of 2008. The humidity correction was based on a model taking into account certain chemical components of aerosol particles. This model was evaluated using measurements done between July and October 2008 (see reference Rastak et al., 2014). Is this model for humidity correction suitable for measurements outside the evaluation period? How much did the chemical composition changed over the entire year? Seeing Fig. 3 in Rastak et al. 2014 manuscript, it seems that EC is much less during the evaluation period compared to the months December-May/June.

Specific comments

Line 73: "above seal level" Typo; sea instead of seal

Line 191: "are water-soluble and insoluble organics, sulfate, sea salt, and black carbon." For avoiding misunderstandings, "sulfate" should be changed to "ammonium sulfate" (see Rastak et al., 2014)

Lines 301-302: Which kind of meteorological data were used (GDAS, ReAanalysis. . .)?

Line 388: "the CALIPSO observation is in poor agreement with the result of the in-situ measurement" I would not say it is poor agreement. It seems more like a disagreement.

Line 389-390: "This emphasized that using a closest approach for comparison of ground-based measurements and CALIPSO observations might not always be the best choice." I disagree with this conclusion. The closest distance approach is related to the idea of spatial homogeneity/inhomogeneity (or representativeness) of a quantity of interest, whereas the approach of this manuscript is related to the idea of probing the same airmass. In the end it is important whether it can be expected that the quantities could be compared or not.

Lines 417-419: "It was found that the most characteristic outliers in Figs. 4 and 5a occur for cases that were identified predominantly as polluted dust, polluted continental, and

dust in the CALIPSO retrieval. These aerosol types are rather uncommon at 78N and suggest misclassification in the CALIPSO retrieval." Misclassification is a possible cause for explaining the outliers. However it is possible that classification was correct, pointing to an important contribution of aerosol absorption which seemed to be not well taken into account by the correction scheme for the nephelometer? Please note that even uncommon aerosol types could easily be present in single measurements.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5687, 2014.