

## ***Interactive comment on “Airborne lidar reflectance measurements at 1.57 $\mu\text{m}$ in support of the A-SCOPE mission for atmospheric CO<sub>2</sub>” by A. Amediek et al.***

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**Comment 1:** Abstract: The authors should think about adding the MODIS comparison to the abstract.

*We agree.*

**Comment 2:** p. 1492, ll. 19-21: The use of consecutive upscaled values (instead of independent values (i, i+n)) is appropriate for comparison with the double-pulse system of A-SCOPE. Unfortunately this information on A-SCOPE is first available in Section 8.

*The information is now already given in section 2.2.1.*

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**Comment 3:** p. 1494, ll. 9-10: This sentence can be skipped as it provides no additional information.

*It is important to mention the effect of the TROPOLEX beam size. We suggest to reformulate the sentence and to move it just after equation (7): “[here equation (7)] In principle equation (7) should also include a deconvolution to correct from the finite size of the TROPOLEX beam. Due to the significantly larger A-SCOPE beam footprint, this additional step has a negligible impact and is not considered in the paper.”*

**Comment 4:** p. 1495, ll. 2: The top hat laser profile is only considered in Section 2. The authors should concentrate on Gaussian profiles...

*Section 2 is presented in a general way intentionally. Additionally, the comparison of the different approaches shows (sec. 6), that the 1-D upscaling without a Gaussian weighting is the more appropriate method.*

**Comment 5:** p. 1499, l. 18: Please mention that the 4 MHz bandwidth results in a vertical resolution of approx. 37.5 m.

*4 MHz is the analog bandwidth of the detector/preamplifier device resulting in a vertical resolution of 12 m. Now, I omit this bandwidth in the manuscript and give the pulse response width of 80 ns FWHM and the corresponding vertical resolution of 12 m.*

**Comment 6:** p. 1500, ll. 1-6: The reader might be confused that CO<sub>2</sub> absorption is irrelevant...

*The sentence is changed.*

**Comment 7:** p. 1502, ll. 4-5: I do not see some indices for large scale structures in Fig. 4 d), but only a larger variability. Please use more appropriate scales, smoothing etc.

*That is correct. Features like the size of surface structures can not be seen in the reflectivity data by eye. Only further analyses, such as an autocorrelation, can show*

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*this. The surface types are mentioned to give the main property of the respective area as a general information about the source of the data.*

**Comment 8:** p. 1502, ll. 7-14: Is the dryer surface in Spain represented by a larger “rho-star”? Please make clear where and how the different surface types can be found in Fig. 5 c).

*This information is also not derived from the measurement data, but a general information about the regions. In this section, only the sources of the data are described without any interpretations.*

**Comment 9:** p. 1503, ll. 6-9: It is confusing that the second paragraph in this section is the introduction...

*The sentence is changed.*

**Comment 10:** p. 1503, ll. 15-16: It is not clear why an averaged calibration factor is sufficient for a whole flight track but not for consecutive flights. Please explain why the calibration can not be extrapolated (e.g. from a 50 km flight track to the whole or to consecutive tracks). Which factors influence the calibration? Is the calibration a linear re-scaling based on a best-fit approach?

*The laser/telescope overlap was re-adjusted after each landing and re-takeoff. This procedure does not lead to fully reproducible results. So different calibration factors occur for each flight, while the overlap was very stable during each particular flight. The calibration itself consists of a linear re-scaling to get the best match between TROPOLEX and MODIS, determined by eye. These facts are added to the manuscript.*

**Comment 11:** p. 1506, l. 27: Please replace “pessimistic” by a more scientific term.

*Done.*

**Comment 12:** p. 1507, ll. 5-7: The order of the sentences within this paragraph is confusing. Why are the unweighted measurements discussed again if they “should not

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be used” as mentioned before?

*This was a mistake. The weighted data are meant here.*

**Comment 13:** p. 1508, ll. 8-10 and Fig. 11: It is not clear which additional information is provided by Fig. 11. Please describe the figure and the conclusion in more detail. In fact, the figure seems to provide no additional information and may be removed. In the figure itself the color bar is missing. Contour lines might be helpful to extract quantitative information.

*It is agreed that the description of figure 11 is too short, and it has been extended in the revised manuscript. The figure helps to realize that the rms reflectivity variations may also depend on the reflectivity itself. The rms variations are not exactly the same for low, average or strong reflectivities. On the other hand, the rms reflectivity variations that are calculated in the paper are values averaged over all possible reflectivities. The difference between these definitions is best understood with the presented 2D histograms. Color bars have been added to the histograms to allow the extraction of quantitative information. The use of contour plots is not appropriate as the data are not smooth enough. A more detailed explanation is added to the manuscript. (Updated figure see below).*

**Comment 14:** p. 1509, ll. 17-26: What are the conclusions drawn from these observations?

*The following points are added to the manuscript: One has to expect a strong dependency of the retrieval precision on the season for regions where snow coverage occurs in winter. Second: The lower variability of the sea surface reflectivity leads to smaller retrieval errors due to the online/offline shift compared to land surfaces. However, the smaller reflectance leads to a lower signal to noise ratio.*

**Comment 15:** p. 1510, ll. 1-24: Please provide a motivation for these examinations. Obviously they are not strictly connected with the work for A-SCOPE.

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The general character of this investigation is now more accentuated in the text. The knowledge about the polarization/depolarization of the backscattered light could be necessary for future designs of the IPDA's receiving optics.

**Comment 16:** The authors partly use “polarisation” (British English) or “polarization” (mostly American English). Please unify.

Done.

**Comment 17:** p. 1505, ll. 11-13: The wording by using references at the beginning and the end seems to be odd. Please change.

Done.

**Comment 18:** References: Please remove the page numbers behind the publishing year.

These numbers were added by AMT.

**Comment 19:** Fig. 9: The different line shapes are hard to distinguish. Please change by using different lines, colours or symbols.

Done (updated figure see below).

**Comment 20:** Fig. 12: see comment to Fig. 9.

Done (updated figure see below).

**Comment 21:** Fig. 13: There seems to be a typo with the mean value: 0.004

Corrected.

**Comment 22:** Fig. 15: Typo in upper axis: “reflectivity”

Corrected.

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Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1487, 2009.

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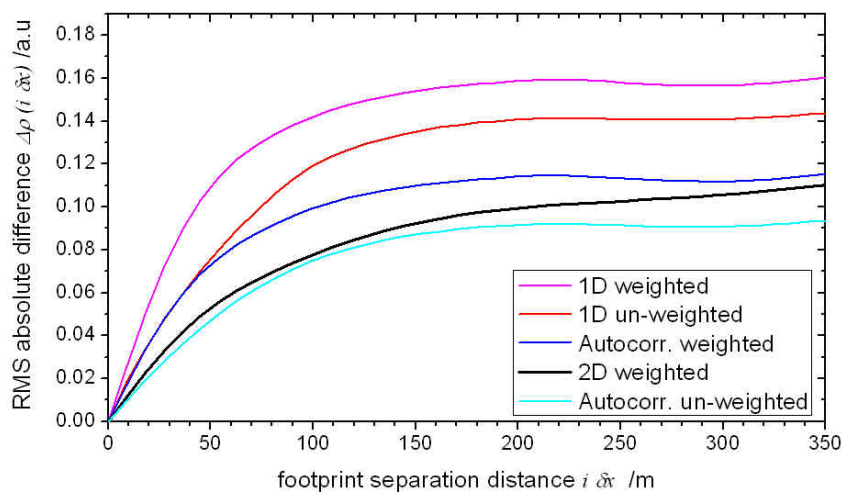


Fig. 1. Preview of updated figure 9

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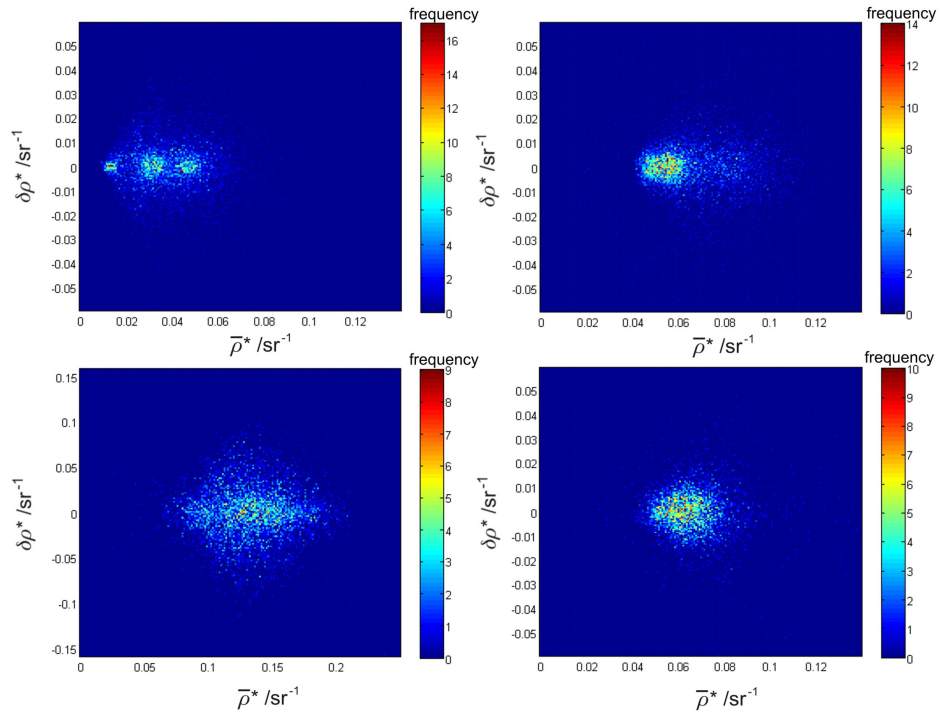


Fig. 2. Preview of updated figure 11

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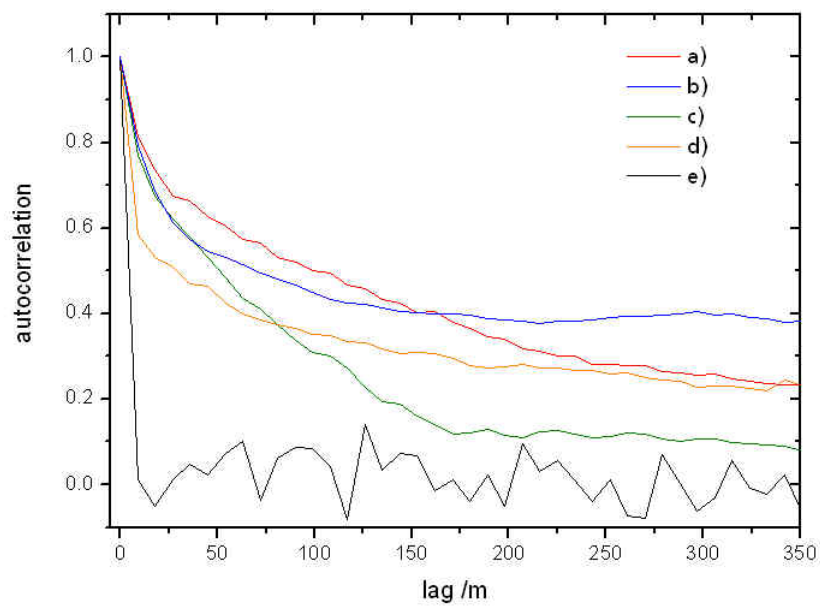


Fig. 3. Preview of updated figure 12

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