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Atmospheric Measurement Techniques Discussions

Interactive comment on "Airborne lidar reflectance measurements at 1.57 μm in support of the A-SCOPE mission for atmospheric CO_2" *by* A. Amediek et al.

Anonymous Referee #1

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The paper of Amediek et al. describes airborne lidar measurements of ground reflectances, their variability, the upscaling to spatial dimensions used by a proposed satellite mission and the implications for the accuracy of these space-based measurements of CO2. Different upscaling methods are described and compared. They are applied to observational data of different flights across mid and southern Europe, showing different ground conditions. Relative reflectances are scaled to absolute numbers using MODIS reference data. In general, the paper is comprehensive and the relevant methods and facts are described detailed and sufficiently. The paper is clearly within the scope of AMT. In the following some general, minor and technical comments are given.

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General comment:

The paper describes a comprehensive and worthwhile study of the expected accuracy of future spaceborne lidar studies of CO2 within the A-SCOPE mission. However, sometimes the reader needs some assistance how a particular section adds to fulfil the scope of this particular study or whether it provides an additional result. E.g. the comparison to MODIS data and the use of MODIS to derive absolute reflectances is worthwhile but the connection to A-SCOPE can not be seen directly. As another example in Section 7.3 the examination of the polarization is described but it is not clear how these results fit into the general context. In general the authors should try to optimize the organisation of the manuscript and make it more clear for the reader. The authors should carefully check that the conclusions from the different sections are clearly described. Original MODIS data are presented in two figures; others provide an additional axis based on the comparison (calibration) with MODIS. It should be made clear whether this additional axis is intended as a true absolute axis for the lidar data. In this case the term "MODIS" should be omitted. Otherwise the authors should think about removing the additional axis as this aspect is outside the main scope of the paper.

Minor comments:

- Abstract: The authors should think about adding the MODIS comparison to the abstract.

- 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.

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

- p. 1495, ll. 2: The top hat laser profile is only considered in Section 2. The authors

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should concentrate on Gaussian profiles (as also done in the other sections) for the sake of a clear and concise manuscript. Please see also p. 1492, ll. 14-18 and p. 1497, ll. 4-8.

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

- p. 1500, ll. 1-6: The reader might be confused that CO2 absorption is irrelevant. It should be repeated here that the present study concentrates on ground reflectance measurements only and not on CO2 measurements, while A-SCOPE aims to measure CO2 by means of ground reflectance measurements at two wavelengths.

- 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.

- 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).

- p. 1503, ll. 6-9: It is confusing that the second paragraph in this section is the introduction to the following paragraph and following section. Please re-organize and make clear that the detailed information is given below.

- 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?

- p. 1506, l. 27: Please replace "pessimistic" by a more scientific term.

- 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 be used" as mentioned before?

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- 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.

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

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

Technical comments:

- The authors partly use “polarisation” (British English) or “polarization” (mostly American English). Please unify.

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

- References: Please remove the page numbers behind the publishing year.

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

- Fig. 12: see comment to Fig. 9.

- Fig. 13: There seems to be a typo with the mean value: 0.004% may be 0.4% as mentioned in the text.

- Fig. 15: Typo in upper axis: “reflectivity”

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