

The paper presents an interesting comparison between the aerosol backscatter coefficients measured by two different lidar systems (a sophisticated multiwavelength lidar with elastic and Raman channels, and a ceilometer) and those obtained by a balloon-borne instrument performing in-situ measurements. The latter is taken as reference to validate the backscatter profiles provided by the lidars.

The paper is well written and describes a sound methodology that, besides providing the validation mentioned in the paper's title, can be useful for similar verifications at other sites.

I think the paper is worth publishing (although given its scope, focusing on techniques and methods rather than on atmospheric processes, perhaps the sister journal *Atmospheric Measurement Techniques* would provide a more suitable forum).

The authors may wish to consider the following remarks that in my view would improve the manuscript.

Main remarks

1. In the paper it is implied that the COBALD instrument is taken as the reference against which the lidar-derived backscatter coefficients are validated, on grounds that an in-situ instrument inherently provides “higher precision and signal-to-noise ratio compared to remote sensing measurements” (line 31, page 2). For this reason, I miss a more detailed description of the instrument specifications, namely, systematic (bias) and random (noise) error.
2. The above remark is somewhat linked to a seemingly lack of explanation for the mean deviations between the lidar-derived backscatter coefficients and those provided by the COBALD instruments in the PBL (+ 6% for RALMO and +13% for the ceilometer, below 2 km, (lines 23-24, page 1)). Is this just a random effect resulting from the limited dataset? Might negative differences be obtained for other datasets? Is this an effect resulting from the wavelength conversion and the FOV correction discussed respectively in sections 3.2 and 3.3? Does it come from other reasons (see next point).
3. Related to the previous point, the authors put forward the possibility (lines 1-2 of page 13) that the 15% positive bias below 2.5 km (line 30 of page 12; by the way, shouldn't it be rather 13%, cf. line 24 of page 1 and fig. 9f) be related to “minor unsolved geometric overlap issues in the ceilometer's retrieval algorithm, or (more likely) related to the assumption of a constant lidar ratio (50 sr)”. Could this also be the cause for the (smaller (+6.5%)) positive bias in the RALMO vs. COBALD comparison below 2 km? The influence of an assumed lidar ratio could be checked with relative ease. Have the authors done it?
4. It would also be advisable that the authors provide some indication on the statistical error in the measurements (estimated error bars), not only for the COBALD sondes, but also for the lidar-derived backscatter coefficients. That would help clarifying how much of the standard deviation found in the comparisons presented is due to the uncertainty of the measurements of each instrument, which must set a lower limit to that standard deviation

affected as well, as the authors point out, by the differences between the atmosphere volumes measured by the sonde and by the lidars.

5. I would suggest restricting the use of relative differences in the comparisons of the backscatter coefficients to the layers with a medium to high aerosol content. I think using it in zones of low aerosol content or in the free troposphere is misleading, as small absolute differences will yield large figures when they are divided by a very small backscatter coefficient, which in turn is probably driven by statistical noise. In this respect, the authors should probably follow the criteria stated in section 4D of their reference Matthais et al., 2004. If the authors want to highlight something important coming out from these comparisons in terms of relative errors at those altitude ranges with little or no aerosol content, they should be more explicit.

Minor issues

1. The statistical analyses of the comparisons of RALMO and the CHM15K ceilometer against COBALD are divided in figs. 6 and 9 in altitude zones, the first one being 0-2 km asl. However, in the text, when discussing the comparisons at the lowermost altitudes, the authors often use the 2.5 km limit (e.g. lines 25 and 28 in page 10, etc.). It would be easier for the reader to follow the discussions if the text and the figures would use the same limits.

2. Page 2, line 26: “the atmospheric number density”. I would suggest “the atmospheric number density of molecules”. Note that the pressure-to-temperature ratio would also do.

3. While the minimum height of measurements for the CHM15K instrument is indirectly given through its full overlap range, this information seems to be missing for the RALMO system. Even though the Raman technique employed in RALMO to derive the aerosol backscatter coefficient allows compensating incomplete overlap effects to some extent, I think RALMO’s minimum usable altitude should be stated for completeness.

4. Note a possible inconsistency in the full overlap range of CHM15K. In line 4 of page 4 it is stated as “800 m above the station”, while in lines 2-3 of page 6 it is said that “We only select measurements from ≈ 300 m above the ground station in order to minimize the effect of a possible incomplete overlap of the lidar systems in the lower part of the profiles”. Left aside the already mentioned fact that no overlap information seems to be given for RALMO, do the authors use CHM15K data obtained below its full overlap range? This deserves some clarification.

5. I suggest that a logarithmic scale be used for the horizontal axes in figs. 2a, b, and c.

6. In the caption of fig. 2 it should be stated that the graph in panel a is obtained for $N=10^3 \text{ cm}^{-3}$. Currently that information is found only in the main text.

7. I suggest trying to find a symbol (and give a name) for the ratio $\beta_{aer} / \beta_{mol}$. Otherwise the authors have to use the rather awkward notation BSR-1 to refer to that ratio and the text may even fall in ambiguities, for example in line 3 of page 8 when they say “Figure 2a shows the

simulated aerosol backscatter ratio (i.e. $\text{BSR} - 1$)". But the backscatter ratio is BSR, not $\text{BSR}-1$. Perhaps just $\beta_{\text{aer}} / \beta_{\text{mol}}$ would do.