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Atmospheric Measurement Techniques Discussions

Interactive comment on "A novel post-processing algorithm for Halo Doppler lidars" *by* Ville Vakkari et al.

Anonymous Referee #2

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General:

The authors present an algorithm to improve the accuracy of the instrumental noise of Halo Photonics Doppler lidars. Therefore, longer integration times become possible; enabling to obtain signals down to -32 dB. This is particularly useful under conditions with low aerosol load.

The algorithm combines two background correction methods. The first method corrects for a variable offset in SNR at each range gate, visible as horizontal stripes in the time-height cross-sections of retrieved backscatter. The description of the source of this error in SNR as well as its correction is novel. The second method corrects for an error in scaling of the raw signal, which can result in vertical stripes in the time-height cross-sections of retrieved backscatter. The second correction method has already

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been published in AMT (Manninen et al., 2016). The authors show that combining both methods gives added value to the retrieval of atmospheric turbulence properties as well as aerosol observations. Additionally, the “novel post-processing” algorithm could be used in other Doppler lidars and other lidar systems.

Overall, the paper is well written and the description of the algorithm is logical and well-structured. However, the authors should include a more detailed discussion of SNR statistics.

Therefore, the given paper presents a valuable contribution to the field of lidar-based remote sensing. I recommend it for publication in AMT after considering the following comments.

Major comments:

1) Does applying the horizontal stripe correction prior to the Manninen correction influence the performance of the cloud screening, which included in the latter?

2) In Figures 6, 7 and 8 the reader can visually see the improvements in SNR looking at SNR0 to SNR2, but it is essential to quantify the improvement, e.g. with a figure along the line of Figures 8 or 9 in Manninen et al. (2016). Such a statistical analysis of the SNR would present a more detailed picture of the algorithms performance. It would also help to answer comment 1).

3) p.2 l. 18: “as uncertainty . . . wind retrievals. . .” + p.9 l.17-21: “With enhanced SNR,. . .”

On the one hand, the introduction refers not only to turbulence but also to wind retrievals. On the other, the conclusion only states turbulence retrievals. Methods, such as velocity azimuth display utilize the SNR to determine reliable radial velocity measurements. Therefore, the correction algorithm increases the data availability and probably decreases the uncertainty in the retrieval.

The paper would gain more attention if this is aspect is included in the discussion;

either by actually showing that the novel post-processing improves the retrieved winds or by including a paragraph on the expected benefit for wind retrievals.

4) Manninen et al. (2016) give the code as well as an example as supporting information. This paper's algorithm could spread a lot faster, if the authors also include the code in the supporting information or point to a repository, e.g. on github.

Minor comments:

1) Figure 1b shows the temperature dependency of the background profiles. Between 20°C and ca. 27°C the increase seems linear with only a small spread. Above 27°C the increase appears linear with a steeper slope and wider spread of values. Is it possible that part of this change (especially the wider spread) is due to the switch on of the active cooling unit of the Halo Streamline? The cooling would insert vibration and additional electronic noise into the lidar system which could alter the background measurement.

2) Figure 2 and the corresponding discussion on page 5 show that even if the technical specifications of two Doppler lidars are the same (see Table 1), their performance can be different due to instrumental characteristics. This presents an additional finding of this paper which is especially important when operating multiple Doppler lidars. The users should be aware of the possible differences in instruments of the same type. Hence, this finding deserves more attention and should be mentioned in the conclusion.

3) Concerning the high and low mode in the XR lidar: Do the authors know by now, why there are two modes?

Halo lidars can be equipped with a depolarization channel. Is instrument number 146 such an instrument and can the authors exclude that the seemingly random shifts in modes are due to switching between co- and cross-polarization?

4) Could methods similar to the horizontal stripe correction be applied to other lidar

systems such as ceilometers?

5) Some readers could be interested in using Doppler lidars semi-operationally, e.g. for a seasonal campaign or even 24/7. Which parts of the algorithm are applicable operationally? How long does the data set have to be in order to successfully apply the post-processing? What about longer data gaps?

The publication would benefit from a remark to operational applicability.

6) The introduction starts by mentioning “turbulent mixing” and “mixing layer height”, but the paper presents no estimate of the MLH in one of the Figures 4 through 8. Including the MLH as a line plot in Figure 4 or elsewhere would round up the discussion that started in the introduction.

7) p.5 l. 11: “t”elescope.

8) Table 1 seems to long and could be shortened by mentioning redundant information only once

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