

Review of the AMT manuscript egusphere-2026-1140: “Deriving the telescope focus function for a pulsed coherent Doppler lidar without a reference instrument”

The manuscript proposes a new methodology to derive the instrumental telescope focus function (T_f) for pulsed coherent Doppler Lidar (DL), without using any external reference instrument. T_f is derived from DL measurements collected using horizontally pointed scans, under the assumption of homogeneous attenuated backscatter coefficient profiles. The methodology is tested through a multi-instrument comparison using eight co-located DLs and validated against T_f values derived from vertically pointing DL measurements, as well as compared with ceilometer observations.

This study is a valuable contribution to the lidar community, as it enables self-sufficient experiments without requiring additional observations or instruments, which are often scarce or limited in the field. The topic is therefore highly relevant to experimental and scientific applications in meteorology. However, after careful reading, I find that the presented method appears more restricted to specific conditions than generally applicable. Therefore, before the manuscript can be considered for publication, it is required to address several aspects as follows:

Major comments:

1. Please fix the cross-references throughout the manuscript, currently they don't work, and it is a bit complicated navigating through the manuscript.
2. The title and abstract present this work as a technical contribution focused on a specific problem, estimating T_f . I would expect the introduction to address the state of the art on the technical, instrumental, or computational challenges of deriving T_f . Instead, the introduction focuses mainly on the importance of the backscatter coefficient itself, and the instruments used to retrieve it. I understand that estimating T_f could be a limitation for retrieving backscattering but my concern is this misaligned a bit with the goal of the paper. I recommend revising the introduction to better motivate why estimating T_f is a sufficiently important challenge.
3. The abstract mentions comparison against a vertically pointing method using "an elastic backscatter lidar as reference," while the introduction and Section 2.3 refer to this instrument as a ceilometer. I suggest using consistent terminology for all instruments throughout the manuscript.
4. Although the paper's scope is the new method to retrieve T_f , Sections 2.1 and 2.2 would benefit from more detail on the operation of the eight DLs. It is not fully clear whether the instruments performed fixed-azimuth scans or VAD scans, and whether all eight ran the same scan configuration continuously and simultaneously. If these details are available in another publication or in supplementary material, please add the corresponding reference.
5. Consider moving lines 112-114 (page 6) to section 2.2 and provide more details.
6. Page 6, lines 130–132: the threshold method presented here is described as suitable for short periods with relatively low SNR variation. Would this method need to be revisited for longer-term observations with, for example, meteorological variability, or is it simply a matter of selecting a more appropriate threshold value?
7. Page 6, Section 3.3: while the method itself is understandable, the description could be restructured to follow the logical sequence of steps more clearly. I strongly recommend including a flowchart (or something similar) summarizing the methodology to improve clarity.

8. Section 4.2 could better exploit the figures it introduces. Figure 3 shows the time-height cross-section of β' retrieved by the DLs (via the different methods) and by the ceilometer. In the ceilometer panel, some streaks appear and are not visible in the other panels, is this related to the β' retrieval method, or is it a purely instrumental artefact?

Broadly, the text describing Fig. 3 does not fully discuss the differences shown in the figure itself, referring only to "significant differences" without a quantitative basis, which then appears late separately in Figure 4. As presented, Figure 3 risks feeling redundant, since its message is not clearly articulated in the text and the quantitative comparison is left entirely to Figure 4.

9. The method presented in this manuscript, especially after reading both Section 4.3 and the conclusions, reads as restricted to the tested conditions rather than broadly generalized method. Could the authors add a discussion of the scope, applicability, and limitations of the methodology?

Minor comments:

- Page 1, Line 15-16: please, add references.
- Page 1, Line 20-21: please, add references.
- Page 2, Line 23: "detecting cloud base heights (Platt et al., 1994)" → add a recent reference.
- Page 2, Line 43: check consistency between "8" and "eight" when referring to the number of DLs throughout the manuscript.
- Page 6, Lines 123-125 and 129-130: please review the grammar for clarity.
- Page 6, Line 140: please clarify what quantity "too high uncertainty" refers to.
- Page 7, Lines 150-53: you provide two potential hypotheses for the difference in D , have you tested them?
- Page 7, Lines 154: please specify for which DLs the values of D and f remain the same between the two horizontal methods.
- Page 7, section 4.1: this section is a bit too speculative, would it be possible to support the hypotheses with some evidence or literature confronting?
- Page 8, Table 2: (a) clarify "Doppler lidar" in the caption and check this consistently throughout the manuscript; (b) the content of column 1 could be clearer, please explain in the caption, and specify what "nr" stands for.
- Page 9, lines 198-199: why do you average to 2 h?
- Page 15, Figure 2 caption: please check the spelling, and consider referencing Table 2.