Response to reviewers comments

September 2026

We have addressed all of the points raised by the reviewers (copied here and shown in black text), and include our responses to each point below (in blue text). Where applicable, we also provide the new text (in blue italics).

1 RC2

1.1 General comments

Please fix the cross-references throughout the manuscript, currently they don't work, and it is a bit complicated navigating through the manuscript.

This is related to the pdf version provided by the journal and is not under the control of the authors. We assume the cross-references should be working in the final version of the manuscript.

The title and abstract present this work as a technical contribution focused on a specific problem, estimating Tf. I would expect the introduction to address the state of the art on the technical, instrumental, or computational challenges of deriving Tf. Instead, the introduction focuses mainly on the importance of the backscatter coefficient itself, and the instruments used to retrieve it. I understand that estimating Tf 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 Tf is a sufficiently important challenge

The following text changes and additions have been included in the introduction:

However, retrieving the profile of attenuated backscatter coefficient, β', from a measured SNR profile requires knowledge of the Doppler lidar's telescope focus function ($T_\mathrm{f}(R)$), which varies non-linearly with the measurement range [Frehlich and Kavaya, 1991, Pentikäinen et al., 2020, Wei et al., 2025]. If $T_\mathrm{f}(R)$ is known or can be calculated [Chouza et al., 2015, Pentikäinen et al., 2020], then the profile of β' can be derived making it possible to use the Doppler lidar as an elastic backscatter lidar such as an automatic lidar ceilometer (ALC, Illingworth et al., 2015). This can lead to cost reductions for measurement campaigns that require both wind and attenuated backscatter coefficient profiles, or *enable a Doppler lidar to be used as a backup instrument for an ALC. However, for most scanning Doppler lidars, $T_\mathrm{f}(R)$ is unknown or deviates significantly from what is assumed, which prevents the accurate calculation of the profile of β' [Chouza et al., 2015, Wei et al., 2025].*

To the authors' knowledge, the literature on the topic of estimating $T_\mathrm{f}(R)$ is limited. Chouza et al. [2015] estimated the heterodyne efficiency (part of $T_\mathrm{f}(R)$) from aircraft mounted Doppler lidar measurements based on the received power from a single height in the atmosphere as a function of the altitude of the aircraft, but the method has limited applicability for ground-based Doppler lidars. Pentikäinen et al. [2020] estimated $T_\mathrm{f}(R)$ for ground-based Doppler lidars using ALCs as reference instruments. While the method shows good results, it requires a period of co-location with an ALC, and depending on the deployment location may require a long measurement period to obtain sufficient data for the two devices operating at different wavelengths to be considered comparable after discarding profiles with clouds or precipitation. Wei et al. [2025] presented a self-calibration method for Doppler lidars where the heterodyne efficiency is derived based on horizontally pointing measurements assuming homogeneous aerosol distribution. This method is the most practical and cost effective of the three methods, requiring no other instruments or platforms for the calibration process. The accompanying theoretical exploration of the impacts of beam focusing, heterodyne efficiency, CNR, lidar constant calibration, lidar-ratio assumptions, and refractive turbulence is comprehensive, but due to limited disclosure regarding the measurement data behind the results, the robustness of the presented validations and their generalisability is difficult to evaluate.

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.

Replaced with "automatic ceilometer lidar (ALC) throughout the manuscript where applicable. A reference to the first use of ALC in the literature has also been added.

Although the paper's scope is the new method to retrieve Tf, 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.

Section 2.1 has been modified to describe the operation setup more clearly: *During this time period eight closely located Doppler lidars, located at Falkenberg, 5 km from Lindenberg, performed simultaneous measurements in staring mode, i.e., pointing in only one direction. Each device measured both in the vertical and at an 5° elevation angle (85° zenith), with all units being in the same operation mode most of the time, resulting in approximately 2-3 days of measurements in both modes for each device.*

Consider moving lines 112-114 (page 6) to section 2.2 and provide more details.

The addtional details are provided in the previous answer, added to 2.1 instead.

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?

This is not strictly necessary, as the methodology would work just as is for a longer time period with more variability in the SNR level. However, it could results in the filter passing plumes as instrumental noise or filtering instrumental noise as plumes when the SNR level would differ from the level seen during the measurement period used here. Using a threshold that is relative to SNR level of each specific profile would increase the number and proportion of high quality samples in the filtered data set and should reduce the uncertainty in the derived estimates of $T_{\mathrm{f}}(R)$.

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.

Flowchart added, see Figure 1

Section 4.2 could better exploit the figures it introduces. Figure 3 shows the time-height crosssection 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.

The streaks in the ceilometer figure above liquid cloud returns are instrumental artefacts, arising from a change in the background noise level (the gain of the receiver automatically adjusts when there is strong backscattering from liquid clouds), and should not be replicated by the Doppler lidars. The main goal of this figure is to show that the two methods employed (panels b and c) agree visually with each other and the ALC (panel d), and that neglecting to perform any telescope focus function correction (or assuming an incorrect telescope focus function) can lead to errors easily exceeding an order of magnitude (panel a).

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?

The method is intended to be a generalised method and the discussion in section 4.3 and the conclusions does not indicate it wouldn't be such. These sections highlight that the data filtering must be done based on different criteria than for the vertical method, as refractive turbulence appears to affect the data more, as might be expected when pointing more to horizontal, and requires the removal of periods of high refractive turbulence from the dataset to avoid bias in the derived $T_\mathrm{f}(R)$. Due to the short time span of the measurement campaign and being limited to a single geographic location, we cannot comment on how significant this issue is in general, i.e. what is the proportion of data that needs to be discarded by the filter for an arbitrary location and measurement period. We are not able to estimate the magnitude of the possible bias in the derived $T_\mathrm{f}(R)$ due to high refractive turbulence for this dataset. Given a longer measurement period, it would be possible to compare differences in the $T_\mathrm{f}(R)$ estimates during low and high refractive turbulence periods, e.g., between night and day. Ideally, co-located refractive turbulence measurements would enable such impacts to be quantified.

1.2 Specific comments

Page 1, Line 15-16: please, add references.

References added: [Frehlich and Kavaya, 1991, Pentikäinen et al., 2020, Wei et al., 2025]

Page 1, Line 20-21: please, add references.

References added: [Chouza et al., 2015, Wei et al., 2025]

Page 2, Line 23: "detecting cloud base heights (Platt et al., 1994)" → add a recent reference.

References added: [Maturilli and Ebell, 2018, An et al., 2020]

Page 2, Line 43: check consistency between "8" and "eight" when referring to the number of DLs throughout the manuscript.

Changed "8" to "eight"

Page 6, Lines 123-125 and 129-130: please review the grammar for clarity.

Lines 123-125 changed to: *With it, it is possible to detect and remove sharp changes in the SNR due to aerosol plumes that break the assumption of homogeneous backscatter, while avoiding filtering out the slower and smoother changes* *in SNR due to the shape of the $T_\mathrm{f}(R)$.*

Lines 129-130 changed to: *In a linearly changing SNR profile this would correspond to change of 0.375 over the length of the wavelet. All data beyond the first instance in the profile where the threshold is exceeded is removed.*

Page 6, Line 140: please clarify what quantity "too high uncertainty" refers to.

Used MSE threshold is 0.001 for the normalised profile. The mean value of the normalized profile is 1. This is now included in the text.

Page 7, Lines 150-53: you provide two potential hypotheses for the difference in D, have you tested them?

Unfortunately, we do not have data that would allow us to do so. Hence, these two hypotheses are left open.

Page 7, Lines 154: please specify for which DLs the values of D and f remain the same between the two horizontal methods.

Updated to: *For five of the analysed Doppler lidars (units nr. 44, 74, 161, 172, and 177), the estimated...*

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?

Unfortunately not. The lidar units operated at this specific measurement configuration only for a short period of time, and hence, we lack the data to draw further conclusions regarding the sensitivity of the results to the length of the measurement period. Additionally we do not have physical access to the Doppler lidar units to inspect whether there are any deficiencies in how they were performing. Literature on a similar methodology is very limited, with only Wei et al. (2025) being only comparable study, and it does not include a detailed description of measurement data used. Slight deviations between the reported pointing angle and the true pointing angle have been reported for similar instruments using the Flip Test (see `https://www.arm.gov/publications/tech_reports/handbooks/dl_handbook.pdf`) but unfortunately these tests were not performed during this campaign.

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.

Updated caption: *Estimates of f and D for all Doppler lidars in the study for vertical data, horizontal data and horizontal data with MSE filtered results. Doppler lidar nr. refers to the serial number of the device.*

Line 183 changed “lidars” to “the Doppler lidars”

Page 9, lines 198-199: why do you average to 2 h?

This is for clarity so that the different lines on the graph are legible (reducing the number of data points and line crossings).

Page 15, Figure 2 caption: please check the spelling, and consider referencing Table 2.

Updated caption: *$T_\mathrm{f}(R)$ for all Doppler lidars estimated with the vertical, horizontal and horizontal MSE filtered methods. The corresponding f and D values are given in Table 2.*

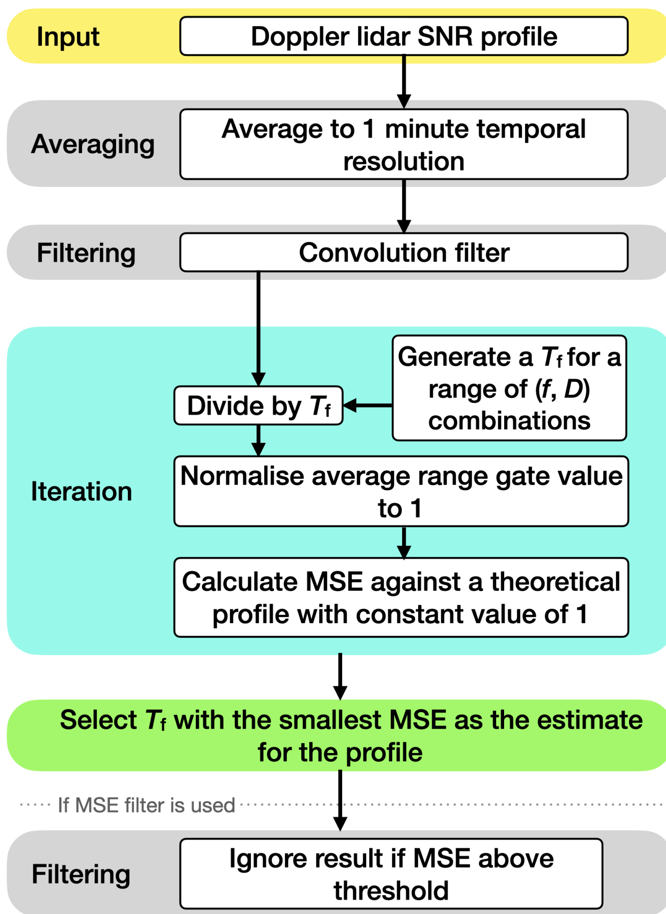

Figure 1: Flow chart summarising the process of estimating f and D from Doppler lidar SNR data with the horizontal method. The last step is only performed if the MSE filter is used.

References

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