Response to the reviewers of Syed, Mann and Manami: "Measurement of turbulence energy dissipation rate by a standalone high-resolution Doppler lidar" (Egusphere-2025-5214)

August 17, 2026

Referee #1 Feng Guo

General comments:

This paper proposes a novel second-order structure function model for lidar measurements. The model can be used to correct turbulence filtering caused by probe-volume averaging and to estimate the turbulence energy dissipation rate. The proposed method is shown to provide energy dissipation rate estimates from a six-beam lidar that are in good agreement with traditional spectra-based methods using a sonic anemometer. Overall, the paper is well written, and the proposed method makes a significant scientific contribution across multiple fields. I have several comments, mostly minor, that the authors may consider to further improve the manuscript.

Authors' Response: We thank Feng Guo for the constructive comments and suggestions, which have helped us improve the clarity and quality of the manuscript. Each specific comment is addressed separately below.

Major comments:

1. **L20 in introduction.** The authors claim that hot-wire sensors are commonly used for atmospheric turbulence measurements. Based on my experience, hot-wire anemometers are used far more often in laboratory experiments. Hot-wire instruments are sensitive and fragile; the thin wire is extremely delicate and breaks easily under real atmospheric conditions. Rain, dust, insects, humidity, and temperature swings can affect their accuracy or damage the sensor. In addition, hot-wire anemometers exhibit calibration drift and require frequent recalibration, making them impractical for remote or long-term field deployments.

   Authors' Response: We agree. We have modified the sentence. See line 20 in the revised version.

2. Some explanations or references on why a pulsed lidar does not measure point but rather a probe volume is helpful. Also, a CW lidar is possible to measure a point-like turbulence as a hot-wire or sonic anemometer if the focused distance is very short (smaller than several meters) to the probe lens.

Authors' Response: The following text is added to the revised version. See line 93 in the revised version.

"The pulsed Doppler lidar measures backscattered light over a finite spatial volume, so-called probe volume, the extent of which is governed by the pulse duration. The return signal, therefore, contains contributions from the aerosol particles distributed throughout the probe volume. The center of the probe volume is typically assigned as the nominal range gate position and determined from the pulse time-of-flight (Peña and Hasager, 2011). Volume measurement is inherently different from point measurements used in reference instruments, such as sonic or cup anemometers."

3. **Line 140,** Some additional details on the six-beam lidar are recommended. Commercial pulsed lidars typically have a radial (range-gate) resolution of 15–60 m, which is determined by the temporal window used in the Fourier transform applied to the heterodyne signal. Can the reported 3 m resolution be achieved simply by selecting a shorter window length, or is a different signal-processing strategy or hardware configuration being used?

   Authors' Response:

   Thank you for this comment. In the analysis of heterodyne signals, the pulse duration defines the Fourier window, as it corresponds to the intrinsic temporal extent of the emitted pulse. Consequently, using a Fourier window shorter than the physical pulse length is generally not considered a standard signal-processing approach.

   The fine range resolution achieved by HALO Photonics lidars originates from the autocorrelation-based signal processing method, also called correlogram, which enables the application of overlapping windows to the autocorrelation matrix.

   Two classical approaches are commonly used for estimating the power spectral density: the periodogram and the correlogram. Both methods have been implemented in commercial wind lidars and are theoretically equivalent according to the Wiener-Khinchin theorem. In practice, however, hardware constraints generally prevent commercial Doppler lidars based on the periodogram approach from applying extensive overlapping Fourier windows directly to the heterodyne signals. As a result, the achievable range resolution remains relatively coarse in periodogram. Advances in FPGA technology (Abdelazim et al., 2018) have made the correlogram approach increasingly feasible by enabling efficient computation of autocorrelation matrices. Overlapping Fourier windows can then be applied to the accumulated autocorrelation matrix, leading to a finer range resolution.

   We have added the following clarification to address this comment (See Section 3: Data and site description, line 152 in the revised version):

   "The 3 m spacing should not be interpreted as the physical extent of the lidar beam's probe volume, which exceeds 20 m for the lidar understudy. Instead, the high range resolution achieved by HALO Photonics lidars results from their autocorrelation-based signal processing technique (Abdelazim et al., 2018), which allows overlapping Fourier windows to be applied to the accumulated autocorrelation matrix."

4. **Line 182.** Is the mean obtained by averaging over time—for example, every 6 seconds for each beam? In Step 2, the six beams will produce different LOS wind-speed spectra because each beam is contaminated by different velocity components according to its projection vector, and because atmospheric turbulence is anisotropic. Please explain why the average is taken over the six beams. In particular, the vertical beam measures only the vertical velocity component, yet it is still included in the averaging.

Authors' Response: The mean structure function is not obtained by directly averaging the instantaneous LOS velocities from the different beams. Instead, the longitudinal structure function is first calculated separately for each beam using the spatial velocity differences between range gates within an individual scan. These beam-specific structure functions are then averaged over the scans available within each 10-min period.

Systematic differences between the structure function estimated from beams in different directions may arise outside the inertial-subrange, especially in complex terrain or sheared flows. However, the present method estimates ε from separations restricted to the inertial subrange, where we assume that the small-scale turbulence is locally isotropic. Under local isotropy, the longitudinal second-order velocity structure function is independent of the separation vector (beam) orientation. Thus, even though large-scale turbulence anisotropy may be present in the atmosphere, the longitudinal structure function for beams in different directions within the inertial subrange shows very little deviation.

5. The impact of the mean vertical wind profile (related to atmosphere stability) on the LOS velocity distribution within the probe volume also requires further discussion.

   Authors' Response: The reviewer is correct in pointing out that the variations in the mean wind profile should be considered. Imagine a calculation of the structure function in the following way. The LOS velocity is divided into the mean and the fluctuations: $v_r = \overline{v}_r + v_r'$. Then the structure function is

$$
\begin{aligned}
D_r(\Delta r) &= \left\langle \left(v_r(r+r) - v_r(r)\right)^2 \right\rangle \\
&= \left\langle \left(v_r'(r+r) - v_r'(r)\right)^2 \right\rangle + \left\langle \left(\overline{v}_r(r+r) - \overline{v}_r(r)\right)^2 \right\rangle \\
&= C_K(\varepsilon\Delta r)^{2/3} + \left\langle \left(\overline{v}_r(r+r) - \overline{v}_r(r)\right)^2 \right\rangle
\end{aligned}
$$

   Here, the last term can distort the structure function, but we do subtract the mean LOS wind profile before the calculation of the structure function, so this should not pose a problem. We have now in the manuscript pointed out explicitly that we subtract the mean before calculating the structure function.

6. It is recommended that the authors discuss how the proposed method for detecting energy dissipation rates could be utilized to improve turbulence intensity estimation in wind-energy applications.

   Authors' Response: We have added the following text to the Discussion section. See line 319 in the revised version.

   "For wind energy applications, turbulence intensity (TI) is a more practical parameter to describe turbulence instead of ε. An ongoing study aims to accurately estimate the TI parameter using the methods described in this paper. The aim is to correct the LOS variance estimates for each beam once we know ε, and then use LOS variances to compute the Reynolds stress tensor using a method described by Eberhard et al. (1989) and Sathe et al. (2015)."

Minor comments:

1. **Abstracts:** in areas ->in areas or at heights

   Authors' Response: Done

2. **Line-of-sight** some times written as line of sight

   Authors' Response: Done

3. **Line 172,** please explain why these wavenumber range is chosen

   Authors' Response: A justification has been added to the text. See line 183 in the revised version.

4. **Line 177,** Is the Taylor's theory is also considered in the accumulation time (is), please clarify

   Authors' Response: No. We do not make any assumptions about Taylor's hypothesis. The accumulation time is 1 s for each beam.

5. **Please** explain N in eq (11)

   Authors' Response: Done

6. **Figure 5,** should be k1 not k in the left figure

   Authors' Response: Done

7. **Line 215,** Can the author further explain why the errors increase with heights?

   Authors' Response: An explanation is now added.

8. **Figure 7.** The point density needs to be explained.

   Authors' Response: Done

9. **Line 249,** DSW already defined

   Authors' Response: Done

Referee #2 Maxime Thiébaut

General comment

The authors present an interesting study addressing the measurement of the turbulence kinetic energy dissipation rate, ε, using the BEAM6X WindSciences lidar from Lumibird. The study focuses on line-of-sight (LOS) velocity measurements from the Lumibird system, thereby avoiding one of the main limitations of Doppler lidars, namely cross-contamination (or inter-beam effects).

The second major limitation of lidar measurements, referred to by the authors as "probe averaging" (intra-beam effects), is addressed through the introduction of a Gaussian weighting function, which is claimed to recover part of the turbulent eddies filtered out by the lidar measurement process. Since this limitation is inherent to all wind lidars, the proposed correction is of significant interest.

Reference measurements are provided by a 3D sonic anemometer and show good agreement with the lidar-derived estimates, demonstrating the potential of the proposed methodology. However, several major methodological issues need to be addressed before the manuscript can be considered for publication.

Authors' Response: We sincerely appreciate Maxime Thiébaut's constructive comments and suggestions, which have improved the overall quality of the manuscript. The individual comments are addressed in detail below.

Major comments

- **l.28, p.2:** You mention the "probe-volume" averaging. While this is indeed a limitation of lidar measurements, it only refers to the spatial component of the averaging. There is also a temporal averaging associated with the accumulation time at each LOS measurement location. It would therefore be more appropriate to refer to this limitation as "probe-time averaging."

  The intra-beam effect is in fact associated with two filtering processes: (1) spatial filtering due to averaging over the probe volume and (2) temporal filtering due to averaging over the pulse accumulation time, Δt. As demonstrated in (Thiébaut et al., 2025), these two contributions define the transfer function applied by the instrument to the measured signal:

  $$|H|^2(\mathbf{k}) = \mathrm{sinc}^2\left(\frac{\Delta t}{2}\,\mathbf{k}\cdot\mathbf{U}\right)\exp\!\left(-\left[\sigma_l^2(\mathbf{k}\cdot\mathbf{b})^2 + \sigma_r^2\left(\|\mathbf{k}\|^2 - (\mathbf{k}\cdot\mathbf{b})^2\right)\right]\right). \tag{1}$$

  where the sinc term represents the temporal averaging and the Gaussian term the spatial averaging. Therefore, the attenuation of turbulent fluctuations does not only originate from the probe volume itself, but also from the finite accumulation time of the LOS measurements. This distinction should be clarified in the manuscript.

  Authors' Response: This is an important comment. This has given rise to quite an extra amount of consideration, and it has improved our results a bit, so we are thankful for this comment. The method for obtaining turbulence energy dissipation rates from lidar is amended to include temporal averaging (See Appendix A1). New results are presented in the Results section, which discusses incorporating temporal averaging in addition to spatial averaging to determine the second-order structure function from the lidar beams. We do, however, not include the transverse beam averaging, since the impact of this is extremely small compared to the along beam averaging and the temporal averaging.

- **Eqs. 5 and 6:** My comment does not only concern these two equations, but more generally the Gaussian weighting function proposed throughout the manuscript. From the current formulation, I understand that the proposed correction only addresses the spatial averaging of turbulent eddies within the probe volume. However, the temporal averaging associated with the accumulation time of the LOS measurements also contributes to the attenuation of turbulent fluctuations and should therefore be considered.

  The manuscript should clarify whether the proposed Gaussian weighting function only compensates for the spatial filtering or if the temporal filtering is also implicitly accounted for. If not, the potential impact of temporal averaging on the estimation of ε should be discussed.

  Authors' Response: See response to previous comment.

- **l.75, p.3:** While this point may be clear for readers familiar with wind lidar technology, it could be misleading for a broader audience to state that the range gate is 3 m without further clarification. One could easily confuse the "range gate" spacing with the actual "probe length" of the lidar measurement. It should be explicitly stated that the probe length of the considered technology is several tens of meters, leading to a significant overlap between consecutive gates spaced every 3 m. From my understanding, it is only through the proposed Gaussian weighting function that an effective or "true" vertical resolution close to 3 m can be achieved. This distinction should be clarified in the manuscript.

  Authors' Response: We have modified the statement now as (See line 71 in the revised version):

  "The lidar system used in this study provides LOS velocity estimates at closely spaced range gates, with a range-gate spacing of 3 m along the beam. Such a small range-gate spacing enables the structure function $D(r)$ to be evaluated at small separations, thereby improving the identification of the inertial subrange."

  We have also added another clarifying statement in Section 3: Data and site description (See line 152 in the revised version):

  "The 3 m spacing should not be interpreted as the physical extent of the lidar beam's probe volume, which exceeds 20 m for the lidar understudy. Instead, the high range resolution achieved by HALO Photonics lidars results from their autocorrelation-based signal processing technique (Abdelazim et al., 2018), which allows overlapping Fourier windows to be applied to the accumulated autocorrelation matrix."

  Thank you for pointing out this potential source of confusion, which was also raised by the first reviewer. For further clarification, please refer to the authors' response to Major Comment 3 from the first reviewer.

- **l.93, p.4:** You choose to set the universal Kolmogorov constant to 2.0. While this choice is acceptable, the value is not as universal as sometimes implied. As an example in Doppler radar measurements of atmospheric turbulence (Sauvageot, 1992), the Kolmogorov constant was chosen $C_K = 2.1 \pm 0.1$. A similar value, $C_K = 2.1 \pm 0.1$, was also obtained by another study (Saddoughi and Veeravalli, 1994) from wind tunnel measurements. In the literature, both values (2.0 and 2.1) are commonly used. From my experience, using 2.1 instead of 2.0 leads to ε estimates approximately 7–9% lower. It would therefore be useful to briefly justify the selected value and discuss the associated uncertainty on the estimation of ε.

  Authors' Response: The following text is added in the Discussion section in response to this comment. See line 291 in the revised version.

"The choice of the Kolmogorov constant C_K also impacts the estimated ε values. C_K is reported to be 2.1±0.1 in the literature (Pope, 2000). For the fixed measured structure function, the turbulence energy dissipation rate scales as $\varepsilon \propto C_K^{-3/2}$, thus using $C_K = 2.1$ instead of $C_K = 2$ would lower the ε estimates by approximately 7%. This is true for the absolute comparison between ε estimates obtained from a single instrument/method. However, in the relative comparison between ε values obtained from lidar and sonic anemometer, the choice of C_K does not matter. The sonic-based dissipation rates are inferred using the spectral Kolmogorov constant $\alpha = 1.52$, which comes from $C_K = 2$ (a value of $C_K = 2.1$ results in $\alpha = 1.6$). Therefore, although the choice of C_K does introduce uncertainty in the absolute magnitude of the obtained ε values, it does not affect the relative lidar-sonic comparison."

- **l.183, p.9:** The choice of averaging all six beams to obtain the mean longitudinal structure function may have a significant impact on the estimation of ε. I encountered a similar issue with a WindCube v2.1 configuration, and I believe the same reasoning can be applied to the BEAM6X system.

  Let us consider beam 1 and the opposite beam 3 of a WindCube v2.1 configuration, with beam 1 facing upstream and beam 3 downstream. One obtains:

  $$D_1 = D_u \sin^2\theta + D_w \cos^2\theta - 2D_{uw} \sin\theta\cos\theta \quad (2)$$

  $$D_3 = D_u \sin^2\theta + D_w \cos^2\theta + 2D_{uw} \sin\theta\cos\theta \quad (3)$$

  where θ is the zenith angle, D_u and D_w are the second-order moments of the along-wind and vertical velocity differences, respectively, and D_{uw} is given by:

  $$D_{uw} = \overline{u'_0 w'_0} + \overline{u'_r w'_r} - \overline{u'_r w'_0} - \overline{u'_0 w'_r} \sim 2\overline{u'w'} \quad (4)$$

  Invoking horizontal homogeneity, $D_1 - D_3 \sim -4\overline{u'w'}$. Speed-bin-averaged ε estimates from upstream-facing beams tend to be systematically larger than those from downstream-facing beams by a factor of 1.3 to 1.8.

  This issue could become critical in complex terrain or wake flows. It is unclear why the analysis was not restricted to the vertical beam only, or why individual beams were combined. Furthermore, a wind rose showing the orientation of the six beams relative to the dominant wind directions would clarify this impact.

  Authors' Response: Systematic differences between the structure function estimated from beams in different directions may arise outside the inertial-subrange, especially in complex terrain or sheared flows. However, the present method estimates ε from separations restricted to the inertial subrange, where we assume that the small-scale turbulence is locally isotropic. Under local isotropy, the longitudinal second-order velocity structure function is independent of the separation vector orientation. Thus, even though large-scale turbulence anisotropy may be present in the atmosphere, the longitudinal structure function for beams in different directions within the inertial subrange shows very little deviation.

- **l.176, p.8:** You are using two different methods to estimate the turbulence dissipation rate: the spectral method for the sonic anemometer measurements to obtain $\varepsilon_{\text{spec}}$, and the structure-function method for the lidar measurements to obtain ε_{SF}. From my experience, even when applied to the same instrument, there is often a systematic tendency for $\varepsilon_{\text{spec}} < \varepsilon_{\text{SF}}$. This may introduce an additional bias when comparing lidar- and sonic-derived estimates. The potential impact of using two different estimation techniques should be discussed more carefully.

  Authors' Response: Thank you for raising this important point. We have added the following text to the Discussion section. See line 298 in the revised manuscript.

"It should be noted that two different methods are used to estimate ε from sonic anemometer and lidar measurements: the spectral and structure function methods, respectively. However, both estimates are based on the same fundamental assumptions of local isotropy in the inertial subrange and Kolmogorov scaling. Some slight systematic differences may arise between the two methods owing to differences in the fitting procedure and measurement filtering."

- **l.147, p.7:** An intensity threshold value is introduced to handle noise contamination. High-frequency noise contamination is visible in Fig. 5a beyond $\sim 10^0$ Hz. Discarding 10-min periods based solely on intensity > 1.015 assumes all remaining values are noise-free. Given the relatively large probe size, noise characteristics depend on probe dimensions. Please justify why noise contamination remains limited for this instrument.

  Additionally, if filtering is based on an intensity parameter, more structure-function estimates might be discarded from certain beam orientations, further biasing ε estimates.

  Authors' Response: Fig 5 (a) refers to the sonic anemometer measurements, and the spectral noise at frequencies above 1 Hz is not used in the analysis. For the lidar measurements, the intensity parameter (signal-to-noise ratio) is a manufacturer-provided metric to diagnose the noise contamination. A value of 1.015 is recommended by the lidar manufacturer, which is already a very conservative threshold for noise control, as it removes a large amount of available data, especially at large heights above the ground, where the return signal quality is reduced. We also examined the dependence of beam orientation on the intensity parameter and found no systematic variation.

- **l.218, p.11:** It may also be possible to estimate turbulence dissipation rates using your method with other lidar technologies, such as the WindCube v2.1, by configuring a 3 m gate spacing over a limited number of altitudes (e.g., around 103 m and 241 m). Five gates may already be sufficient to apply your method locally. Therefore, the main advantage of the BEAM6X lidar is the possibility of achieving a 3 m resolution throughout the entire atmospheric column. This point should be stated more explicitly.

  Authors' Response: Thank you for pointing this out. We have added the following text to the Discussion Section (See line 309):

  "The method presented in this article is not specific to the BEAM 6x lidar. In principle, it could be applied to any other pulsed Doppler lidar if the range-gate spacing is sufficiently smaller over the region of interest. The advantage of BEAM 6x configuration here is that 3 m range gate spacing is available throughout the atmospheric column, allowing the method to be applicable over an extended range."

- **l.206, p.9:** I am usually not a strong supporter of mixing the Results and Discussion sections. Here, you clearly have enough material to develop a very impactful discussion section addressing temporal averaging, beam-direction asymmetry, noise contamination, and beam averaging limitations. Expanding the discussion around these points would considerably strengthen the manuscript.

  Authors' Response: Thank you for this excellent recommendation. We have separated the Results and Discussion Sections.

- **l.152, p.7:** You are using the term "longitudinal" both to describe the along-wind direction and the along-beam structure function. These correspond to two different directions and may create confusion. Reserve "longitudinal" for the along-beam direction only, and use "along-wind", "cross-wind", and "vertical" when referring to u, v, and w velocity components.

Authors' Response: Done.

- **l.285, p.15:** You appear to assume that platform motion has no effect on the estimation of ε. The LOS velocity time series will inevitably be contaminated by platform motion, which should be removed prior to estimating dissipation. Correcting structure functions on moving platforms requires projecting LOS measurements at each timestep into a corrected frame using instantaneous u_x, u_y, u_z, which are themselves subject to cross-contamination. The spectral method may be more robust than the structure-function approach on floating/moving platforms.

  Authors' Response: The platform motion can significantly contaminate turbulence quantities derived from temporal fluctuations (TI, spectra) of a floating-lidar LOS measurements. However, the longitudinal structure function (a function of space) is not affected by any additional velocity induced by the platform motion. Consider two range gates that provide LOS values in the same instantaneous beam direction. The longitudinal structure function uses the difference in LOS between the two gates. Since both range gates experience the same platform-velocity contribution, it cancels out. The platform motion may impact the calculated structure function in another way: by changing the instantaneous orientations of the beams. However, this impact is heavily reduced under the assumption of local isotropy, i.e., the longitudinal second-order structure function depends on the separation magnitude rather than the beam orientation in the inertial subrange. We believe that the longitudinal spatial structure function is a far superior method than the spectral method to estimate small-scale turbulence quantities from floating platforms, exactly due to the mentioned reasons.

Minor comments

- l.169, p.8, Eq. 9: Clarify the role of $F_v(k_1)$ or remove it if unnecessary.

  Authors' Response: We have emphasized in the text that F_v and F_w are the spectra of the two transverse velocity components

- l.176, p.8: Explicitly define $\varepsilon_{\text{lidar}}$.

  Authors' Response: We have added "meaning that we fit (6), where ε is the only unknown, to the measured lidar data" so it is explained where ε is coming from

- l.194, p.9: Report how many 10-minute ensembles were discarded by the 15% and 30% thresholds.

  Authors' Response: This is already mentioned in the Results section. See line 261 in the revised version.

- Define FWHM when first introduced.

  Authors' Response: Done

- DBS has already been defined earlier and need not be reintroduced.

  Authors' Response: Done

- Justify the use of the Pearson correlation coefficient and introduce it earlier in the methodology.

  Authors' Response: The following text has been added to the Workflow section. See line 224 in the revised version.

  "The agreement between the lidar- and sonic-derived dissipation rates is evaluated in log space because ε spans several orders of magnitude. Two different metrics are used to judge the quality of comparison. The first one is the Pearson correlation coefficient (ρ) calculated between $log_{10}(\varepsilon_{lidar})$ and $log_{10}(\varepsilon_{sonic})$. This metric represents the scatter between the lidar and sonic measurements, i.e., the strength of their linear association. To judge whether the sonic and lidar-obtained ε values vary at the same rate and if there is a systematic offset, a second metric is used: an ordinary least-squares (OLS) regression. For the OLS regression, $log_{10}(\varepsilon_{lidar}) = a + b\ log_{10}(\varepsilon_{sonic})$, where a and b represent the intercept and slope of the fit. "

References

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