

Review of Syed, Mann and Manami : "Measurement of turbulence energy dissipation rate by a standalone high-resolution Doppler lidar"

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Reviewer : Maxime Thiébaud

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. These points are detailed below. Note that the comments are not necessarily presented in the order of appearance in the manuscript.

Major comments

- 1.28, 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 Thiébaud et al. (2025) (cite if relevant), these two contributions define the transfer function applied by the instrument to the measured signal:

$$|H|^2(\mathbf{k}) = \text{sinc}^2\left(\frac{\Delta t}{2}\mathbf{k} \cdot \mathbf{U}\right) \exp\left(-\left[\sigma_t^2(\mathbf{k} \cdot \mathbf{b})^2 + \sigma_r^2\left(\|\mathbf{k}\|^2 - (\mathbf{k} \cdot \mathbf{b})^2\right)\right]\right), \quad (1)$$

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.

- 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.

- 1.75, 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.
- 1.93, 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. For instance, Sauvageot (1992) used Doppler radar measurements of atmospheric turbulence and reported $C_K = 2.1 \pm 0.1$. A similar value, $C_K = 2.1 \pm 0.1$, was also obtained by 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 .
- 1.183, 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:

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

where u' and w' are the turbulent velocities at the positions denoted by the subscripts [e.g., $u'_0 = u'(r_0)$]. Because the correlations at spatially separated points are expected to be lower than the correlations at the same point, D_{uw} can be approximated as:

$$D_{uw} = \overline{u'_0 w'_0} + \overline{u'_r w'_r} \sim 2\overline{u' w'} \quad (5)$$

Invoking the standard assumption of horizontal homogeneity of second-order turbulence moments, Eqs. (2), (3), and (5) can be combined to give $D_1 - D_3 \sim -4\overline{u' w'}$.

I found that speed-bin-averaged ϵ estimates from the upstream-facing beam tend to be larger than those from the downstream-facing beam by a factor ranging from 1.3 to 1.8. Therefore, dissipation rates estimated from the upstream-facing direction appear to be systematically biased high relative to the downstream-facing direction, even under relatively “normal” flow conditions.

This issue could become even more critical in complex terrain or in strongly disturbed flows such as wake-affected conditions, as mentioned in the introduction (1.24, p.1, “aircraft wake vortices”). In such situations, averaging all beams together may introduce significant biases in the estimation of ϵ . It is therefore unclear why the analysis was not restricted to the vertical beam only, which would avoid directional asymmetry effects associated with the inclined beams.

In addition, a wind rose showing the orientation of the six beams relative to the dominant wind directions would probably help the interpretation of the results and clarify the potential impact of upstream- versus downstream-facing beams on the estimated dissipation rates.

- 1.176, p.8: You are using two different methods to estimate the turbulence dissipation rate: the spectral method for the sonic anemometer measurements to obtain what I will call ϵ_{spec} , and the structure-function method for the lidar measurements to obtain what I will call ϵ_{SF} . From my experience, even when applied to the same instrument, there is often a tendency for $\epsilon_{\text{spec}} < \epsilon_{\text{SF}}$. This may therefore introduce an additional bias or methodological inconsistency when comparing lidar- and sonic-derived estimates of ϵ . The potential impact of using two different estimation techniques should be discussed more carefully in the manuscript.
- 1.147, p.7: An intensity threshold value is introduced to handle noise contamination. In my opinion, the proper characterization of noise is fundamental for turbulence dissipation rate estimates. In Fig. 5a, there is a clear contamination of the spectrum by noise at frequencies higher than approximately 10^0 Hz. By discarding only the 10-min periods for which the intensity parameter exceeds 1.015, it means that all remaining values are

implicitly assumed to be acceptable. I am not asking for a complete characterization of the noise contribution, as this would likely require revisiting the entire analysis, but the discussion should be strengthened.

Given the relatively large probe size of this type of device, and considering that noise characteristics are known to depend on the probe dimensions, one could argue that noise contamination may remain limited for this particular instrument. However, this point should be explicitly justified.

In addition, this issue is directly connected to my previous comment regarding the use of all six beams to compute the structure function. Since the filtering is based on an intensity parameter, it is possible that more structure-function estimates are discarded from some beams than from others. Depending on the beam orientation (e.g., upstream-versus downstream-facing beams), this could further bias the resulting ϵ estimates.

- 1.218, 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. For example, around 103 m, one could select the gates 97, 100, 103, 106, and 109 m, and around 241 m: 235, 238, 241, 244, and 247 m. In my opinion, five gates may already be sufficient to apply your method locally.

Therefore, the main advantage of the BEAM6X lidar is not necessarily the method itself, but rather the possibility of achieving a 3 m resolution throughout the entire atmospheric column. This point should be stated more explicitly in the manuscript.

With this technology, one can retrieve vertical profiles of the turbulence dissipation rate. However, for applications such as model validation or TKE budget analyses, it is not always clear whether a full profile is actually required. In some situations, estimates at only one or two altitudes may already be sufficient, in which case a more conventional system such as the WindCube could potentially be adequate.

- 1.206, 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. In particular, several methodological aspects raised throughout this review could naturally be addressed there, such as the impact of temporal averaging, beam-direction asymmetry, noise contamination, and the limitations associated with averaging all six beams. Expanding the discussion around these points would considerably strengthen the manuscript.
- 1.152, p.7: You are using the term “longitudinal” both to describe the along-wind direction and the along-beam structure function (1.109, p.4, and possibly elsewhere in the manuscript). However, these correspond to two different directions and may create confusion for the reader. I would suggest reserving the term “longitudinal” for the along-beam direction only, and instead using “along-wind”, “cross-wind”, and “vertical” when referring to the u , v , and w velocity components, respectively.
- 1.285, p.15: You appear to assume that platform motion has no effect on the estimation of ϵ . It is not clear why this would be the case. The LOS velocity time series will inevitably be

contaminated by the platform motion, and this contribution should therefore be removed prior to estimating the dissipation rate.

However, this correction is not straightforward. Since your method is based on the structure function of the LOS velocities, one would need to project the LOS measurements at each time step into a corrected coordinate system accounting for the instantaneous motion of the lidar. To perform such a projection, instantaneous estimates of u_x , u_y , and u_z would be required. Yet these reconstructed velocity components are themselves affected by cross-contamination, which would then propagate into the corrected LOS time series and ultimately contaminate the ϵ estimates.

In my opinion, the spectral method is likely more appropriate for dissipation-rate estimation when the lidar is deployed on a moving platform, as it may be more robust to motion-induced contamination than the structure-function approach.

Minor comments

- l.169, p.8, Eq. 9: You present $F_v(k_1)$, but this quantity does not appear to be used anywhere else in the manuscript. Please clarify its role or remove it if unnecessary.
- l.176, p.8: I could not find a clear definition of ϵ_{lidar} anywhere in the manuscript. I assume it is derived from Eq. 6, but this should be stated explicitly.
- l.194, p.9: The thresholds of 15% and 30% appear somewhat arbitrary. How many 10-min ensembles were discarded using these criteria? Providing this information would help assess the impact of the filtering procedure on the dataset.
- Please define FWHM when it first appears in the manuscript.
- l.145, p.6: DBS has already been defined in l. 37 and does not need to be reintroduced.
- l.223, p.11: It is not clear why the Pearson correlation coefficient was chosen to quantify the agreement or error between the measurements. While I am not necessarily against using it, this choice is not very common for this type of analysis and should therefore be briefly justified. In addition, the metric should be introduced earlier in the methodology section rather than appearing for the first time in the Results section.

References

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- Sauvageot, H.: *Radar meteorology*, Artech House Publishers, 1992.
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