Deriving atmospheric turbulence intensity from profiling pulsed lidar measurements

Reviewer's comments

1 General comment

The manuscript "Deriving atmospheric turbulence intensity from profiling pulsed lidar measurements" by Thiébault et al. deals with the estimation of the turbulence intensity of the horizontal wind velocity component using the 5-beam DBS scanning mode of the WindCube v2.1 lidar. The manuscript focuses first on the turbulent kinetic energy (TKE) spectrum and then assesses the accuracy of the different methods and lidar configurations to study the turbulence intensity using various metrics.

The manuscript addresses challenging, contemporary and interesting questions about turbulence measurements at heights relevant to large wind turbines. Therefore, the paper is of broad international interest and within the scope of Wind Energy Science (WES), although it may be better suited to Atmospheric Measurement Techniques (AMT). The paper brings a novel idea for the calculation of the velocity variance, inspired by ocean science, which is welcome. The objective and methods are clearly outlined.

However, the analysis and method need some improvements as they sometimes rely on unverified or incorrect assumptions. Some preliminary steps lack rigour. More critically, the discussion and results are sometimes self-contradictory, for example, the discussion on the probe length. The content can be more concise. For example, section 3.1 (turbulent kinetic energy spectra) diverges sometimes from the initial scope of the paper. Section 3.3 could be summarized in a couple of sentences. Even if the paper is smoothly written and read quite easily, a major revision is, therefore, needed. In summary, the manuscript relies on a good idea, but the method, results and discussion need considerable improvements to be considered for publication in WES.

2 Specific comments

Point 1

The turbulence intensity (TI) can be calculated for the three wind velocity components: I_u, I_v and I_w with $I_u > I_v > I_w$. For wind turbine design, the along-wind component u and cross-wind components v are the most important. In the manuscript, eq (3) gives the turbulence intensity of the horizontal components, i.e. $u^2 + v^2$, which is of limited interest for wind turbine design. Therefore, I suggest focusing on the u and v components separately.

Point 2

The manuscript correctly mentions that the TI is a central and practical metric for the study of atmospheric turbulence. However, the velocity spectrum is ultimately needed for the wind loading calculation. Therefore, the importance of the TI for wind energy and wind engineering should not be overestimated. This aspect may be pointed out in the introduction of the manuscript.

Point 3

Line 29-30: I suggest not to write that lidars have emerged as an alternative to met masts but rather as a "complementary tool". This is particularly true for turbulence measurements: both profilers and scanning Doppler wind lidars are currently unable to match the performances and data availability of 3D sonic anemometers.

Point 4

Line 41: The manuscript argues that the along-beam spatial averaging effect will not be addressed. I think it should, on the contrary, be studied. Several of the questions raised by the manuscript can be linked to the fact that the along-beam spatial averaging is not discussed jointly with the sampling frequency, for example.

Point 5

In the manuscript, arguments for using a higher sampling frequency are given. I agree that increasing the sampling frequency f_s should helps reducing the statistical uncertainties (see e.g. Kaimal and Finnigan (Section 7.1, 1994)). However, because of the along-beam spatial averaging of 20 m or more, increasing f_s to values higher than 1 Hz (or even 0.25 Hz?) may not help to capture smaller eddies.

Point 6

Section 2.2: A sketch to show the different wind velocity components in the wind-based coordinate system (u, v and w) and the lidar-based coordinate system V_x, V_y, V_z would be a good idea for the sake of pedagogy.

Point 7

Section 2.3.2: It is a little unclear to me where the pitch and roll angle comes from. I understand that these angles are based on the publication by Dewey and Stringer (2007) about the ADCP system, but I don't know if these are necessary for a Doppler wind lidar. If yes, how were they measured? were they non-negligible?

Point 8

As far as I know, the study by Dewey and Stringer (2007) is actually unpublished. The reference used in the manuscript seems to be given by Google Scholar, which may be incorrect.

Point 9

Eq. 10 seems to be unnecessary complex since one could argue that $w \approx b_5$. Section 3.3. of the manuscript seems to show that assuming $w \approx b_5$ is good enough. Therefore, I suggest moving and squeezing the content of section 3.3. right after equation 10. In general, one wants to avoid presenting an excessively heavy formula for the sake of clarity.

Point 10

Section 2.4.1: The company Thies Clima produces different types of sonic anemometers. It is unclear to me which model was used. Note that if a 2D sonic anemometer was used, one should keep in mind that this type of sensor is mainly used as a weather station. So this sensor is not expected to perform as well as a 3D sonic anemometer to study turbulence. This is an important point to remember if the goal is to validate Doppler wind lidar turbulence measurements against sonic anemometry.

Point 11

Fig 2 contains elements that are too small to be easily readable. I suggest redrawing it. Ideally, the figure should indicate whether the sonic anemometer is on the northwest or southeast side of the tower. From the text, it seems it is on the southeast side. Maybe a digital Elevation Map can be used instead of satellite images? Otherwise, it can be difficult to visualize the local topography.

Point 12

Line 170: The comparison between sonic anemometer data and the lidar data is done using the same sampling frequency f_s (0.25 Hz and 1 Hz). I agree with the authors on this approach. However, it also means that the turbulence intensity will be underestimated compared to a standard 3D sonic anemometer using $f_s > 10$ Hz. This should be clarified in the manuscript.

Point 13

Line 172-178: There is no guarantee that a 10 min duration is short enough to ensure stationarity of the velocity fluctuations. To assess the assumption of stationarity, a trend test (non-parametric) or a parametric test should be used.

Point 14

Line 172-178: The standard deviation σ_H of the horizontal wind velocity component H is studied using an averaging time of 10 min. For turbulence measurements, averaging times of 30 min to 60 min are often used. Considering the Simiu and Scanlan spectrum for the along-wind component u, it can be shown that using 10 min instead of 60 min (30 min) will lead to an underestimation of σ_u by 15% (11%).

The standards and codes for wind turbine design use often 10 min (i.e. 600 s) as averaging time because the wind loading is computed utilizing the velocity spectra. Considering frequencies down to $\frac{1}{600\,\mathrm{s}} = 0.00167$ Hz is often good enough to describe the full range of vibrations of a large engineering structure. However, it does not mean that 10 min is long enough to study integral turbulence characteristics.

Point 15

Line 186: Does the 75% data availability means that time series with 25% or less of NaNs were kept? if yes, this may be too much. I suggest dismissing samples if the percentage of Nans is 10% or higher. In atmospheric science, the acceptable percentage of NaNs is usually from 2.5% to 5%, but this may be too strict for Doppler wind lidar data.

Point 16

Line 187-188: I do not understand the sentence “A percentage ranging [...] was rejected”. May it be possible to clarify it?

Point 17

Section 2.6: For the study of the gradient Richardson number, I recommend using the potential temperature θ or virtual potential temperature θ_v instead of the absolute temperature T. From figure 2 in the manuscript, the pressure and humidity seem to be measured at 10 m and 95 m, so θ and θ_v may be calculated successfully. It may also be a good idea to use a more accurate classification of atmospheric turbulence than $R_i < 0$ or $R_i > 0$, which could be too rough.

Point 18

Section 3.1: This section contains some unnecessary sentences (lines 211-215) and may have to be re-written to better anchor it to the research question. Figure 3 should be redrawn. It is vital to better highlight the presence (or absence) of an inertial subrange. Because of the low-sampling frequency, the cross-contamination and the spatial averaging, the inertial subrange may not be easily visible.

To improve the visualization of the velocity spectra: (1) the velocity spectra should be split between u and v components, and (2) the velocity spectra should be pre-multiplied with the frequency or wavenumber. If needed, you can further normalize the spectra using the variance σ_u^2 measured by the sonic anemometer at 95 m; (3) The frequency should be replaced by the wavenumber or a normalized frequency (4) the velocity spectra should be split between unstable or stable stratifications.

Point 19

Line 236-243: These lines seem unnecessary. They could be removed without affecting the content of the paper.

Point 20

Have you tried estimating the standard deviation of the u and v components by integrating a fitted empirical velocity spectrum as

$$\sigma_u^2 = \int_0^{\infty} S_u(f) d_f \tag{1}$$

where f is the frequency in Hz and S_u has the following form:

$$fS_u(f) = \frac{Af_r}{(1+Bf_r)^{5/3}} \tag{2}$$

$$f_r = \frac{fz}{\overline{u}} \tag{3}$$

where A and B are empirically obtained by least-square fit to the estimated spectrum and $\overline{u}$ is the mean wind speed at height z. If yes, how does this method compares with the noise-removal approach adopted in the present study? Note that you may have to apply a method similar to Kelberlau and Mann (2020) to reduce the cross-contamination in the velocity spectrum.

Following Kelberlau and Mann (2020), the velocity spectra from DBS scans should not be fitted by turbulence models. Although they do not specify what they call "turbulence models", one can assume they refer to the uniform shear model or 3D isotropic spectral turbulence models. I ignore if their recommendation for "turbulence models" includes eq. (2). Nevertheless, attempting to estimate σ_u using eq. (1) and eq. (2) may be worthwhile, especially when the lidar system is aligned with the mean wind direction.

Point 21

Fig 4: It is unclear to me what the figure aims to demonstrate. If the quadratic relationship is not used elsewhere in the manuscript, should it be kept? Is the R^2 value shown in this figure the Pearson or Spearman correlation coefficient? I suggest using the Spearman correlation coefficient if the relationship between the two variables is non-linear monotonic. Alternatively, the RMSE can be used instead.

Point 22

Fig 6 does not seem to clearly support the conclusions of the manuscript. Was it because there was no noise removal here? I encourage the use of a colormap that is perceptually uniform instead of the jet colormap.

Point 23

Section 4: The discussion seems to recommend a large probe length to study turbulence. In general, when turbulence is studied with a Doppler wind lidar, one wants the probe volume length to be as small as possible. A 20 m probe volume length is already quite large, which is the reason continuous-wave Doppler (scanning) lidars with a probe volume length smaller than 10 m have a higher potential than pulsed lidar to study turbulence. Therefore, this section may need some reformulations.

Point 24

Lines 381-394: This paragraph seems more adapted to the beginning of the manuscript since it reviews some previous results. Maybe this can be moved there and shortened?

Point 25

The conclusion may have to be reformulated as it includes several recommendations that could be criticized. I agree that operating a lidar at a higher sampling rate is a good idea, but this will not be useful if the probe volume is not reduced. As pointed out by the authors, reducing the probe volume increases, in return, the measurement noise. So the situation is rather complex. Does it mean that DBS scans should only be used to study the mean flow characteristics? Should new scanning modes be developed instead?

Also, the conclusion mentions the use of lower beam inclination θ to improve the measurement accuracy. This may be a good idea. However, if θ becomes too small, the measurement uncertainty will increase substantially because the angle between the beam and the horizontal direction will get close to 90°. So the ideal value of the beam inclination theta is not trivial either. Do you have any specific value in mind?

Point 26

The manuscript contains some typographical errors. I recommend a quick proofread. The online web app Grammarly (`https://app.grammarly.com`) is quite good for this purpose.

References

Kaimal, J.C., Finnigan, J.J.. Atmospheric boundary layer flows: their structure and measurement. Oxford university press; 1994.

Dewey, R., Stringer, S.. Reynolds stresses and turbulent kinetic energy estimates from various adcp beam configurations: Theory. Unpublished 2007;.

Kelberlau, F., Mann, J.. Cross-contamination effect on turbulence spectra from doppler beam swinging wind lidar. Wind Energy Science 2020;5(2):519–541.