Deriving atmospheric turbulence intensity from profiling pulsed lidar measurements - wes-2022-53

Response on reviewer's comments - RC1

October 21, 2022

Thank you to the three reviewers (RC1/RC2 and CC1) for their very valuable comments on our manuscript. We rewrote more than half of the paper to satisfy their recommendations. The main changes appear in blue in the revised version. The main changes are the following:

- In the introduction, a paragraph has been added to put into context the noise (doppler noise) as it is defined in the present paper and the noise (signal-to-noise) addressed in the literature.
- In "Data collection and methods", a full section is now dedicated to the definition of the Doppler noise. The influence of the cell size and sampling rate of the magnitude of the Doppler noise are addressed. A step by step procedure is proposed to evaluate the Doppler noise and its variance that induces overestimation of TI. In addition, the cutting frequency, set to 80% of the Nyquist frequency in the first version of the manuscript is now determined by a method involving an error minimization of the least-square regression of the LOS velocity spectra.
- Section 3.1 (turbulent kinetic energy spectra) and section 3.3 (Vertical stress) have been removed. Section 3.1 was considered out of the scope of this paper by the reviewers. Section 3.3 was considered irrelevant.
- Mathematical expressions of $\overline{u'^2}$ and $\overline{v'^2}$ do not consider the pitch and roll anymore. The deployment of both lidars were done such as the pitch and roll were almost zero, there are thus negligible. Without pitch and roll, expressions of $\overline{u'^2}$ and $\overline{v'^2}$ match the classic expressions proposed by [1]. $\overline{w'^2}$ was removed because it has been considered irrelevant for this study.
- A stationarity study of the 10-min subsets of the LOS velocity time series is proposed through the Augmented Dickey-Fuller test.
- The discussion and conclusion have been completely rewritten. Limits of the variance method are discussed. The "benefice" of the higher sampling rate is also discussed with regards to other limitations of lidars such as the probe-volume averaging. Recommendations are made to improve the next generation of lidars such as the addition of extra beams and simultaneous acquisition of the LOS velocities.

1 General comments

The manuscript “Deriving atmospheric turbulence intensity from profiling pulsed lidar measurements” by Thiébaut 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.

Reply

We agree that the along and cross-wind components are the most important for wind turbine design. Our first idea was to rotate the variance $\overline{u'^2}$ and $\overline{v'^2}$ such as the velocity component u_{rot} is aligned with the mean wind direction and v_{rot} is forced to 0. The rotated variance are described as follows [2, 3]:

$$\overline{u'^2}_{\mathrm{rot}} = \overline{u'^2}\sin^2\Theta + \overline{v'^2}\cos^2\Theta + \overline{u'v'}\sin 2\Theta \tag{1}$$

$$\overline{v'^2}_{\mathrm{rot}} = \overline{u'^2}\cos^2\Theta + \overline{v'^2}\sin^2\Theta - \overline{u'v'}\sin 2\Theta \tag{2}$$

where Θ is the mean wind direction and the subscript "rot" refers to variance components in the rotated coordinate system. Equations (1) and (2) involve the shear stress $\overline{u'v'}$. This is the only component of the Reynolds stress tensor that remains unknown from WindCube 2.1 measurement. However, $\overline{u'v'}$ can be calculated from measurement of the sonic anemometer. For example, in [3], the authors derived $\overline{u'v'}$ from sonic anemometer measurements and found that $\overline{u'v'}$ was near zero throughout the day and thus had a negligible effect on the coordinate rotation, thus justifying to neglect the term $\overline{u'v'}$ in Equations (1) and (2). In our study, however, we decided to use the sonic anemometer measurements only for validation of TI and not for validation of assumptions since the trend in the offshore wind community is to use lidars (and lidar only) instead of anemometers mounted on tower to perform the site assessment stage. We added a sentence specifying this point (Line 165). We also mentioned in the discussion (updated version of the manuscript) that a possible way to apply the variance method on the along and cross wind velocity time series will be the addition of a sixth beam to the lidar as it has been done in Ocean science with ADCP. This will allow to resolve the six components of the Reynolds stress tensor.

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.

Reply

We tempered the impact of TI for wind energy applications in the introduction with these new sentences: "The key parameters are the turbulence intensity, TI, i.e., the ratio of velocity fluctuations to the velocity mean, and the turbulent kinetic energy (TKE) spectra, i.e., the velocity variance as a function of frequency. Both parameters are directly involved in modeling applications of wake effects within wind farms which can significantly decrease the power production and increase the cost of electricity (Howland et al., 2019)". Lines 17-20.

Point 3

Lines 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.

Reply

We changed "alternative" by "complementary". Line 30.

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.

Reply

This point is now addressed in the discussion (Lines 305-321) and conclusion (Lines 361-366). As you mentioned in Point 5, it is likely that the increase of the frequency rate by a factor 4 in comparison to the commercial lidar, is still not enough to bring any improvement in TI estimates. The eddies, and their associated variance, that are supposed to be captured with the higher sampling rate might remains undetectable because of the probe-volume averaging.

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. [4] (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.

Reply

We replied to this remark in Point 4. After reading the section you recommended, we agree that the "better" performance of the prototype configuration in TI reconstruction is probably due to the reduce statistical uncertainties that brings the higher sampling rate. We now mentioned this point in the discussion. Lines 322 - 326.

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.

Reply

We added a sketch in Figure 2 (left hand-side figure) showing the orientation of the beam 1, 2, 3 and 4. Beam 1 is aligned with u and V_x and beam 4 is aligned with v and V_y.

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 [5] 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?

Reply

Both pitch and roll are given by the lidar interface during the deployment and there are also available in the header of each .rtd files. A proper deployment requires to set the pitch and roll to values close to zero. In our deployment, the pitch and roll of the commercial configuration were set to -0.126° and 0.021° respectively and the pitch and roll of the prototype configuration were set to -0.74° and 0.23° respectively. We agree that in the case of bottom-fixed lidar installation the pitch and roll are negligible. We wanted to stick to the full expressions proposed by Dewey and Stringer. In the revised version of the manuscript, the expressions are simplified by considering that the pitch and roll are negligible (Eq. 12 and 13).

Point 8

As far as I know, the study by Dewey and Stringer [5] is actually unpublished. The reference used in the manuscript seems to be given by Google Scholar, which may be incorrect.

Reply

You're correct. The study by Dewey and Stringer has never been published. However, this manuscript is more and more employed in the field of ocean science since the development of a new generation of ADCP employing five beams instead of four. The fifth beam being oriented vertically upward.

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.

Reply

We removed this equation and the section 3.3 which are unnecessary according to the two others reviewers.

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.

Reply

The sonic anemometer is a 3D Thies (No. 4.3830). We added this information on the updated version of the manuscript. Line 207.

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.

Reply

We redraw the Fig. 2 following your suggestion. There is now a map showing 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.

Reply

The downsampling will indeed bring underestimation of TI. We illustrated this point by calculating TI derived from time series sampled at 4 Hz and downsampled at 1 Hz and 0.25 Hz respectively. This gave the respective values of 7.4%, 7.2% and 6.95%. Lines 228-231.

Point 13

Lines 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.

Reply

We added a section dedicated to the stationarity study of the 10-min subsets of the LOS velocity time series (section 2.7). The stationarity has been evaluated by the Augmented Dickey-Fuller (ADF) test. ADF tests the null hypothesis that a unit root is present in a time series sample. The interpretation of the result is done using the p-value given from the test. A p-value of less than 5% means the test rejects the null hypothesis, thus, the time series is stationary. p-value of the 10-min LOS velocity time series associated each beam of the commercial and prototype configurations was found varying within the range [1.1%, 2.3%], thus, we can argue that the 10-min LOS velocity time series are stationary and that the 10-min temporal window is of sufficient length to perform turbulence analysis of the present wind dataset.

Point 14

Lines 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

$\frac{1}{600s} = 0.00167$ 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.

Reply

We totally agree on this point. We performed our analysis on 10-min subsets only because the temperature and pressure at our disposal were 10-min averaged values. We needed these values to calculate the potential temperature and classify the dataset according to stable and unstable atmospheric conditions. Otherwise, we would have chosen to perform the turbulence analysis on 30-min subsets.

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. As a result, 326 more subsets were removed from the analysis.

Reply

Yes, 75% data availability means that time series with 25% or less of NaNs were kept. However, we followed your recommendation and set the threshold of data rejection to 90% of data availability within each 10-min subset.

Point 16

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

Reply

We rephrased this: ”For the commercial configuration, the maximum percentage of subsets rejection, i.e., 1.7%, was associated with the beam 4 whereas the minimum percentage, i.e., 0.6%, was associated with the beam 1. For the prototype lidar, these percentages were found to be more than twice higher”. Lines 207-209.

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.

Reply

Your comment is in line with a comment of the second reviewer. In the revised version of the manuscript, the classification was done through the study of the sign of the vertical gradient of the potential temperature, $d\theta/dz$. A convective unstable wind flow is associated with $d\theta/dz < 0$ while stable wind flow is associated with $d\theta/dz > 0$ as proposed in [6]. Our first idea was to decompose the dataset into 3 atmospheric classes (unstable, stable and strongly stable) instead of only 2. However, just few subsets (≈ 50) were recorded during strongly stable conditions. It is a too low number in comparison to the number of subsets recorded during stable and unstable conditions. Thus, the comparison of TI would have been biased.

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.

Reply

The section 3.1, and thus the spectra, have been removed in the updated version of the manuscript. However, the mean spectrum of the LOS velocities associated with the beam 1 of the commercial and prototype lidars is shown. The mean spectra are shown to illustrate the step-by-step method for the Doppler noise identification. For this step, an identification of the cutting frequency is required, thus justifying the representation of the frequency instead of the wavenumber. The representation of spectra recorded during stable and unstable conditions is now irrelevant.

Point 19

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

Reply

These lines have been removed.

Point 20

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

$$\sigma_u^2 = \int_0^\infty S_u(f)df \tag{3}$$

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

$$fS_u(f) = \frac{Af_r}{(1+Bf_r)^{5/3}} \tag{4}$$

$$f_r = \frac{fz}{\overline{u}} \tag{5}$$

where A and B are empirically obtained by least-square fit to the estimated spectrum and 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 [7] to reduce the cross-contamination in the velocity spectrum.

Following [7], 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. (4). Nevertheless, attempting to estimate σ_u using eq. (3) and eq. (4) may be worthwhile, especially when the lidar system is aligned with the mean wind direction.

Reply

We did not try to solve the standard deviation by fitted an empirical velocity spectrum. However we plan to test this approach with the measurements we are going to collect in Le Planier Island, in the Mediterranean Sea. The lidar will be deployed such that one beam, let's say beam 1, will be aligned with the Mistral to get direct measurements of the turbulence structures associated with this specific wind event. With the removal of the Doppler noise, the eq. (3) would become (for beam 1):

$$\sigma_1^2 = \int_0^{\infty} [S_1(f) - N_1] df \qquad (6)$$

where N_1 is the constant spectral density associated with the Doppler noise. N_1 would be determined following section 2.3 of the revised version of the manuscript. The result would the fitting of a spectrum whose energy is slightly lower than the spectrum fitted with eq. 3. However, and you mentioned this point, it is not recommended to fit a spectrum derived from DBS measurements, specifically the part where probe-volume averaging is the most important, i.e., the inertial domain and its associated -5/3 slope. Unless we know how much filtering we have due to the probe-volume averaging and how this affect the spectra, we cannot just compare the slopes of the measured lidar spectra with classical turbulence slopes like the –5/3 one. See major comment n°7 of the reviewer CC1.

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.

Reply

The fitting of the scatterplots is irrelevant. We removed it. The purpose of this figure is to show that the Doppler noise increases with increasing wind speed and that the prototype configuration gives Doppler noise lower than the commercial configuration probably because the noise is distributed along a wider frequency range.

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.

Reply

Maybe it wasn't clear in the conclusion. We rewrote this part. Globally the variance method gives half of TI estimates that are overestimated because of the cross-contamination effect which means that during certain wind conditions the main source of error is the cross-contamination, whereas, the other half is underestimated which means that the volume averaging is the mean source of error. In figure 6 (first version), now figure 7, includes a removal of the Doppler noise. This generates scatter TI estimates because the removal of the Doppler noise is not made on individual fluctuations but rather on the mean fluctuations averaged over each 10-min subset. Thus, the noise correction is sensitive to the number of realizations considered and Doppler noise will always result in some spreading of the corrected TI estimates. That is the main drawback of this method. Moreover, we redraw the figure and used a uniform color map 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.

Reply

Our recommendation was awkward. We agree. It is relevant for acoustic Doppler current profiler (ADCP) deployment in ocean science since the cell size are configurable and can be set to only few dozen of centimeters. This recommendation is not appropriate for the 20 m probe length of the WindCube. A recommendation would rather be the reduction of the probe length to capture smaller eddies. This will induce a higher level of noise whose variance can be relatively well evaluated and remove from turbulence estimates.

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?

Reply

The discussion has been completely rewritten. We removed this paragraph.

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 ϕ to improve the measurement accuracy. This may be a good idea. However, if ϕ 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 ϕ is not trivial either. Do you have any specific value in mind?

Reply

The conclusion has been completely rewritten. We now agree that a faster sampling rate would have a limited interest as soon as the probe length remains the same. A reduction of the probe length would be the priority before eventually considering an increase of the sampling rate.

DBS scans is indeed relevant to study mean flow characteristics but it comes with several limitations to study the wind fluctuations. Instead of swinging from one beam to another (DBS technique), a new technique would be to collect LOS velocity simultaneously as it is done with ADCP in ocean science. This has the potential to limit or annihilate the cross-contamination effect which generate systematic overestimation of TI due to the two-point correlation of different wind field components. Addition of extra beams should also be investigated. This would enable resolving the full Reynolds stress tensor thus allowing the application of the variance method to provide TI estimates associated with the along and cross-wind components as required by the wind energy industry.

Moreover, as you said, the reduction of the beam inclination, ϕ is not trivial. The angle of 28° is optimum in terms of accuracy of the measured wind speed and direction and it is unlikely that effort would be put on reducing this angle for the next generation of WindCube. Note that the beam inclination of ADCP, for example the Nortek Signature, is 25°.

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.

Reply

We did a quick proofread. Thank you for proposing the web app Grammarly. Very useful.

References

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[7] F. Kelberlau and J. Mann, "Cross-contamination effect on turbulence spectra from Doppler beam swinging wind lidar," *Wind Energy Science*, vol. 5, no. 2, pp. 519–541, 2020. Publisher: Copernicus GmbH.