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

Response on reviewer's comments - RC2

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

Thiébaut et al. describe a new method to estimate turbulence intensity from profiling lidar measurements. Such studies are quite relevant for site assessment in wind energy. Better estimates of turbulence intensity are very important for load predictions. It is well known that profiling lidars have some shortcomings when it comes to turbulence estimation and new methods to improve the results are thus welcome. The idea in this manuscript to use methods from acoustic Doppler current profilers (ADCP) which are used in oceanic sciences is interesting and attractive. Three main changes are made to commercial profiling lidars: the sampling rate is increased, the variance method as used with ADCPs is implemented and a noise removal is applied. While the increase of sampling rate is straight forward and the explanations are easy to follow, the critical differences between ADCP and profiling lidars for atmospheric measurements are not sufficiently well elaborated. For this reason, I cannot suggest the paper for submission in WES before some major revisions.

Point 1

The main difference between ADCP and lidar DBS is the character of the atmosphere, which can be much more instationary than ocean currents, and the fact that a lidar DBS does not measure all beams simultaneously. A minor difference is that the onshore lidar as it is used in this study does not move, so it has a constant roll and pitch angle which equals zero in best case. It is unclear why the authors use the full roll and pitch equation from Dewey and Stringer (2007) and not the much more simple zero pitch and roll equations. From those equations, it would also be very easy to check if the assumptions of homogeneity and stationarity are valid for the dataset by checking the equations 11 and 12 from Dewey and Stringer against the sonic anemometer data:

$$\overline{b_1'}^2 + \overline{b_2'}^2 = 2\overline{u'^2}\,\sin^2\phi + 2\overline{w'^2}\,\cos^2\phi \tag{1}$$

$$\overline{b_3'}^2 + \overline{b_4'}^2 = 2\overline{v'^2}\,\sin^2\phi + 2\overline{w'^2}\,\cos^2\phi \tag{2}$$

Does the left hand side as measured from the lidar DBS equal the right hand side as measured from the sonic anemometer for different wind speeds and different stability conditions? Only checking the vertical wind is not sufficient.

Reply

Mathematical expressions of $\overline{u'^2}$ and $\overline{v'^2}$ do not consider the pitch and roll anymore. The deployments 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 as irrelevant for this study.

Your proposition of using eq. (7) and (8) above is indeed a good idea. However, the vertical velocity were not recorded by the sonic anemometer to avoid overload of the data logger. Thus, we cannot check the homogeneity and stationarity following your suggestion. In the revised version of the manuscript, a stationarity study of the 10-min subsets of the LOS velocity time series is proposed through the Augmented Dickey-Fuller test.

Point 2

The definition of Doppler noise seems to be a quite vague to me. Parts of the explanation are given in different parts of the manuscript, but it should be better introduced in the beginning. It is not true that lidar noise has not been studied and included in models for turbulence retrieval from lidars before. The work by R.G. Frehlich as well as I. Smalikho and E.J. O'Connor contain much information about lidar noise. The noise which is meant here should be put into context to these other studies.

Reply

In the revised version of the manuscript, a full section (section 2.3) 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 mentioned. A step by step procedure is proposed to evaluate the Doppler noise and its variance that induces overestimation of TI.

In the introduction, the noise as it is defined in the present manuscript is put into context. Previous works have been investigated noise in term of signal-to-noise (SNR). Lines 48 - 54.

Point 3

The way that stability is derived from the Richardson number here is not correct for atmospheric sciences. What is calculated is the bulk Richardson number, which has a non-zero critical value between stable and unstable flow. This has an impact on the whole comparison between stable and unstable conditions in this study and needs to be revised.

Reply

Your comment is in line with a comment of the first reviewer. In the revised version of the manuscript the classification is 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 [2]. The decomposition proposed in the first version of the manuscript was wrong. Now, 72.7% of the 10-min subsets are associated with unstable conditions whereas 27.3% are associated with stable conditions.

2 Specific comments

- p.2,l.40: what about availability?

  Reply

  Thank you, we added this limitation. Line 42.

- p.2, ll.51f: lidar noise is explored quite extensively in works by R.G. Frehlich, I. Smalikho and E.J. O'Connor. These works should be reflected.

  Reply

  This comment is addressed in Point 2.

- p.4,l.83: "volume of atmosphere"? This is a strange expression.

  Reply

  This expression has been removed.

- Fig.2: basic information of the maps are missing: scale, northing, lat/lon, copyright of the map pictures.

  Reply

  Fig. 2 has been modified following your suggestion.

- p.8,l.170: I do not understand the reason for the resampling of the sonic. Is it not the goal to compare the lidar derived TI to the best possible measurement? If the sonic is downsampled, it will also lack very small scale turbulence. Probably the difference is not much, but I would suggest to use the best possible TI estimate for comparison, unless you want to isolate the errors by specific processing steps.

  Reply

  We wanted to isolate the error due to the different sampling rates. The first reviewer agree on this approach (see point 12 - RC1). We added three sentences in the revised version: "Due to the resample, the reference TI estimates presented in this paper are underestimated. To illustrate this point, the median of the reference TI was calculated from the 10-min subsets sampled at 4 Hz and downsampled at 1 Hz and 0.25 Hz. This gave the respective values of 7.4%, 7.2% and 6.95%". Lines 195-198.

- p.8,l.174: it is a questionable statement that 10-minutes are enough to retain the longest ime scales of coherent turbulent structures. In basic ABL research, 30-minutes are rather the standard.

  Reply

  This comment is in line with a comment of the first reviewer (Point 13 - RC1). We agree that 30-min is the standard. 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.

  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 5rejects 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.

- p.8, l.197: It is too much simplified to use the measured temperature difference and standard atmosphere lapse rate for the calculation of the Richardson number. Please calculate the potential temperature difference.

  Reply

  In the revised version, we calculated the potential temperature to classify our dataset. See response to your comment "Point 3".

- p.9, l.205: It might be partly because of the way the Richardson number was calculated, but it is certainly a strong overestimation if 89.4% of the data are considered to be measured in a stable atmosphere. What is actually calculated here is the bulk Richardson number which has a critical value of 0.25 for unstable flows.

  Reply

  Exactly, it was a strong overestimation. We made a mistake here which has been fixed in the revised version.

- p.11, ll.242f: I am less concerned about the inertial subrange being present at frequencies higher than the Nyquist frequency, but more concerned if the integral length scale is large enough to yield an inertial subrange at the resolved frequencies, especially in stable atmospheric conditions. Typically, for lidar turbulence retrievals, there is a minimum integral length scale below which the estimates need to be discarded.

  Reply

  Absolutely. With a pulsed lidar employing DBS scan, we think that the limited turbulence length scale that can be measure accurately in the light-of-sight direction is the probe length (20 m for the WindCube). However, for turbine design specifications, turbulence length scales associated with the wind horizontal wind components are the most relevant. The characterization of these scales and their associated turbulent energy requires the combination of beams. Thus, when characterizing horizontal turbulence, the beam spread is the limiting length scale. Due to the diverging beams, the volume, proportional to the beam spread Δb, in which the measurements are being integrated increases with increasing altitude, changing the volume averaging of the turbulence metrics. A recommendation would be to restrict the analysis to length scales (and corresponding frequencies) that are higher than the beam spread. The beam spread is a function of height, H, above ground such that $\Delta b = 2H\tan\phi$. Considering the beams inclination, $\phi =$

28°, of the WindCube v2.1, the beam spread between two opposite beams at 97-m altitude is 103 m which matches the land-based wind turbines rotor diameter. Since wind turbines respond to turbulence on scales similar to the rotor diameters [$\mathcal{O}(100)$ m] one can say that the commercial configuration of the WindCube v2.1 is able to measure accurately the large horizontal turbulent structures of interest for wind energy applications.

- p.11, l.247: does N_i and n_i have to be determined for each beam individually? How much do they differ and why?

  Reply

  Yes, N_i and n_i needs to determined for each beam individually. In ocean science, it has been shown with ADCP measurements that the Doppler noise increases with increasing flow speed [3, 4]. For lidar measurement, it is the same. We demonstrated this (Fig. 5 in the revised version). 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).

  It is difficult to tell why the the Doppler noise is not constant and instead, depend on the flow speed. The Doppler noise is generated by random scatterer motions within the sample volumes which results to errors in measuring the frequency change or phase shift of the reflected pulses. We would say that at higher flow speed the scatter motions are more "chaotic" which might impact the error in measuring the frequency change.

- p.11, l.248: this seems to be a random choice for the characteristic frequency. Can this be justified by error quantification?

  Reply

  In the revised version of the manuscript (section 2.3), the cutting frequency is now determined by a method involving an error minimization of the least-square regression of the LOS velocity spectra.

- p.11, l.252: I assume you mean Eq. 5.

  Reply

  Yes. It has been corrected.

- p.17, l.363: what does "a frequency domain 9 times wider" mean?

  Reply

  We removed this sentence.

- p.18, ll.370ff: These explanations and theories would be much easier to follow and understand if the Doppler noise was presented in a concise mathematical formulation before.

  Reply

  We added a section dedicated to Doppler noise (section 2.3) to help the reader following the explanations.

- p.19, l. 411: Reducing the beam spread needs to be carefully traded off against horizontal wind speed retrieval accuracy.

  Reply

  You are right. 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. Lines 319 - 321.

References

[1] W. L. Eberhard, R. E. Cupp, and K. R. Healy, "Doppler lidar measurement of profiles of turbulence and momentum flux," *Journal of Atmospheric and Oceanic Technology*, vol. 6, no. 5, pp. 809–819, 1989.

[2] D. L. Hartmann, *Global physical climatology*, vol. 103. Newnes, 2015.

[3] J. Thomson, B. Polagye, V. Durgesh, and M. C. Richmond, "Measurements of turbulence at two tidal energy sites in Puget Sound, WA," *Oceanic Engineering, IEEE Journal of Oceanic Engineering*, vol. 37, no. 3, pp. 363–374, 2012.

[4] M. Thiébaut, J.-F. Filipot, C. Maisondieu, G. Damblans, R. Duarte, E. Droniou, N. Chaplain, and S. Guillou, "A comprehensive assessment of turbulence at a tidal-stream energy site influenced by wind-generated ocean waves," *Energy*, vol. 191, p. 116550, 2020.