Deriving atmospheric turbulence intensity from profiling pulsed lidar measurements

Response on reviewer's comments - RC3

October 21, 2022

Thank you to the three reviewers (RC1/RC2/RC3) 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 Major comments

Thanks a lot for your manuscript. I have several mayor comments as well as minor comments that I think are mostly related to misconceptions with regards to the nature of measuring turbulence with lidars and lack of clarity.

Point 1

In the abstract, as an example, the authors say that a new method is proposed to estimate TI from pulsed profilers. First, the method the authors used was proposed by Dewey and Stringer [2]. Second, the use of beam variances have also been used before to estimate velocity-component variances at the least by both profilers and nacelle-based lidars [3, 4]. So the novelty should be clarified.

Reply
The novelty has been clarified. In the revised version, the abstract starts with "A method developed in ocean science and based on acoustic Doppler current profiler (ADCP) measurements is implemented to provide estimates of the atmospheric turbulence intensity (TI) derived from measurements of pulsed lidars employing the Doppler beam swinging technique". Lines 1-3.

Point 2

It could be the writing style but I am not sure whether the authors understand the problem of measuring turbulence with a lidar. For example, the cross-contamination is not due to the different structures of the field (line 40) but due to the influence of different velocity components on the line-of-sight variance, which is a result of the lidars scanning strategy. Also, in line 45 they say that cross-contamination causes and overestimation of TI. That is not always true; it might happen but that depends on how much filtering due to probe volume averaging you have. So, in many cases, if not most cases, filtering is the biggest threat in lidars.

Reply
We rewrote this part of the paper by mentioning that there are two systematic errors when measuring turbulence with a lidar: "In comparison to TI derived from measurement of a reference instrument such as a sonic anemometer, TI derived from lidar measurements is biased by two main systematic errors, i.e., underestimation due to the probe-volume averaging, and overestimation due to the cross-contamination effect causes by the influence of different velocity components on the line-of-sight (LOS) variance, which is a result of the lidars scanning strategy". Lines 38-41. Also, in the discussion we mentioned that both sources of error do not cancel each other.

Point 3

Also important is that by acquiring the velocity faster the change in the variance should not be high and so neither in the TI. You will increase the uncertainty on the variance by measuring slower but the bias should not change unless you definitively and systematically are missing fluctuations by the turbulence structures but this is not the case of this unit measuring this non-complex flow.

Reply
You're right. The increase of the sampling rate does not bring any significant improvement in TI estimates. Although the increase of the sampling rate has brought TI estimates slightly closer to that given by the reference measurements, this might be the result of a reduced statistical uncertainty generated by a higher number of measurement points within the 10-min subsets (Kaimal and Finnigan, 1994) rather than the ability of the prototype configuration to capture the variance associated with smaller eddies. These eddies might still be filtered out due to the probe-volume averaging.

Point 4

Instrument noise corrections have been explored before (line 53) (e.g., [5]). Perhaps you could explore that method as in that work they study a pulsed lidar too. This would avoid using a threshold to establish the frequency at which you expect noise, which is what I think you are doing. Your method does seem very sensitive to this choice and you should show how sensitive or not indeed is.

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.

Point 5

You have defined TI to be the parameter you want to look for. However, you should also present mean and variance comparisons of the velocity components as the problems with turbulence should be clearer seen when computing the variance and you want to make sure both lidars measure the same mean wind as your sonic. Also in wind energy the TI is normally defined based on the along-wind component or horizontal wind. If you have your fixed lidar beams then the horizontal velocity variance σ_S^2 is not $\left(\sigma_x^2+\sigma_y^2\right)/2$ as you imply but $(V_x^2\sigma_x^2+V_y^2\sigma_y^2)/S^2$ where S is the horizontal velocity magnitude in the case the covariance between the two horizontal components is assumed zero (which is probably ok in your case). So I wonder why you choose to define TI like this. Perhaps you can make a comparison of sonic variance as you imply (half of the sum of both variances) against deriving the variance from the horizontal velocity time series from the sonic.

Reply
The definition of the two-dimensional (2D) TI was found in the literature (e.g., [6, 7]) where the 2D TI is given by:

$$\mathrm{TI}_{\mathrm{2D}} = 100 \times \sqrt{\frac{\frac{1}{2}\left(\sigma_x^2+\sigma_y^2\right)}{\overline{V_x}^2+\overline{V_y}^2}} \tag{1}$$

A comparison of $\sigma_S^2 = \left(\sigma_x^2+\sigma_y^2\right)/2$ and $\sigma_S^2 = (V_x^2\sigma_x^2+V_y^2\sigma_y^2)/S^2$ is shown in Fig. 1. Both time series are almost superimposed.

Point 6

My most important comment is the use of the method by Dewey and Stringer [2]. I cannot find what values of pitch and roll the authors use in Eqs. (8)–(10). If they use close to zero values (as I guess

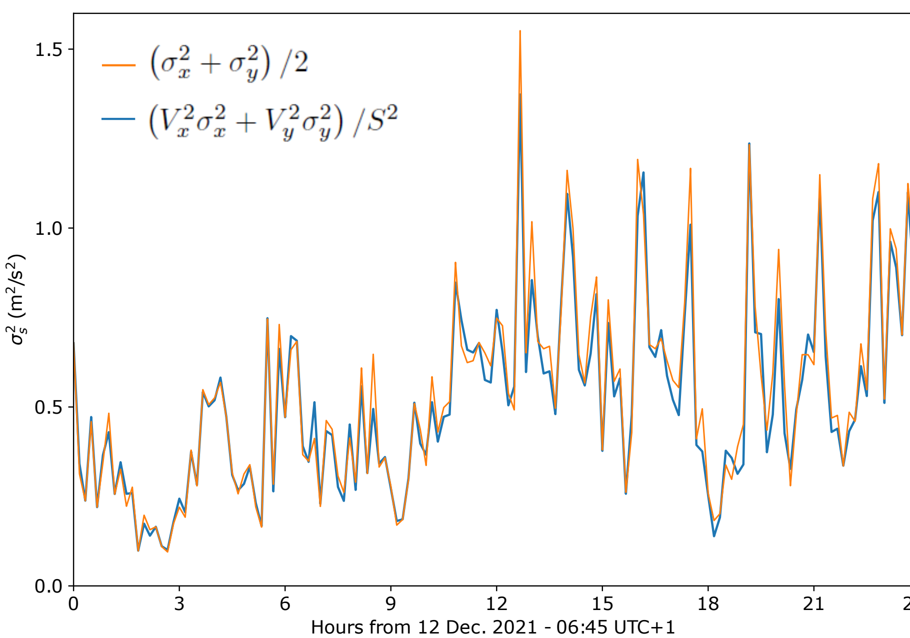

Figure 1: Comparison of $\sigma_S^2 = \left(\sigma_x^2+\sigma_y^2\right)/2$ and $\sigma_S^2 = (V_x^2\sigma_x^2+V_y^2\sigma_y^2)/S^2$.

they place the lidar relatively well), then those equations are not needed but the classical Eberhard expressions [1] for the beam velocity variance, which one can easily show lead to:

$$\langle u'u' \rangle = \left(\sigma_3^2 + \sigma_1^2 - 2\sigma_5 \cos^2\phi\right) / \left(2 \sin^2\phi\right) \tag{2}$$

$$\langle v'v' \rangle = \left(\sigma_2^2 + \sigma_4^2 - 2\sigma_5 \cos^2\phi\right) / \left(2 \sin^2\phi\right) \tag{3}$$

For $\langle w'w' \rangle$ you do not need an expression as you measure with a vertical beam (so you can see that when ϕ_1 and ϕ_2 are close to zero in Eq. (10), the variance of w and that of beam 5 are the same).

Reply
Your comment is in line with comments of the two other reviewers. 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 7

Section 3.1. makes little sense to me. First in Fig. 3 you seem to be plotting the beam power spectra and not the TKE spectra as you state in the text. Second, and most important, you are here analyzing the slope of the spectra within the frequency range where the probe volume filtering is more visible and important! So unless you know how much filtering you have due to the probe volume averaging and how this affect the spectra, you cannot just compare the slopes of the measured lidar spectra with classical turbulence slopes like the $-5/3$ one.

Point 8

Section 3.3 also does not make sense to me (title should not be stress but variance by the way). Unless I just missed the values of pitch and roll different from zero, the results in Fig. 5 should be perfect (zero bias and R = 1 when plotting the vertical variance and beam 5 variance as Eq. (10) shows). So why are they not perfect? It just seems very strange to me.

Reply for both points 7 and 8
Sections 3.1 and 3.3 have been removed. Yourself and the two others reviewers have considered these two sections as out of the scope of the paper and/or irrelevant.

2 Minor comments

- Line 2 and other instances in the paper: you can remove "(light detection and ranging)". Lidars as radars and sodars are already "works" and do not need explanation.

  Reply
  'light detection and ranging" has been removed.

- Line 5 Replace "compared to that derived" to "compare to those derived".

  Reply
  Done. Thank you.

- Line 27 and other instances in the paper: replace "altitude" by "height".

  Reply
  Done for each instance.

- Line 32 Replace "This system" by "lidars".

  Reply
  Done

- Line 40 the explanation of the cross-contamination is not true. This is due to the influence of the different velocity components, which result from the way a lidar normally scans.

  Reply
  Thank you. We corrected the definition: "In comparison to TI derived from measurement of a reference instrument such as a sonic anemometer, TI derived from lidar measurements is biased by two main systematic errors, i.e., underestimation due to the probe-volume averaging, and overestimation due to the cross-contamination effect causes by the influence of different velocity components on the line-of-sight (LOS) variance, which is a result of the lidars scanning strategy". Lines 38-41.

- Line 42 I think you do not mean TKE spectra, but velocity component spectra.

  Reply
  Exactly. It has been corrected.

- Line 59 Add "the" before instrument.

  Reply
  This part has been deleted.

- Line 79 Add "the" before Doppler.

  Reply
  This part has been deleted.

- Line 82 and maybe other instances "40 meters to 300 meters" should read "40 to 300 m".

  Reply
  This part has been deleted. Changed for other instances.

- Line 94 sometimes you say "beam" and sometimes "Beam", be consistent and check all instances.

  Reply
  We checked all instances and stick to "beam".

- Equation (6) appears alone in the text.

  Reply
  This equation is useless and has been removed.

- Equations (8)–(10) should appear after the "are given" in line 32.

  Reply
  This part has been deleted.

- Line 139 Two "involves" should be "involved".

  Reply
  Thank you. It has been corrected.

- Line 145 delete the s in measurements in this and other instances.

  Reply
  Done for each instance.

- Line 146 Flat terrain does not mean you will get good atmospheric conditions. Maybe you mean orography undisturbed flow or similar.

  Reply
  Thank you for the proposition, we modified the text following your suggestion.

- Figure 3 y-axis is not TKE but beam power spectra density or similar.

  Reply
  It was a mistake, corrected in Fig. 3 and Fig. 4 of the revised version of the manuscript.

- I am not sure the right word is “distributions” for what you show in Fig. 6. In line 277 you say TI distributions are not governed by wind speed. Well they should if you did plot the TI as function of wind speed.

  Reply
  ”distributions” might not be appropriate. We changed it by ”estimates”. In line 277, our idea was poorly expressed. It is, indeed, well known that TI depends on wind speed...

- Line 323 not true (see main comment 1).

  Reply
  See the answer to comment 1 (Point 1)

- Line 335 vertical resolution and probe length are not the same!

  Reply
  It is not indeed. Corrected.

- Line 378 and around. A large probe lenght increases largely the filtering, which is perhaps the biggest threat of lidars.

  Reply
  After the present study it is still difficult to tell either the volume-averaging or cross-contamination is the biggest threat.

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] R. Dewey and S. Stringer, "Reynolds stresses and turbulent kinetic energy estimates from various ADCP beam configurations: Theory," *J. of Phys. Ocean*, pp. 1–35, 2007.

[3] A. Sathe, J. Mann, N. Vasiljevic, and G. Lea, "A six-beam method to measure turbulence statistics using ground-based wind lidars," *Atmospheric Measurement Techniques*, vol. 8, no. 2, pp. 729–740, 2015. Publisher: Copernicus GmbH.

[4] W. Fu, A. Peña, and J. Mann, "Turbulence statistics from three different nacelle lidars," *Wind Energy Science Discussions*, pp. 1–29, 2021. Publisher: Copernicus GmbH.

[5] A. Peña, J. Mann, and N. Dimitrov, "Turbulence characterization from a forward-looking nacelle lidar," *Wind Energy Science*, vol. 2, no. 1, pp. 133–152, 2017. Publisher: Copernicus GmbH.

[6] P. Mycek, B. Gaurier, G. Germain, G. Pinon, and E. Rivoalen, "Experimental study of the turbulence intensity effects on marine current turbines behaviour. Part I: One single turbine," *Renewable Energy*, vol. 66, pp. 729–746, 2014.

[7] M. Allmark, R. Martinez, S. Ordonez-Sanchez, C. Lloyd, T. O'doherty, G. Germain, B. Gaurier, and C. Johnstone, "A phenomenological study of lab-scale tidal turbine loading under combined irregular wave and shear flow conditions," *Journal of Marine Science and Engineering*, vol. 9, no. 6, p. 593, 2021. Publisher: MDPI.