Review of “Deriving atmospheric TI from profiling pulsed lidar measurements”

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.

Major comments

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 [2007]. 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 [Sathe et al., 2014, Fu et al., 2022]. So the novelty should be clarified.

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.

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.

4. Instrument noise corrections have been explored before (line 53) [e.g., Peña et al., 2017]. 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.

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 $\left(V_x^2\sigma_x^2+V_y^2\sigma_y^2\right)/S^2$ 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.

6. My most important comment is the use of the method by Dewey and Stringer [2007]. 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 they place the lidar relatively well), then those equations are not needed but the classical Eberhard et al. [1989] expressions for the beam velocity variance, which one can easily show lead to:

$$\langle u'u'\rangle = \left(\sigma_3^2+\sigma_1^2-2\sigma_5\cos^2\theta\right)/\left(2\sin^2\theta\right), \quad (1)$$

$$\langle v'v'\rangle = \left(\sigma_2^2+\sigma_4^2-2\sigma_5\cos^2\theta\right)/\left(2\sin^2\theta\right) \quad (2)$$

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).

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.

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.

Minor comments

1. Line 2 and other instances in the paper: you can remove "(light detection and ranging)". Lidars as radars and sodars are already "workds" and do not need explanation
2. Line 5 Replace "compared to that derived" to "compare to those derived"
3. Line 27 and other instances in the paper: replace "altitude" by "height"
4. Line 32 Replace "This system" by "lidars"
5. 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
6. Line 42 I think you do not mean TKE spectra, but velocity component spectra
7. Line 59 Add "the" before instrument
8. Line 79 Add "the" before Doppler
9. Line 82 and maybe other instances "40 meters to 300 meters" should read "40 to 300 m"
10. Line 94 sometimes you say "beam" and sometimes "Beam", be consistent and check all instances
11. Equation (6) appears alone in the text
12. Equations (8)–(10) should appear after the "are given" in line 32
13. Line 139 Two "involves" should be "involved"
14. Line 145 delete the s in measurements in this and other instances
15. Line 146 Flat terrain does not mean you will get good atmospheric conditions. Maybe you mean orography undisturbed flow or similar
16. Figure 3 y-axis is not TKE but beam power spectra density or similar
17. 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
18. Line 323 not true (see main comment 1)
19. Line 335 vertical resolution and probe length are not the same!
20. Line 378 and around. A large probe lenght increases largely the filtering, which is perhaps the biggest threat of lidars

References

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