Review for manuscript wes-2022-53

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Thiebaut 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.

1 General comments

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&12 from Dewey and Stringer against the sonic anemometer data:

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

$$\overline{b_3'^2} + \overline{b_4'^2} = 2\overline{v'^2}\sin^2\theta + 2\overline{w'^2}\cos^2\theta \quad . \tag{2}$$

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.

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.

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.

2 Specific comments

- p.2,l.40: what about availability?
- 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.
- p.4,l.83: "volume of atmosphere"? This is a strange expression.
- Fig.2: basic information of the maps are missing: scale, northing, lat/lon, copyright of the map pictures.
- 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.
- 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.
- 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.
- 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.

- 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.
- 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?
- p.11, l.248: this seems to be a random choice for the characteristic frequency. Can this be justified by error quantification?
- p.11, l.252: I assume you mean Eq. 5.
- p.17, l.363: what does "a frequency domain 9 times wider" mean?
- 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.
- p.19, l. 411: Reducing the beam spread needs to be carefully traded off against horizontal wind speed retrieval accuracy.