Review of Thiébaut and Luxcey "Dual-lidar profilers for measuring atmospheric turbulence" submitted to Wind Energy Science

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1 General comment

This paper investigates the line-of-sight variance method to extract turbulence from profiling lidars. The authors cleverly use two standard five-beam pulsed Doppler lidars rotated 45° relative to each other to essentially for a nine-beam lidars. This allows of obtaining the variances of the horizontal velocities without combining line-of-sight velocities instantaneously from different beams, similarly to how it is done in Sathe, Mann, et al. (2015).

The method is tested with relevant offshore data where the turbulence estimations from the lidars are compared to sonic anemometer "ground truth". The results are promising, but the impact of measurement volume averaging is still not successfully addressed.

In general, the paper is publishable, but there are a number of comments that has to be addressed, and the paper is at places too long and textbook-like.

2 Specific comments

l 65 – 69 The considerations here are not entirely correct. If you measure with an instrument with point-like measurement volume and high time-resolution, you will get the real turbulence variance. If you pick only one sample every second, you still get the correct turbulence variance, so you resolve all scales. Similarly, for the six-beam WindScanner setup it is not he 15 s that is important. It is the sample volume and the averaging time of the individual beam that determined what scales contribute to the variance. Had the sampling volume and the averaging time been small, then the variance would have been unbiased, even if the cycle were completed in 15 s.

l 93 *Aldernay Race* sounds like a ship race, but it is a geographic location. Maybe you could help the reader realize that.

l 140 This comment relates to the first. There is no reason to downsample the cup anemometer for a consistent comparison. I think what is most relevant to compare with the the variance of the full cup anemometer signal. It is also unclear what the low-pass filter is doing in this comparison. Please specify the low-pass exactly, and why it is applied.

l 149 The choice of 30-min versus 10-min averages is very important. I think it is very reasonable, but it is not in the DNV error metrics. This point deserves some more emphasis.

l 162 Aerosol fall speed? Do you mean rain?

l 176 A similar technique was appied by Mayor et al. (1997). Cite, if you see fit.

sec 2.6.1 This section is too long and text book like. Please short considerable. Why is $\mathrm{TI}_u = \sqrt{\sigma_u^2}/U$ and not just $\mathrm{TI}_u = \sigma_u/U$? Same for Eqs 25+26.

Eqs 16 + 17 Is this really how the variances are calculated? First you calculate the spectrum and then integrate over frequencies.

Eq 18 + 19 There is something wrong with the notation (I don't think anything is basically wrong with the math). In 18, a 10 by 3 matrix is multiplied by a 3 by 3 matrix. This gives a 10 by 3 matrix. That is added to a 10 by 3 matrix, giving again a 10 by 3 matrix. It is unclear how that turnrs into a 10 by 6 matrix in 19. In 19 the LHS is a matrix, but you refer to the elements (e.g $Q_{q,m}$?). Are you summing over repeated indices? Please clean up the notation for easier reading.

l 319 The abbreviations MRSE and RRMSE were defined in the abstract. Maybe it would be helpful to do it again here.

l 345 Why not simply force the fits through zero? (late, in l 371 you actually state that, which I think is good)

l 346 I disagree that a percentage facilitates interpretation. Could be omitted.

Figure 4 I'm not sure this figure is necessary. It could be omitted to reduce the length of an already too long paper.

Tab 2, Fig 5 I think it is very difficult to compare experiments that have been performed in different climates, different instruments, and different beam geometry.

Fig 6 Excellent, but I insist that you plot premultiplied spectra $f \cdot S(f)$ when you have a log frequency axis. Explain how the spectra are average. Just plain averaging or averages of S/U^2 or S/σ^2? The latter has the advantage that one very strong wind case does not dominate everything.

l 358 It is surprising that the velocity spectra derived with the variance method do not match at low frequencies, while the traditional indeed do match. Can this be explained? In Sathe and Mann (2012) you see good match at low frequencies for the traditional method for the u- and w-components, but not for the v-component. It is also a bit strange that the overestimation of the traditional along-wind spectra differ at high frequencies with stability. Can that be explained?

Figure 7 Not because I want it in the paper, but you do similar analysis of TI_w?

Figure 8 Please do not show $|MRBE|$. A bias should be only shown with its sign.

Discussion A very good discussion i, in general!

l 410 In Sathe, Mann, et al. (2015) it is stated that the WindScanner system can have either 400 or 200 ns pulses. Although not entirely clear from the paper, the 200 ns pulse was used. That corresponds to a FWHM probe volume of approximately 30 m, not 100 m, as stated in the text.

l 417 Relating to previous comments, the sample rate in it self should not bias the variance. Only the probe volume and the accumulation time should have an impact.

Eq 36 It is unnecessary to include the σ_r^2 term in the equation. It is completely negligible for a lidar, and only confuses the reader.

l 422 σ_l and σ_r are not weighting factors, but length scales.

l 436 Again, it should be the averaging, not the sample rate that is important.

≈ l 411 It think it is worth mentioning Manami et al. (2025) in the discussion. In this paper we try to annihilate the probe volume effect of at pulsed lidar.

l 443 – 452 Again, if the ZXLidar is taking 50 ms acculation time, then that, together with the probe volume, is what is important. Spending only a fraction of the time at one height does not introduce a bias in the variance. That is actually also discussed in Lenschow, Mann, and Kristensen (1994).

l 453 – 457 I don't understand this discussion. Under stable conditions, the standard knowledge is that the length scale is *smaller* for stable conditions.

l 465 How can systematic (that is stationary) spatial gradients introduce more variance in the traditional method? I would think that it only introduces a bias in the mean.

473 – 476 Interesting that the error is significantly larger for the cross-wind component, but can you explain why? I cannot really follow the logic in the explanation. Maybe there is a hint in Sathe and Mann (2012).

l 507 "identified the problematic". I guess you mean "problem", or "research issue".

References

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