Review of

"An Error Reduction Algorithm to Inprove Lidar Turbulence Estimates for Wind Energy"

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Submitted to Wind Energy Science- October 2016

General comments:

The paper present the L-TERRA algorithm developed for correcting ground based profiler lidar measured turbulence intensity. Correcting here implies retrieving the same TI as that measured by a sonic anemometer. The approach is interesting since there is an incontestable need for a "practical" way to use TI measured by Doppler lidars.

The paper has been very much improved since the first submission (version from July 2016). It is now very focused and comprehensive. The comments from the first review have been well taken into account.

The analysis has been further detailed in several aspects:

1. Intermediate results from the L-TERRA-S algorithm showed a better agreement on average of lidar TI with sonic/cup TI with the physic based correction (correction for noise, spatial averaging and variance contamination), before applying a machine learning process. The regression slope was closer to one but the scatter was slightly increased.
2. The process was tested on subsets of the datasets corresponding to different atmospheric stability classes and showed that different combinations of corrections were required for different stability classes (stable conditions require extrapolation of the power spectrum to compensate for the volume averaging effect, unstable conditions require correction for the cross contamination). Those results are coherent with previous studies on turbulence measured by Doppler lidars.
3. Two different machine learning process were tested out and it was shown that it did not improve the results obtained with the physics-based corrections alone.

The paper is well written with a good introduction, a comprehensive literature review, clear results and clear conclusions. The abstract is also very comprehensive.

Below are minor detailed comments.

Detailed comments:

P2, l16 to 22 and Figure 1: This is absolutely correct but this could be made shorter as it is done for resource assessment. The effect of turbulence on power curve is fairly well known, it is therefore not necessary to discuss and illustrate it here. A statement that turbulence intensity measurement is necessary for power performance measurement with some relevant references (e.g. Malcolm and Hansen, 2006) would be enough.

P7, l17:

1. could you give more details on the "constant temporal spacing" used here? Is it 1s and 4s? Have the sonic and cup data been down sampled to have identical frequency?
2. Has any data filtering been applied e.g. on SNR?

P8, section 3.2:

Would it be possible to have a brief explanation of the spike filter and the Lenschow methods mentioned here so that a reader who does not know about them can get an idea of what it is about without having to read those papers?

P8, l18: the word "evaluated" is confusing here as you have actually only applied one of them (the second one: modeling and extrapolation of the spectrum). Please try to make it clear throughout sections 3 and 5 that you have only evaluated methods applied to u,v,w (left side of the flow chart in Fig 2) and not those applied to Vrad). I suggest to make it is clear here that you are describing all methods but have not tested them all. Suggestion for re-wording: "Two methods can be used to mitigate…".

P10 and l12: could you provide some information about the flatness/complexity of the site (e.g. max elevation difference within a couple of km around the WC)

P10, l11 and l16: was the WC configured to measure at the same heights as the cups/sonics used as reference? E.g. 60m at ARM site

P10, l19: would it be possible to indicate how far from the turbines the WC was located (any effect from induction within conical scan)?

P10, l20: could you also provide some indication of the comparison of the mean wind speed between the WC and the reference instrument? If the mean wind speed comparison is not nearly "perfect" (1:1 slope and very low scatter) there is little chance that the TI compares well.

P11, l9-15: a more quantitative description of the results would be relevant:

1. What was the MAE range (maximum) overall tested combinations? Has the MAE been increased in some cases compared to the MAE without correction? This would show that wrong choices of correction or wrong combination would be worse than no correction at all.
2. What do you mean by "similar MAE values" (line 10)? Could you give a range?
3. How many other methods were in that range?

P12, l3: interesting results with the different stability classes. Could the scatter be due to the proxy used to define the classes? I mean could the scatter be due to data for which the stability class is not what is approximated by the shear exponent and in those cases the correction applied is not optimal?

I think it would be very relevant to discuss this a bit further in the paper as in the end it is mitigating a bit the "practical" approach of the method since, from the results presented now, it seems that to apply an effective correction to the lidar TI, we first need to identify the stability class for each 10 minute data. Quantifying the stability class from solely lidar information is a challenge.

Table 3:

1. Could you please indicate the number of data for each data set or subset?
2. Indicate that the results are for the ARM site in the caption

Table 5:

The wind speed column is not clear. It can be confused with the “Wind speed” decision box of the flow chart in Fig 2. I suggest to indicate “1 sec” and “4 sec” or “1Hz” and “0.25Hz2 instead of “raw” and “VAD”.

Figure 2:

1. What do you mean by “raw WC data” in top box? Unfiltered data? Radial wind speeds?
2. The option for not appliying a correction (e.g. for volume averaging) does not appear clearly as an option. Perhaps it is represented by the direct arrow between “Volume averaging?” and “Variance contamination?” but then there should also be arrows between “Spectral fit 1/2” and “Variance contamination?” to be consistent. And that shoud be consistent through the whole flow chart.