Repsonses to Reviewer 1's Comments

(the author's responses are given in **RED** text)

Manuscript Number: AMT 2016-312

Title: Validating Precision Estimates in Horizontal Wind Measurements from a Doppler

Lidar

Lead Author: Rob K. Newsom

Summary

In this paper, different methods are used to estimate precision in Doppler wind measurements from a scanning lidar deployed during XPIA. These precision estimates are compared to the actual horizontal wind speed and wind direction differences between the lidar and sonic anemometers on a tower to assess the ability of the various precision estimates to characterize lidar error. These types of precision estimates are extremely useful in the wind energy community, as they enable the calculation of uncertainty (if the lidar is unbiased) without measurements from a reference instrument.

Overall, the paper is clear and well-written. The clarification between uncertainty and precision is very helpful, although this clarification is sprinkled throughout the manuscript. It would be more helpful to state upfront (i.e., in the introduction section) what factors contribute to the random and systematic errors in Doppler lidar velocity data and how these terms relate to the definitions of uncertainty and precision.

The use of the terms uncertainty and precision is still a bit unclear in some parts of the manuscript. For example, in Section 3, sri is described as the measurement uncertainty due to random errors (p. 6, Line 2). But isn't uncertainty due to random errors just equivalent to precision? And in Section 4.1, you use the term uncertainty through p. 12, state that you can equate uncertainty with precision in this case because the CDL data are unbiased (p. 12, Lines 11-13), then continue to use the term uncertainty through the rest of Section 4.1. A clarification between these terms, perhaps with some additional symbols or equations, would be extremely useful. These terms are currently not very well-defined in the lidar literature, so laying out clear definitions of precision and uncertainty in the context of lidar measurements would make this paper a valuable reference.

In the original manuscript we used the terms like "random uncertainty" or "uncertainty due to random errors" interchangeably with "precision." In the revised manuscript we have attempted to clarify that random uncertainty = precision in the introduction. We have also replaced "uncertainty" with "precision" where appropriate throughout the entire revised manuscript.

Specific comments on the manuscript are listed below.

Specific Comments

Abstract

p. 1, Line 18: Should uncertainty be changed to precision here?

We have changed “uncertainty” to “precision” here and in many other places throughout the revised manuscript.

p.1, Lines 24-25: Briefly describe why ignoring turbulence effects results in uncertainty being equivalent to precision (i.e., how is turbulence defined in the context of random vs. systematic errors?)

The idea we’re trying express is described by equation (11), i.e. the square of the total uncertainty is the sum of the real atmospheric variance and the instrumental noise variance. If we *assume* that the atmospheric variance is zero, then the radial velocity uncertainty is equal to the instrumental precision, by equation (11). What we’re trying to say here is that if you make this assumption you get really bad estimates for the wind speed and direction uncertainties. Since this is the abstract we have to keep things as concise as possible. So we have modified the last sentence of the abstract to read

“By contrast, when instrumental measurement precision is assumed to be the only contribution the radial velocity uncertainty, the retrievals resulted in wind speed and direction precisions were biased far too low and poor indicators of data quality.”

1. Introduction

p. 3, Line 7: change “step-stair” to “step-stare”

Done

2. Experimental Setup and Instrumentation

p. 5, Lines 12-18: I assume wind speeds from both sonics at each height were used to make these figures. How did you determine which sonic to use during each 10-minute period?

In the revised paper we have added the following text to p5 lines 6-8:

“For each 10-min averaging interval we used the sonic anemometer on the upwind side of the tower, as tower wake effects were observed to be quite significant (McCaffrey, et. al. 2016).”

3. Lidar Wind Retrieval and Precision Estimation

p. 7, Line 5: What is N in these equations?

N is the number of beams (LOSs) in the PPI scan that are used to retrieve the wind profile. This should have been mentioned in the text after equation 1 – sorry about that. We have since added a brief description after equation 1 in the revised manuscript.

p. 8, Lines 14-18: It looks like there's an SNR maximum at approximately 300 m. Does this correspond to the focus height of the lidar?

No not necessarily. In this case the focus was set at infinity.

4. Results

p. 8, Lines 21-23: Make it clear from the beginning of the section what you are calculating the uncertainty of (10-min. wind speeds? Wind speeds from each 40-second VAD scan?)

We have modified the first paragraph in section 4.1 to read "Three trials were conducted to evaluate different methods for estimating the radial velocity uncertainty and the resultant uncertainty in the derived wind components. For all of these trials, winds were computed from the 40-s PPI scans and the wind retrieval algorithm was configured to retrieve all three components of the wind field and use all eight beams of the PPI scans. These trials are described in Table 1."

p. 8, Line 25: Change "the all the" to "all the"

Done

p. 9, Line 2: How far apart in time were the consecutive scans conducted? If they were spaced 10-15 minutes apart in time, how well do these variance measurements actually characterize atmospheric turbulence?

As stated previously in section 2.1 "…the instrument was operated using a fixed scan schedule consisting of PPI scans once every 12 minutes." However, it doesn't hurt to remind the reader here, so we have added the following sentence to page 9 lines 4-5 in the revised manuscript:

"In the present case, the PPI scans are spaced 12 minutes apart, so we assume the atmosphere to be statistically stationary over a 24-minute period."

p. 9, Lines 15-16: This line (or a sentence with similar content) should be placed before the trials are described, so the reader is aware from the beginning that "trials" refer to different ways of processing the same data.

We have deleted the two sentences that appeared on p9 lines 15-16 of the original manuscript. Instead, we have added the following sentences to the end of the first paragraph of section 4.1 in the revised manuscript:

"All three trials use the same PPI scan data. The only difference between the trials is in the treatment of the radial velocity precision, σ_r ."

p. 9, Line 20: The use of the term “larger precisions” is misleading here. I would suggest changing it to something like “lower precision (higher uncertainty)”.

In this paper we have defined the precision to be the uncertainty due to random (zero-mean) error. So using this definition “high precision” means “high random uncertainty,” which I admit sounds strange. To avoid this conundrum we have changed “larger precisions” to “larger random uncertainties.”

p. 9, Lines 22-23: Briefly discuss why the uncertainty in Trial 3 shows no distinct diurnal variation.

We have added a sentence to the end of this paragraph (p9 lines 17-20 of the revised manuscript) to address the reviewers comment. The sentence reads:

“The reason for this difference is that the radial velocity uncertainties for Trial 3 were determined solely from the SNR (see Fig 4a). The SNR responds to variations in aerosol properties (including size distribution and number density) and is less affected than the radial velocity by variations in atmospheric stability.”

p. 10, Lines 4-5: Briefly discuss why the averaging time for the sonics was set to twice the time of the PPI scans.

During the course of our study we tested several averaging intervals ranging from 1x to 4x the PPI scan time. The results were not affected significantly. Although we were close to the tower (~140m), we weren’t strictly collocated. Had we been collocated we would have set the averaging interval to the match the PPI scan time. Given that we were 140m from the tower we opted to go with an averaging interval that was 2x the PPI scan time.

p. 10, Lines 5-6: It is a bit unclear what you mean by “under-sampling” here, and how this relates to the 12 min. PPI intervals and the 40-s scan time.

The sonic data averaging interval is about 80 sec. The center times of the sonic averaging intervals are the same as the center times of the PPI scans, which are performed once every 12 min, or 720 sec. By “under-sampling” we mean that there is no overlap between the averaging intervals. If there was any overlap we would call it “oversampling.” To make this point clear we have added the following text to p9 line 26 of the revised manuscript:

“(i.e. no overlap between averaging intervals)”

p. 10, Line 9: Change “rather the wind speeds…” to “rather than the wind speeds…”

Done

p. 10, Lines 10-11: How did you define the wake sectors for the tower?

For each level on the tower and for each averaging interval we used data from the anemometer that was on the upwind side of the tower. We did this by computing the mean wind direction from the two anemometers (using vector averaging). The “upwind” sector was then defined to be the sector that was within +/-90 deg of the mean wind direction. The anemometer that was within that sector was used in our comparisons. To include this information in the revised manuscript we rearranged this paragraph to read (p10 lines 1-6):

"To avoid tower wake effects, only those anemometers on the upwind side of the tower were used in the comparison with the CDL measurements. For each tower level and averaging interval, the upwind side was defined by the azimuth sector that was within ±90^{o} of the mean wind direction, as determined froma vector average of the two sonic anemometers. The temporally averaged (upwind) sonic anemometer data were then interpolated to the heights of the lidar range gates closest to the sonic anemometer heights (these heights were 142.9, 194.9, 246.8 and 298.8 m). The interpolation was handled by interpolating the horizontal vector components, rather the wind speeds and direction."

p. 11, Line 15: Delete extra period after "sonic winds"

Done.

p. 12, Lines 8-14: The significance of this statistical test is a bit unclear, so it would help to elaborate on the meaning of the test.

After taking a second look at our statistical test we've come to conclusion that it doesn't actually prove anything about the statistical significance of the difference in the wind speed means. Instead, we have applied the standard "student t-test." Those results indicate that the differences are significant. We have modified the text on p12 lines5-12 as follows:

"Table 2 shows that wind speed biases range from -1 to 7 cm s^{-1}, and wind direction biases tend to cluster near -1^{o}. A student's t-test for paired data (Press et al. 1988) suggests that these biases, albeit small, are statistically significant. Table 2 also shows that the wind speed and wind direction biases are insensitive to the treatment of the radial velocity uncertainty in Equation (1) and are not affected significantly by data rejection. For 0% data rejection all three trials produce similar results. The differences between the trials are more evident when we compare results with and without data rejection. The results for trials 1 and 2 show significant improvement in the wind speed difference standard deviation, regression and correlation as the data rejection rate is increased from 0 to 50%. By contrast, Trial 3 shows no improvement in these quantities, suggesting that the uncertainty estimates for Trial 3 are poor indicators of data quality."

p. 12, Line 22: Please define the relative uncertainty (i.e., how is the uncertainty being normalized?) The term "relative uncertainty threshold" should be changed to "relative precision threshold" to be consistent with the terminology used in Fig. 7.

Done.

p. 13, Lines 17-22: Please discuss some possible reasons for the diurnal variability in wind speed and direction differences.

The wind speed and direction differences generally increase during the daytime due to the higher turbulence levels. In the original manuscript we point out that "The diurnal variation in the wind speed and wind direction differences is roughly correlated with the TKE." To help clarify this point we have modified the text as follows (p13 lines 12-14 of the revised manuscript):

"The observed wind speed and wind direction differences increase as turbulence levels increase. Thus, the diurnal variation in the wind speed and wind direction differences are strongly correlated with the diurnal variation in TKE (Fig 9b)."

p. 14, Lines 5-7: Were there any noticeable differences in wind speed and direction correlations between the lidar and the tower for different heights or mean wind directions?

No, not over the relatively shallow layer (~150m to ~300m) that our analysis is constrained to.

p. 14, Lines 9-14: Please elaborate on the significance of these findings and relate the different sub-trials to current lidar scanning techniques. For example, Trial 2a2D is similar to low-elevation scans conducted by scanning lidars to measure, e.g., turbine wakes (although the scanning angle used for this CDL was quite high). Trial 2b is similar to a DBS scan used by a vertically profiling lidar. Practically speaking, at what point is the trade-off between decrease in scanning time and increase in uncertainty worthwhile? It should also be noted that if only 4 beams are used in the PPI scan rather than 8, the scan time would be much faster and it would be more feasible to conduct multiple contiguous PPI scans and/or spend more time collecting data at each azimuth angle. It's a bit surprising that there isn't a significant change in the mean wind speed difference and wind speed difference standard deviation when the w wind component is neglected. I would expect a noticeable change in uncertainty, as the w component contributes significantly to the radial velocity at this PPI elevation angle.

At the suggestion of the other reviewer we have opted to omit this material from our revised manuscript.

5. Summary

p. 15, Line 12: Add period after "wind retrieval algorithm"

Done

Tables and Figures

Table 1: It would help to add a column or two to this table to summarize the major assumptions made in each trial.

I believe the main differences/assumptions for each Trial are already summarized in the Table. The only differences are in the way that the radial velocity uncertainties are handled and the way the u and v uncertainties are computed.

Figure 3a: Gridlines in the background should be made darker.

Done.

Figure 4: It would help to also give values of SNR in decibels on the x-axis of these plots, as SNR is often given in decibels in the lidar community.

We have the changed the log scale to a dB scale on both plots in Fig 4.