General Comments

"A method to assess the accuracy of sonic anemometer measurements" evaluates turbulence power spectra to estimate biases in sonic anemometer measurements. As energy is transformed from large eddies to the smallest eddies where it is finally dissipated, within a range of 'middle' sized eddies there energy flows from larger scales of turbulence to smaller scales. This middle range of turbulence is called the inertial subrange, and within it the flow of energy is relatively constant with turbulence scale. Because of this, turbulence within the inertial subrange follows predictable laws. In the manuscript these laws are used to evaluate turbulence measurements recorded using different types of sonic anemometers at different sites. This is done in part because a standard for the measure of turbulence is not readily and commonly available.

The manuscript is well written, with appropriate and clear figures, and is generally well composed. The topic is certainly worth investigating, as sonic anemometers are relied upon for measuring eddy covariance fluxes and turbulence, and many studies have cast the accuracy of their measurements into question. The technique proposed is somewhat novel, at least as a method of evaluating sonic anemometer measurements, and as such it may be useful. The technique suffers, by the authors' own admission, of being a relative measure, rather than an absolute one; the ideal ratio of 4/3 between the W and U spectra can be achieved when both W and U are incorrect, just as long as they are incorrect to the same degree. In addition, the method can only be applied to measurements that are recorded well above the surface, in well-developed turbulence, where the inertial subrange is clearly distinguishable. However the manuscript confronts these shortcomings directly, and demonstrates how the technique is still quite useful for evaluating the accuracy of sonic anemometer measurements.

Specific Comments

P. 2, l. 34 – 35. Although an ATI was briefly evaluated in Kochendorfer at al. (2012), Kochendorfer et al. (2012) derived their corrections using three identical R. M. Young anemometers, by changing the orientation of the center anemometer and assuming that the outer two anemometers were capable of accurately measuring the horizontal wind speed when the angle of attack was near-zero. This is the method referred to as "the third variant" used by Nakai and Shimoyama (2012) in the manuscript (l. 22 – 31), and was originally presented by Meyers and Heuer, (2006). Regarding the "busy" setup, turbulent statistics can be compared when all anemometers are oriented vertically to evaluate biases in the wind field (e.g. Kochendorfer et al., 2013).

P. 3, l. 2. Frank et al. (2013) was unique in that the anemometers were re-oriented to check for self-consistency between different measurement axes – their experiment was not similar to the Kochendorfer et al. (2012) experiment, which only used data with zero angle of attack.

P. 3, l. 4. Explain what is mean by "a combination of all three methods".

P. 3, l. 13. This is a semantic, but still significant issue: The manuscript presents a new method for evaluating biases in sonic anemometers, but it is misleading to call it a 'new reference'. For example, if two sonic anemometers differ in their measurements, this method may not necessarily be capable of determining which one is more accurate, as it does not include an independent measurement of the wind speed; it is possible that both anemometers could have a 4/3 slope, and yet still differ from each other. The manuscript would be stronger and more accurate if descriptions of the new method as a 'reference' (e.g. p. 3, l. 14 and l. 16) are reworded.

P. 3, l. 31. I'm confused by this: "all one-point correlations between velocity components become zero". This would imply that the momentum flux (u'w') is zero within the inertial subrange, but that doesn't sound possible. Please explain. Perhaps "become zero" should be reworded as "tend toward zero"?

P. 6, l. 22 and elsewhere. Change “Measurements are collected” to “Measurements were collected”. Events that occurred in the past should be described using the past tense. See https://www.nature.com/scitable/topicpage/effective-writing-13815989 for examples and further explanation. All of the description of the work that was performed should be written in the past tense – this includes the majority of Sections 3, 4, and 5.

P. 7, l. 2. How were the effects of the wind turbines on the spectra evaluated or ruled out? It might be worth including something in the manuscript describing the evaluation of the spectra or distances and wind directions.

Figure 3. It is probably clearer to denote the right and left panels using letters (a and b), rather than right and left. The same can be said for the other paired figure panels.

P. 9, l. 12. Replace “wind conditions” with “wind direction”. And as Figure 4 shows, this statement isn’t strictly true. I get the general idea, but perhaps it should be written more precisely.

P. 12, l. 14. “we limit the range to a close to noise-free wavenumber” is grammatically incorrect – the sentence should probably end with, “a close to noise-free wavenumber range”, but then it becomes even more verbose. Rewrite the entire sentence improve clarity, brevity, and grammar. Here’s a suggestion: “The wavenumber range was limited to exclude noise apparent at higher wavenumbers ($k_1 > 1\ m^{-1}$).”

P. 14, l. 17. Change, “only those spectra, which showed…” to, “only those spectra that showed…”.

P. 14, l. 20 (odd break in the line numbers here, perhaps due to a premature page break or the conversion to pdf). Change “spectra are calculated” to the past tense, “spectra were calculated”.

P. 15, l. 27 – 28. This is presumably only true when the measurements support the existence of a clearly defined inertial subrange. It seems like a bit of a chicken and egg problem– if the inertial subrange isn’t easily identified, is it because the measurements are compromised, or because the turbulence doesn’t follow the textbook?

P. 15, l. 39 (last line of p.15 – another weird brake in the line numbers here). No criticism here, just a note to the authors: Many of us interested in this type of work are hoping that LIDAR measurements will still provide a true wind velocity reference – please keep working on them! Tom Horst told me about this approach long ago, and I’m still waiting to see what comes of it…

References

Kochendorfer, J., Meyers, T. P., Frank, J. M., Massman, W. J., and Heuer, M. W.: Reply to the Comment by Mauder on “How Well Can We Measure the Vertical Wind Speed? Implications for Fluxes of Energy and Mass”, Boundary-Layer Meteorology, 147, 337-345, 2013.

Meyers, T. P. and Heuer, M.: A field methodology to evaluate sonic anemometer angle of attack errors, 27th Conference on Agric For Meteorol, San Diego, California, 2006.