Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2018-293-RC1, 2018

Atmospheric Measurement Techniques
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Discussions

Interactive comment on "A method to assess the accuracy of sonic anemometer measurements" *by* Alfredo Peña et al.

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Received and published: 22 October 2018

In this paper, the authors present a novel methodology to evaluate the relative accuracy of u, v, and w measurements from a sonic anemometer by applying Kolmogorov theory to the relative magnitude of the u, v, and w spectra within the inertial subrange. Based on that theory, the v and w spectra should be 4/3 the magnitude of the u spectra. Using field data from Metek USA-1 and CSAT3 sonic anemometers at different towers, different field sites, without any shadowing correction, with shadowing correction, and with path averaging correction, the 4/3 relationship was tested. For the Metek, while the uncorrected anemometer was much lower than 4/3 for the w-to-u relationship, after applying a wind tunnel based calibration provided by the manufacturer this relationship becomes $\sim$4/3. For the uncorrected CSAT3, the w-to-u relationship is a little close to 4/3 than was the case for the uncorrected Metek, but after applying a shadowing

correction, the ratio is still lower than 4/3, being $\sim$1.2. Thus, the CSAT3 correction could be interpreted as providing only a partial amount of the correction required to achieve a 4/3 relationship.

This is a very novel idea, and has the potential to be fairly influential in the discipline. There are two items that seem very important that should be given more emphasis. First, this technique is among the few that does not require a comparison between one anemometer and another. Rather, it can be applied for any single anemometer at more-or-less any field site (I recognize that this is mentioned in the paper, but this is EXTRMEMELY important, so make sure there is no doubt the reader appreciates how powerful this statement is). Second, because this methodology is entirely based on Kolmogorov theory about the 4/3 ration between w-u and v-u spectra, it should be emphasized that in general all of the results from the v-u tests conform to this theory. While the reader must evaluate results such as Table 2 to determine whether w-u ratios of $\sim$1.0, $\sim$1.1, $\sim$1.2 are evidence of underestimated w measurements, it is crucial to note that the v-u ratios are almost entirely 4/3 for all cases. This gives a lot of credibility that the theory of isotropy is correct, and that the 4/3 standard is reasonable.

One improvement that will be necessary is to better clarify the differences between 4/3, 5/3, and 2/3 slopes in the inertial subrange. I was completely confused upon my first reading, and I had to consult my Kaimal and Finnigan (1994) book to sort this out. While it is clear from Equation 1 that the slope of the inertial subrange is -5/3, figure 1 is a -2/3 slope, which presumable is because it is a "frequency weighted" spectra where the y-axis is actually spectra multiplied by wave number. This is related to second sentence in section 5.1, where the frequency-weighted spectra is multiplied by kˆ(2/3) to give a straight line. This is entirely confusing unless the reader recognizes that it is already multiplied by kˆ(3/3), which is essentially kˆ(5/3), i.e., it makes the -5/3 slope in Equation 1 appear as a flat line. Yet, all of these details are rather unimportant when compared to the 4/3 value in equation 2, which is actually written as 3/4, but constitutes the most important relationship of the paper. This is the assumption from which all of the conclusions of this paper are drawn from. In summary, the reader should not have to consult Kaimal and Finnigan (1994) to sort out the meaning of the different ratios in this paper.

I would recommend that more quantitative techniques be used to ensure that spectra included in the analyses conform to the theoretical -5/3 slope. On suggestion is that rather than only fitting a 0th order polynomial, a better method is to fit a 1st order polynomial with statistical software, then to test for the statistical significance of the 1st order coefficient. If, for example, the p-value for the 1st order (i.e., slope) coefficient is > 0.05, it could be considered non-significant, and then it could essentially be concluded that the slope of the inertial subrange cannot be distinguished from -5/3. Once this is done, using arguments similar to model selection analysis, the 1st order polynomial model can then be reduced to the 0th order model currently described in the paper. In a manner, the goal of this is similar to the "sharpened" criteria in section 5.2/Table 2. The benefit of the statistical criteria is that it provides a defensible justification as to which 10-min periods should be included in the analysis based on their inertial subrange. Another similar idea could be to use a statistical break-point or change-point analysis for each 10-min period to determine the range over which the inertial subrange slope is -5/3 (i.e., use statistics to optimize the range of the inertial subrange for each 10-min period). This seems more complicated to me, but it could also work.

A more quantitative approach than a running mean such as in Figures 4, 5, 7, and 9 or mean/standard deviation such as in Table 2 would improve the interpretation that the results are statistical different/similar to 4/3. One suggestion I have for the figures is to replace the running mean with a local (i.e., LOESS or LOWESS) regression. This is a statistical technique that provides results similar to a running mean, but it also comes with confidence intervals. Thus, a similar figure could be produced, but with the added benefit that for any wind direction it can be tested whether or not the best fit line is significantly different from 4/3. A great example is figure 7 (right frame) where obviously for some wind directions neither the red or black lines are even close

to 4/3, but for other directions the red is similar to 4/3. A LOESS fit would give a quantitative metric to determine where this is significantly different from 4/3 and where it is significantly similar.

I would encourage the authors to reconsider their interpretation of the Huq et al. (2017) paper. While it is correct that those results suggest a magnitude of correction similar to Horst et al. (2015) (i.e., 3-7% as mentioned in their abstract, or ~6%, which is the average of second column of their Table 2), one important distinction is that when Huq et al. (2017) applied the Kaimal (1979) and Wyngaard and Zhang (1985) corrections to their numerically simulated data, the improvement in relative error was rather small (i.e., 2.4-3.4% correction, as derived from their Table 2). Frank et al. (2016) presented data from seven sites around North and Central America where these corrections increased the w measurements by 4.5-6.8% (their Table 2). While it takes a bit of interpretation to compare the results from these papers, one interpretation could be that the numerically simulated turbulence in Huq et al. (2017) tends to produce corrections (either Kaimal or Wyngaard) that are less than what are typically observed in nature. Thus, while an overall correction of ~6% is similar to that of Horst et al. (2015), the Kaimal/Wyngaard correction only accounts for ~50% of this. From this perspective, the findings of Huq et al. (2017) are very similar to those of this paper, which is to say, the currently accepted CSAT3 corrections do improve w measurements, but perhaps only provide a portion of the correction that is ultimately required.

I believe that by addressing these major comments and the following specific comments listed below, that this paper will be appropriate for Atmospheric Measurement Techniques.

Specific comments:

Page 3, line 30: A better definition for "isotropic" should be given before ", which also means". The assumption of isotropic is critical for the theory that leads to the 4/3 ratio from which the entire paper is based. So, a clear definition is important.

Page 4, line 1: The statement "… the velocity power spectra follows the relation," is not self-evident to the casual reader. I would recommend clarifying that Kolmogorov determined this.

Page 4, line 5-6: Clarify "outer scale" Does "the most energy containing scales" refer to something similar to the peak of the spectra as shown in figure 1? Is there a way to describe the "Kolmogorov length scale", i.e., when energy dissipation begins?

Figure 1. Could add a -2/3 slope reference line for comparison.

Page 5, line 7: Does the component i refer to u, v, or w?

Page 5, lines 6-15: I found this "crude" description confusing. k2 and k3 should be defined. The Phi function should be defined. The sentence on line 15 is a repeat of an earlier statement. My big question is whether or not this section is necessary? I'm not sure it really matters much to the main understanding of the paper why the path averaging correction affects u different than v and w. At least, it might not be important enough to derive the theory behind it.

Page 6 line 7 versus Page 7 line 9: Be careful where U is defined as instantaneous versus U defined as an average over 10-minutes.

Table 1: In generally, is there a reason why H06 was only applied to 2 of the 3 datasets? This should be clarified. Also, it is not clear until Table 2 exactly which permutations of the different calculations were analyzed. It wasn't clear from Table 1 and throughout this section which different versions of these data sets were actually tested.

Page 8, lines 18 and 23: The terms "quality signal equal to zero" and "no warnings" are confusing. I am assuming these refer to the manufacturer's diagnostic value that comes from the CSAT3.

Page 8, lines 26-30: This is a strange introduction to the results. It is somewhat telling that the results are described as "we show examples". My intention by encouraging the authors to perform more rigorous statistical analysis (via 1st order polynomial p-values

or LOESS regression, etc.) is to make the results less about "examples" and more about rigorous objective metrics. The word "closely" at the end on line 28 implies some sort of goodness of fit test.

Page 8, last line on page/Page 9 line1: The first part should probably belong in the methods. For the second part, is this something that was observed from this study, or a more general finding that should have a citation?

Figure 3: Which lines does the "c" plot (i.e., 0th order polynomial) fit, w or v? It probably isn't u since that is much lower on the graph. On the caption, when it says "perpendicular", does this mean wind can flow in either direction, e.g., left-right as well as right-left? I assume the range +-10 deg means the average wind direction, not the range of instantaneous wind direction within the 10-min period?

Page 9, lines 10-12: This might be a vast overreach of the data to assume that because "both intervals" in figure 3 appear to fit within a specific inertial subrange, that it applies "irrespective of the wind conditions".

Page 9, second to last sentence: the use of 4/3 is somewhat misleading here. It really has nothing to do with the 4/3 in Equation 2. It is purely coincidence that the uncorrected Metek had a w-u ratio of $\sim$1, such that the improvement from uncorrected to corrected increases the value by $\sim$4/3. To emphasize that this value is not the same as the 4/3 in equation 2, I would simply state it was a 33% increase.

Figure 4. Why is the same graph of w-u red on the left and black on the right? If the running mean was replaced with a LOESS fit, then the confidence interval lines could also be added. In the caption, should clarify if this is the average "wind direction" over the 10-min period.

Table 2. The sharpening criteria should be mentioned earlier in the methods. Also, with a 1st-order polynomial/p-value criteria to include only 10-minute periods with no significant deviation from the -5/3 line, then the sharpening criteria would not be necessary.

Page 11, line 18: Does "lower absolute directions" mean "directions more in line with the boom"?

Page 11, line 22: The threshold +/- 0.003 seems arbitrary without some justification. The Fuw/sqrt(FuFw) <0.02 criteria should be explained in the methods with the definition of isotropy.

Page 12, line 8: This statement also applies to the Metek, although it is much smaller.

Page 12, line 19-20: These do not look that much different to me.

Figure 7: There are three different calculation scenarios presented on the right (no correction, H15, H15+H06). Which one of these applies to the left?

Page 14, line 19: The presentation of ~0.5-1.5% is somewhat confusing. It might be simpler to describe this as "increase by 0.005-0.015", though by looking at the table this would be "0.008-0.018".

Page 14, line 22: Does this really mean that the sonic was physically rotated? This probably refers to rotating the u, v, and w measurements. Also, this methodology seems overly confusing, when it would be much simpler to reprocess the data with the planar-fit rotation.

Page 15, line 14-15: This assumes that the uncorrected portion of the w measurement is simply a scaling issue.

Page 15, line 20-21: I would remove this statement. It is far too oversimplified, and probably extremely unlikely.

Page 15, line 27-28: This is a very bold statement, but it may be justified.

Page 15, line 5 (near the bottom): Should clarify "from the corrected USA-1".

Page 16, line 26: I am confused about the verb tense. By saying "we propose to perform such an analysis" it reads like a recommendation for future research. That

is fine, but if so, a recommendation like this should probably be near the end of the conclusions.

Page 16, line 30: Similar to an earlier comment, the use of ~4/3 here is misleading because it does not have anything to do with the 4/3 in Equation 2. I would use "33% higher than the".

References:

Kaimal, J.C. and Finnigan, J.J. (1994) Atmospheric boundary layer flows: their structure and measurement, Oxford University Press, USA.

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