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Atmospheric Measurement Techniques Discussions

Interactive comment on "Random uncertainties of flux measurements by the eddy covariance technique" *by* Ü. Rannik et al.

Anonymous Referee #1

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This manuscript describes the authors research into several different methods of determining various components of the uncertainty in eddy covariance fluxes. Until recently, this has been a badly neglected subject and it is good to see interest from the community.

While this is overall a very good paper, I find that there are a few issues that could use further explanation and clarification. One issue is contained in the authors equation 8. Through this, they imply that arbitrarily small instrument noise can be obtained through longer and longer integration times. While this is true for a limited range of integrations, it is not true in general. In fact, this is the basis of Allen Variance plots which show the actual noise floor for an instrument. This should be made clear in the text, and they should offer some evidence that their computations do not violate the limits where eqn. 8 holds.

Another issue is their statement that "...the noise component of the vertical wind speed measurement is negligible." While this is small, it must be viewed in the context of usually small vertical wind speed fluctuations. If this were true, then the same could be said of horizontal wind speeds and sonic temperatures which all derive from the same fundamental measurement (sound pulse transit times). If they wish to stand by this statement, they need to provide evidence that it is true, especially in the context of other sonic anemometer parameters. One problem that may contribute to this confusion is the nature by which sonic anemometer data are often recorded. If the wind speeds (and temperature or speed of sound) are recorded digitally, the data streams often are comprised of ASCII character strings where the data are truncated. This may give the impression that the instrument has little or no noise for a particular measurement, but examination of the same data stream, formatted as a binary output might show otherwise. How would a significant noise factor in vertical wind speed change equations 7 and 8?

When the authors discuss the "shuffle" method, they claim that equation 11 is equivalent to equation 7. Where is the justification for this. It's not clear that this is so, and a better derivation would be helpful here. Finally it seems that equations 7 and 12 assume perfect correlation between the noise components of vertical wind speed and "s". One often seen definition of correlation coefficient is the ratio of equation 11 to equation 7. Why does this factor disappear in this analysis? In section 4.3.2, the authors assert that the "shuffle" method over-estimates the instrument system noise because it includes residual turbulent fluctuation information. This would be expected from the description contained in Billesbach's paper. In it, they show that some level of averaging (over different ensembles) is needed to generate a robust noise estimate. The question then arises "What level of averaging did the current authors use, and would they arrive at different conclusions if they included a larger ensemble sample in their analysis?"

These are all issues that ought to be considered by the authors. They are questions that will arise for many readers, and addressing them will certainly add to the usefulness of this already nice work.

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