

Interactive comment on “Improving Lidar-Derived Turbulence Estimates for Wind Energy” by Jennifer F. Newman and Andrew Clifton

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Dear Jennifer,

I think there is still an issue. The idea of my comment was to point out that you can theoretically predict how the radial velocity spectra of the lidar is by knowing 1) the lidar probe volume 2) the velocity tensor and 3) the scanning configuration. This means that this is in principle the way to estimate the difference between variances measured by a sonic and a lidar if they are computed from "single" radial velocity measurements. The issue is that the 2) is not that trivial and the Mann model is just an approximation to the problem (you can always try another model for the velocity tensor). But your discussion should not be centered on the drawbacks of the Mann model but perhaps on the difficulties of the 2). Hope you understand this point.

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FYI: the Mann model has been used to predict the spectra on different atmospheric conditions, heights, wind speeds (see Peña et al. 2010) and for different terrain types (see the documentation of WAsP Engineering and the work by Chougule et al., 2014).

And related to the "correcting lidar turbulence": you can clarify that in a sentence but in my opinion I think the term is just incorrect. The reader will still think that it is wrong. Why not simply state always something like "lidar-based turbulence" or something similar?

Regards, Alfredo

Peña et al. (2010) On the length-scale of the wind profile. QJRMS: 136:2119-2131
Chougule et al. (2014) Spectral tensor parameters for wind turbine load modeling from forested and agricultural landscapes. Wind Energy: 18:469-481

[Interactive comment on Wind Energ. Sci. Discuss., doi:10.5194/wes-2016-22, 2016.](#)

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