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Comment on wes-2021-68

Anonymous Referee #2

Referee comment on "Approaches for predicting wind turbine hub-height turbulence metrics" by Hannah Livingston et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-68-RC2, 2021

Relationships between hub-height turbulence and ground-level atmospheric measurements would be helpful, no doubt. However, as experienced by the authors, there is no such thing. It would be excellent if simple power-law correlations could solve turbulence.

The manuscript is nothing more than a series of plots showing the curve fitting of the experimental data.

There is no justification for using and assuming one single mathematical function: power law.

Discussion of the results is feeble. For instance, why do coefficients and exponents differ among the various data sets?

There is no quantification of the success of the curve fitting.

Tower TSE09, in the valley, is not relevant for wind energy studies. The flow in the valley is much different from the flow near tower TSE04, as evidenced in the figures, and it is not reasonable to expect similar trends if they existed.

TI (turbulence intensity, eq. 1) was calculated over 10-minute intervals to match the current wind energy industry standard, and TKE (turbulent kinetic energy, eq. 2) was

calculated over 30-minute intervals, a common choice to study atmospheric boundary layer processes. This practice is not consistent and, therefore, unacceptable.

The authors' main objective is to find correlations between hub-height turbulence and ground-level atmospheric measurements. However, why did they take this endeavour? Were they aware or had any signs of the success of their methodology under ideal conditions, i.e., flat terrain and neutral conditions? Of course, they were not, simply because there are none.

In conclusion, I cannot recommend the publication.