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Reviewer comment on wes-2021-68

Anonymous Referee #1

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-RC1, 2021

The manuscript discusses an approach for estimating turbulence intensity (TI) and turbulent kinetic energy (TKE) from near-surface measurements in complex terrain sites. The selected topic is important and especially relevant for onshore wind energy applications with growing hub heights. For that, the authors show correlation plots between TI and TKE at distinct heights with chosen datasets combined in 100 bins with variables sizes and uniform sample size. A polynomial regression is than applied to the binned data. The authors conclude that no simple or universal relationship can be drawn from their univariate approach.

This manuscript has **major issues**.

The authors claim the goal is to assess to what degree TI or TKE at 80 m above ground (called “hub-height” by the authors) can be estimated via near-surface measurements. However, the proposed methodology is based solely on a correlation between variables. The validity of the presented regression plots to other heights in the exact location or other locations is most likely not applicable, especially in complex terrain. For the binned data, the error bars are not shown, which is important to assess how representative the regression functions are. For the polynomial functions, no regression coefficient is shown, which also hinders the analysis. The authors show that not even the TI vs. wind speed relation holds when using near-surface observations. Even for the heights and locations presented, the authors do not quantify the error in case the fitted polynomial functions were to be used.

In my view, the quantity and choice of datasets was unfortunate. The authors choose only three pairs of measurements (at 10 m and 80 m above ground) from two complex terrain experiments (Perdigão and WFIP2), which does not allow any general conclusion for the observed trends in complex terrain, apart from site-specific remarks or straightforward

conclusions, such as a positive correlation between TI and TKE. Also, the relations between 10 m and 80 m at WFIP2 are done with measurements located more than 1 km apart, i.e., likely further than the upstream fetch of the 80 m observations. In terms of temporal coverage, the authors do not detail the length of the filtered dataset. Other sites could be used for this purpose, e.g., other experiments within the New European Wind Atlas (NEWA) project that have multiple masts with 3D ultrasonic anemometers sampling at 20Hz for a one-year period.

I cannot foresee any additional analysis within the lines of the proposed methodology that would justify the publication. The authors promote machine learning techniques as a potential solution for this research problem. However, the extension of the presented results using such tools would be, in my view, the scope for a completely new paper. Therefore, I suggest the rejection of this manuscript in its current state.