Reply to the Editor's review of "Brief communication: A note on the variance of wind speed and turbulence intensity"

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Please note that the Editor's comments are in *italic*, my responses in regular font, and the changes to the manuscript in blue color.

- *About mean(v)* $\neq 0$

  As the reviewer pointed out, the manuscript considers the case where mean(v) $\neq 0$*. However, in boundary-layer meteorology, turbulence intensity is typically calculated after rotating the coordinate system into a wind-aligned frame, which by definition results in mean(v) = 0. From this perspective, performing such a rotation is a necessary step before calculating turbulence intensity, whether for an individual component or for the horizontal wind. To me, it would be unthinkable not to apply this rotation beforehand. But perhaps this approach is not as widely known outside the micrometeorology community as I assumed. Maybe this is another example of a clash between disciplines? In any case, this is indeed important to point this out.*

  I rewrote the introduction to better introduce the issue of the coordinate system and discuss how different disciplines use different axis conventions, which may have contributed to the confusion in the literature. Please see the paragraphs in the Introduction that start and end with:

  "The first problem is the system of coordinates. [...] In summary, the relationship between the variance of wind speed and those of the wind components depends on the system of coordinates and therefore confusion can arise among disciplines because of their different axis conventions.

- *About reviewer 2's question*

  Regarding your response to Reviewer 2 (on the correlation between the u and v components): It is true that u and v are not entirely uncorrelated. However, in most situations, the correlation is low enough that they are often treated as uncorrelated. I've only observed significant correlation in very specific environments, such as Norwegian fjords, where sonic anemometers were positioned near steep mountain slopes.

  The wind components are weakly correlated in general, including in the AWAKEN dataset that I used in this manuscript. As you can see from Figure 1a and Table 1, when the covariance of u and v (σ_{uv}) is accounted for (Eq. 27), the error is lower than when the covariance is ignored (Eq. 28). I agree that the error is small and in fact Eq. 28, which assumes that the wind components are uncorrelated, is a very good approximation.

  However, what I stated in my reply ("Note that σ_U^2 would be equal to the simple sum of the variances of the wind components ($\sigma_u^2 + \sigma_v^2 + \sigma_w^2$) if and only if the wind components were independent from each other and therefore their covariances were zero.") is not true and therefore I removed it from the manuscript. It would be true if the wind speed magnitude was the linear sum of the two wind components; but it is a vector sum, thus a non-linear function (square root of the sum of the squares of the wind components). Using the sum of the wind component variances is just wrong in all situations, including when the two wind components might be entirely uncorrelated or even independent, because it has no theoretical or practical basis.

"There is no theoretical or statistical justification for this incorrect expression and no special case (e.g., independent or uncorrelated variables, or a specific statistical distribution, or particular spatial conditions) for which it would apply."

- *Now to Reviewer 2's final comment:*

  "Else I agree with the editor's comment on the lack of physical sense of characterising turbulence using one TI based on U versus having different TIs for u, v, and w as we would typically do in wind systems engineering. Now, that is in practice combined with an assumption of uncorrelated components, which I agree is discussable. What would be needed is a covariance matrix."

  I believe the answer regarding the applicability of the IEC standard is not entirely straightforward, as the IEC may be internally inconsistent. According to IEC Standard 61400-1, Equation (11), turbulence intensity is defined for the longitudinal component. In that sense, the standard does not contradict the reviewer's comment. More specifically, σ_1 *is defined as the standard deviation of the longitudinal wind component (Section 6.3), while* σ_2 *and* σ_3 *correspond to the lateral and vertical components, respectively. The subscript "1" clearly refers to the longitudinal component—not the horizontal component—as further supported by Equation (7) and the well-known "hypothesis of local isotropy in the inertial subrange." Importantly, this hypothesis does not apply to the horizontal wind component.*

  Nevertheless, as you correctly point out, the IEC also defines turbulence intensity more generally as "the ratio of the wind speed standard deviation to the mean wind speed, determined from the same set of measured data samples of wind speed, and taken over a specified period of time." To me, this reflects a significant inconsistency in the IEC's definition of turbulence intensity—one that may need to be addressed. If you agree, it might be worth highlighting this issue in your manuscript.

  I reviewed the IEC standard carefully and I agree with you that it uses the rotated axis convention for which $\sigma_U^2 \approx \sigma_1^2$. I expanded the discussion of the IEC standard in the Introduction as follows:

  "The second problem is that of internal and external inconsistencies in the IEC standard. While the IEC standard clearly defines TI as the "the ratio of the wind speed standard deviation to the mean wind speed" in the "Terms and definitions" section, in later sections it actually appears to use σ_1, not σ_U, to define normal turbulence conditions and for fatigue load calculations (e.g., their Eq. 11). This would imply, wrongfully, that only the longitudinal fluctuations of the wind vector are relevant to a wind turbine performance. Also, the IEC standard is possibly the only case in which a single value of turbulence intensity is adopted. In most fields, including wind systems engineering, three turbulence intensities are typically used, one for each direction ($TI_x = \sigma_u/\bar{U}$, and similarly for TI_y and TI_z). Lastly, the IEC standard assumes explicitly that the "turbulence standard deviation σ_1 [...] shall be assumed to be invariant with height", while it is well known that there is a vertical gradient of TI in the atmospheric boundary layer, thus the turbulence fluctuations measured, for example, near the ground are not representative of those at hub height."