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

January 3, 2025

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

- *Line 15-20: The distinction between aligning the x-axis with the wind direction or with the East-West coordinate system is not unique to wind energy; it largely depends on the spatial and temporal scales of interest. In boundary-layer meteorology, particularly micrometeorology, the x-axis is typically aligned with the mean wind direction due to the focus on turbulence, as detailed in Kaimal and Finnigan (1994). In mesoscale meteorology, where the emphasis is on mean wind speed, the x-axis is, indeed, often aligned with the East-West direction. To avoid conflating discipline-specific conventions, I recommend acknowledging this broader context.*

  I agree that the convention of aligning the x-axis along the mean wind is not unique to the wind energy field. I added the following at line 13:

  This convention is also adopted in boundary-layer meteorology, particularly in micrometeorology, due to the focus on turbulence (Kaimal and Finnigan 1994).

  and the following at line 14:

  By contrast, in mesoscale meteorology and, more broadly, in geophysical applications, such as meteorological field campaigns or simulations of weather events, the convention is ...

- *I would go beyond the statement that the variance of the wind speed is often miscalculated. I would argue that using the variance of the wind speed itself—rather than treating the variance of the along-wind and cross-wind velocity components separately—is fundamentally problematic. In wind engineering and micrometeorology, these components are considered separately due to their distinct characteristics. The design of wind turbines, particularly for structural and turbulent loading considerations, is based on the variances of the along-wind and cross-wind components, not the wind speed. The continued use of wind speed variance might be a legacy of outdated practices.*

  Line 28-29: The statement "turbulence intensity is a function of the standard deviation of wind speed" could be misleading. From micrometeorology and wind engineering perspectives, turbulence intensity is typically defined based on the individual velocity components (along-wind, cross-wind, and vertical), not wind speed. Defining turbulence intensity based on wind speed lacks physical relevance. In my humble opinion, its continued use in wind energy science is puzzling.

  I agree with you on both statements, and that is partly why I wrote this note. Turbulence intensity to me does not make sense without specifying along which direction. And yet the IEC standard uses exactly the definition that you are referring to. As such I modified the text as follows:

  Since turbulence intensity is defined in the IEC standard as "ratio of the wind speed standard deviation to the mean wind speed" ...

  and

  It is important to note that the IEC standard is possibly the only case in which a single value of turbulence intensity is adopted. In most fields, three turbulence intensities are typically used, one for each direction ($i_x = \sigma_u/\bar{U}$, and similarly for i_y and i_z), where x, y, and z are either the three Cartesian directions (e.g., in mesoscale meteorology) or the along-wind, cross-wind, and vertical directions (e.g., in micrometeorology, wind turbine design, and wind turbine load studies).

- *Line 31: While it is true that mesoscale meteorology often simplifies wind velocity as a 2D vector, this approach does not hold in micrometeorology or wind energy, where the vertical velocity component significantly contributes to turbulence kinetic energy (TKE). I understand that the inclusion of TKE in this discussion depends on the desired level of detail. If brevity is prioritized, this aspect could be omitted.*

  Point well taken. I modified the notation in Sections 2 and 3 to be fully 3D. Then I introduced the simplification of a 2D vector at the end of Section 3, for the sake of simplicity and because the 2D approximation has often been used in wind energy applications. I believe the reason why the 2D approximation has often been adopted in wind energy is that cup anemometers have been historically used instead of sonics, and therefore it was not possible to measure the vertical component of the wind anyway. Here is the modified text:

  To simplify the notation without loosing generality, we hereafter assume that the wind is a two-dimensional vector. This assumption is often used in mesoscale meteorology and is needed when only 2D measurements of the wind are available (e.g., with a cup anemometer). Thus all terms that are a function of w drop from Eq. 17:

- *Conflict of Definitions in Different Fields: There may be conflicting definitions of "turbulence" between mesoscale and microscale meteorology that require clarification. In micrometeorology, turbulence is typically considered a three-dimensional process occurring within temporal scales of up to one hour and spatial scales smaller than a few kilometres. In micrometeorology, the variance of the along-wind and across-wind components differs significantly. Motions exceeding these scales are often classified as "non-turbulent motion," consistent with the concept of the spectral gap. However, mesoscale meteorology may occasionally describe such motions as "2D turbulence." These differences reflect divergent focuses and terminologies across disciplines and should be recognized explicitly.*

  The reason why I introduced the simplification of a 2D wind vector in the first version of this note was that all the papers in the literature that have used the incorrect approximation were using it for 2D, i.e., they were summing the variances along x and y only. Since I made the change that you recommended and used the full 3D notation in the revised version, I do not think I should discuss the different definitions of turbulence across different fields here.

- *Table 1: The two examples in Table 1 effectively demonstrate the value of the proposed equation. However, it is unclear whether the statistics are based on six hours of data or shorter sub-samples. If turbulence is the focus, time averaging over six hours is not appropriate, especially since the second panel shows clear non-stationary fluctuations. If the table uses shorter intervals (e.g., 10 minutes to 30 min), I recommend expanding the analysis to include all samples from Figure 1. Comparing the wind speed variance estimated by the older equation with that from your proposed equation would strengthen the analysis. A scatter plot of these comparisons across the full dataset would complement Table 1. This visualization would make it easier to assess the overall performance and accuracy of the new equation relative to the older one.*

  Thank you for pointing out that the second case was non-stationary and for explaining that a six-hour window for turbulence statistics is too long. I shortened the averaging window to three hours (instead of six) and found another case that was stationary (10/21/2016). All the statistics have been recalculated in Table 1.

  As for the comment "Comparing the wind speed variance estimated by the older equation with that from your proposed equation would strengthen the analysis," I had already done this comparison in Table 1 (last 2 rows). To make it clearer, in the revised version I added the labels "Wrong σ_U^2" and "Wrong TI."

  The data collected during the VERTEX campaign were post-processed and averaged over 5-minute intervals and those are the only data available. The original 20-Hz dataset was not retained unfortunately. As such, the scatter plot comparison that you are proposing is not possible.