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

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

Reviewer # 1

- *This paper deals with the difference between the variance of the wind component along the mean wind vector and the variance of the length of the wind vector, also called the wind speed. It is well known that those quantities are under most circumstances (i.e. not too high turbulence intensity) almost equal (e.g. L.. Kristensen 1998, JTech, vol 5, p6). The transverse component enters only the speed variance to second order in the turbulence intensity (see eq 8 in the mentioned paper). These observations do not change if the coordinate system is not aligned with the wind.*

  The variance of the wind speed σ_U^2 and the variance of the component of the wind vector that is aligned along the mean wind direction σ_u^2 (if available), are indeed almost equal. For example, for the AWAKEN data that are discussed in the revised version of the manuscript, I calculated the variance of the x- and y-components after rotating the axes so that the x-axis would align with the mean wind direction, recalculated every 10 minutes. I found that σ_u^2 is indeed a very good approximation for the variance of the wind speed σ_U^2, with an average absolute percent error of 2.5%.

  This fact is acknowledged in the manuscript:

  "With this convention [to align the x-axis along the mean wind direction], the variance of wind speed is accurately approximated as the variance of the u-component of the wind, i.e., the component along x."

  However, in the manuscript I am not talking about the "rotated" coordinate system, in which the x-coordinate is in alignment with the mean wind direction; I am talking about the geophysical coordinate system with coordinates aligned with the east-west (x), north-south (y), and vertical (z) directions, like in sonic anemometers (e.g., see the first sentence of the Definitions section: "Let us use the geophysical system of coordinates."). The rotated statistics are not always available and, to calculate them accurately, the raw data are needed. But, if the raw data are available, one might as well calculate the wind speed and its variance directly without bothering with the coordinate rotation.

  The points I am trying to make are that:

  1. an inaccurate equation has been used in the literature for cases with the **geophysical** coordinate system; and
  2. the wrong equation has been used as the **definition** of the standard deviation of wind speed, whereas it just provides an approximation for it, and not a good one.

  The paper by Kristensen (1998) deals with the calibration of cup anemometers in wind tunnels under steady-state conditions. Eq. 8 in particular is:

$$U = \sqrt{u^2 + v^2} \approx \bar{u} + u' + \frac{v'^2}{2\bar{u}}. \tag{1}$$

You cite this equation to support that transverse perturbations only affect the variance of wind speed to the second order; however, this is **not** an equation for the variance of wind speed, it is an equation for the wind speed, thus the point is not proven with this equation.

The following sentence was modified to clarify that the proposed equations are for the geophysical reference system:

"This note addresses this issue by proposing an analytical approximation for the wind speed variance and one for turbulence intensity for the geophysical system of coordinates."

- *The other subject paper is an apparent mistake in the literature. The author states that the variance of the wind speed is sometimes mistakingly said to be equal to the sum of the variances of the two horizontal components. This is obviously wrong, as the author clearly states, but I'm am unaware of these mistakes in the literature. The author does not provide evidence for these mistakes, which makes the need for this paper limited. The author might be wary to point out mistakes in specific papers, but this is unfortunately what has to be done in order to advance science. You cannot leave it to the readers to find documentation for this possible mistake in the literature.*

  I am indeed uncomfortable publishing a note that directly points out mistakes by fellow scientists. In addition, it would take a huge effort to try to find all occurrences of the mistake in the literature. The point of my note is to provide a clear reference as to why the two variances are not the same. As such, I provide below a list of five papers with the above-mentioned error. My intention here is to satisfy your legitimate request for evidence, but I do not intend to add this list in the main document. Since the entire review process is public in WES, it will be possible in the future to find this information anyway, but, as far as I am concerned, not in the main manuscript.

  - Eq. 6 in Joffre and Laurila (1988);
  - Eq. 1 in Mortarini et al. (2016);
  - Eq. 1 in Lee and Lundquist (2017);
  - Eq. 1 in Bodini et al. (2020); and
  - Eq. 11 in Klemmer et al. (2024).

  As you recommended in the online public discussion, I decided to add a citation to the oldest of the five papers above, i.e., Joffre and Laurila (1988) as follows:

  "and often treated, incorrectly, as an exact definition (see for example Eq. 6 in Joffre and Laurila (1988))."

Editor

- *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 the "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.*

I added a discussion on the time scales and disciplines, per your and Reviewer # 2' suggestion, as follows:

"The IEC definition of TI is also troubling because it does not specify which temporal scales should be considered in its calculation. Strictly speaking, turbulence intensity should refer only to fluctuations of the wind in the micro-scale (i.e., time averages of the order of minutes), thus to the right of the spectral gap in the wind spectrum. By contrast, wind fluctuations associated with meso or synoptic scale features belong to the left of the spectral gap and should not be called turbulent. In such cases, the ratio of the wind speed standard deviation over the mean, calculated over longer time intervals (i.e., hours to days), can still be obtained, but it should not be called a "turbulence" intensity. The equations derived here may be applied to any scale, but the focus is on the micro-scale."

- *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 was able to obtain another dataset, from the AWAKEN field campaign, which contains raw data at 20 Hz, and therefore I could calculate the 10-minute statistics directly and compare them against my formulas. I was able to prepare new figures with the scatter plots that you suggested. The message is even clearer now.

Reviewer # 2

- *In my opinion, this relates to 1) split disciplines between wind systems engineering and micrometeorology, and 2) a confusion between timescales (and intrisically linked space scales) in the literature and current practice. There I would first like to mention that while wind engineering is my background, I know only little about meteorology.*

  I think that you "nailed" the issue perfectly. I was originally thinking that reason 1) was the main culprit, but your suggestion about time scales is very interesting.

- *Wind turbine structures are typically only concerned by microscale, and 10-minutes load cases are tradiationally used. There it is assumed that turbines would yaw to align with an assumed constant wind direction. Fluctuations are then represented around this direction and separated into along-wind and cross-wind drections. In this case the mean cross-wind component v_bar is always zero, and the original equation to compute TIs is valid. An exception may be for wake steering applications where a yaw misalignment with mean flow is intentionally created, but the coordinate system used to represent the wind speed is still relative to the slowly-varying wind direction.*

  Even if the mean wind direction is used as the x-axis, and therefore $\bar{v}$ is zero, neither the original formula for TI (now in Eq. 9) nor that for σ_U^2 (now in Eq. 8) are valid. I think the origin of the error is in the calculation of σ_U^2, which is equal to the original wrong formula if and only if the x- and y-components are independent from each other and therefore the co-variances are zero; in other words, if turbulence is purely isotropic (never in the real atmosphere). Only in such a case would the original formula be correct.

  Note that, with the x-axis aligned with the mean wind direction, an excellent approximation for σ_U^2 is actually σ_u^2 (not the original wrong formula in Eq. 8). The error is a few percent at most. I recognized this in the manuscript:

"With this convention [to align the x-axis along the mean wind direction], the variance of wind speed is accurately approximated as the variance of the u-component of the wind, i.e., the component along x."

- *However, longer load cases may be of interest when for instance looking at slowly-varying motions of floating substructures or power fluctuations from wind farms heavily influenced by mesocale fluctuations. In this case, the concept itself of characterising wind fluctuations by turbulence intensities covering all timescales (integrated over the entire width of the wind spectrum) is discussable, and might be outdated practice. Strictly speaking, TIs should only be used to describe microscale fluctuations (i.e what is commonly referred to as turbulence, to the right of the spectral gap in the wind spectrum), while mesoscale fluctuations (to the left of the spectral gap in the wind spectrum) should be described by a distinct quantity. Assuming "mesoscale turbulence intensities" are used for this purpose, I agree that they should be calculated using the method you suggest.*

  The method that I suggest is valid for any scale, not just for the mesoscale. The one and only assumption is that the fluctuations should not be too large. However, I agree that the ratio of standard deviation of wind speed over mean wind speed should be called "turbulence intensity", as the IEC standard does, only if it refers to micro-scale turbulence.

  In meso- or large-scale meteorology, there are obviously means and perturbations, but the latter are never referred to as turbulence, rather, as "eddy" or "transient" features. I have also seen "zonal" and "meridional" being used to indicate the mean flow (generally along the latitudinal zones) and the perturbation (generally north-south), respectively. The a-geostrophic wind, for example, is nothing but a perturbation of the wind around the geostrophic wind vector; yet, nobody would refer to it as turbulence.

  I used a six-hour window to calculate the statistics in the figures included in the original manuscript. This was a poor choice on my side, which also the Editor pointed out, because it may have given the impression that my equations are only valid at the mesoscale and not at the micro-scale. I suspect you may have been confused by that too. In the revised version, I redid the analysis over 10-minute windows with another dataset, the AWAKEN field campaign, for which 20 Hz raw data were available.

- *To improve the quality and impact of your manuscript, I would suggest to 1) make this distinction between scales, disciplines and applications clearer, particularly through the role of turbine yawing*

  I added a discussion on the time scales and disciplines, per your and the Editor's suggestion, as follows:

  "The IEC definition of TI is also troubling because it does not specify which temporal scales should be considered in its calculation. Strictly speaking, turbulence intensity should refer only to fluctuations of the wind in the micro-scale (i.e., time averages of the order of minutes), thus to the right of the spectral gap in the wind spectrum. By contrast, wind fluctuations associated with meso or synoptic scale features belong to the left of the spectral gap and should not be called turbulent. In such cases, the ratio of the wind speed standard deviation over the mean, calculated over longer time intervals (i.e., hours to days), can still be obtained, but it should not be called a "turbulence" intensity. The equations derived here may be applied to any scale, but the focus is on the micro-scale."

- *2) add references to where you claim erroneous formulations have been used (coming from a different field, this statement looks superficial without examples)*

  Also Reviewer #1 suggested that the papers where the issue was found should be listed, but I do not want to do it because, first, I do not want to "point the finger" at colleagues, and second, I cannot possibly provide a complete list. Here is a quick list of five papers:

  – Eq. 6 in Joffre and Laurila (1988);
  – Eq. 1 in Mortarini et al. (2016);
  – Eq. 1 in Lee and Lundquist (2017);
  – Eq. 1 in Bodini et al. (2020); and
  – Eq. 11 in Klemmer et al. (2024).

Note that it is WES policy that the review files remain publicly available, thus this list, if one really wanted, can always be found. As a compromise, Reviewer #1 suggested that I list only the very first paper in the main text, which I did as follows:

"and often treated, incorrectly, as an exact definition (see for example Eq. 6 in Joffre and Laurila (1988))."

References

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