Responses to reviews

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We thank the reviewers and the editor for the valuable comments and suggestions, which helped us greatly in improving the quality of the paper. We provide responses to the editor's comments and suggestions (in blue) in the following, point-by-point, in black. A track-change ready version of the paper is made ready.

1 Reply to Comments and suggestions from the initial file validation

- Please add the full address including street, city, country to the affiliation.

  Suggestion taken.

- Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly. –> Figs. 2, 3, 7

  Figures 2, 3 and 7 have been revised and updated following the suggestion of using color blind-friendly color schemes.

2 Replies to the Editor's comments and suggestions

Given my familiarity with and interest in the subject, I have taken the liberty of providing a few feedbacks. I hope the authors will receive the following comments in a constructive spirit. They are intended primarily to reduce the risk of potential misunderstandings by readers and to improve clarity.

- Section 1, Paragraph 2 – Cost and Practical Constraints of Measurements The argument related to financial cost is relevant and valuable, but it would benefit from clarification. The manuscript currently postulates that sonic anemometers (SA) are the most expensive sensors; however, in practice, lidars are typically 5–20 times more expensive than a 3D sonic anemometer. The major cost driver of in-situ measurements, particularly offshore, is the meteorological mast itself rather than the SA. This is precisely why lidar-based solutions are often preferred in offshore contexts. It would therefore be helpful for the manuscript to explicitly distinguish between sensor costs and infrastructure costs. To give a sense of scale, I mention some order of magnitudes for these sensors (at least from what I remember): • Construction of a tall offshore mast (e.g. FINO1): $\geq$ 10 M • 3D sonic anemometer: 5–30 k • Profiling lidar: 70–130 k • Long-range scanning lidar: 100–500 k

Indeed, it is the overall cost associated with the use of the infrastructure that it is meant. We accept the editor's advice and changed the sentence to "but the associated cost is also the highest".

- The manuscript highlights a one-year time constraint for wind measurements. In practice, this is not necessarily the dominant limiting factor. Financial, planning, and regulatory processes, particularly permitting and environmental impact assessments, often extend over several years. Wind resource measurements are commonly undertaken at early stages of project development and can usually be conducted in parallel with these processes. As a result, measurement duration is rarely the critical path, unless in-situ measurements are not required or are otherwise constrained by project-specific conditions.

  "One-year measurement" was mentioned once in the paper in connection with the use of a figure from literature in which one-year sonic anemometer data were used. The focus of that figure is rather on the temporal range of hours to minutes – the gray zone – than the length of the data. To avoid the confusion, we replaced "one-year" with "long-term" in the sentence.

- Equation (2) – Turbulence Intensity Definition and ISO Standard The ISO definition of turbulence intensity (TI) is indeed rooted in the work of Andersen and Løvseth from the 1990s at Frøya (Norway). However, the current wording appears to suggest the reverse, namely, that Andersen and Løvseth based their work on the ISO standard. This should be rephrased for historical accuracy. As an optional but potentially valuable improvement, the turbulence intensity model of Andersen and Løvseth (2006) could be moved from the appendix into the main body of the paper. For reference, this model has been tested against measurements at the FINO1 offshore platform and shown to perform reasonably well, even for wind speeds below 10 m/s (Cheynet et al., 2024).

  Thanks for pointing this out and we rephrased the corresponding text as suggested accordingly. We used "Before the ISO standard, Andersen and Løvseth…", and we moved their linear model from appendix to the main text. The 10 m/s is introduced following Andersen and Løvseth, but we now changed to "about 10 m/s", as the study that the editor pointed out, Cheynet et al. (2024) did validated it with FINO1 data to 8 m/s. We assume the reviewer refered to this study: "Metocean conditions at two Norwegian sites for development of offshore wind farms", https://doi.org/10.1016/j.renene.2024.120184). We added this to the paper too. The main message in our paper is the decreasing dependence of TI with U at lower wind speed.

- Equations (1–4) – Relationship Between Turbulence Intensity and Wind Speed In general, caution is advised when relating turbulence intensity directly to wind speed. By definition, TI is proportional to the inverse of the mean wind speed, which introduces explicit self-correlation and limits physical interpretability. This point is well known but often overlooked, and it may be useful to explicitly warn the reader. A more rigorous approach would involve analysing the standard deviation of the velocity components as a function of mean wind speed rather than TI itself. That said, such an analysis may fall outside the intended scope of the paper.

The authors agree that self-correlation can be a factor contributing to the complicated relationship between TI and U. As TI is the parameter that is being used in many applications, it is difficult to skip it and focus on standard deviation of wind speed. We agree that it is a subject outside of the scope of this paper.

- Relatedly, the Mann model does not possess an inherent turbulence intensity; its implied TI depends on the target spectrum used for calibration. Furthermore, it is unclear where Veers (1988) is proposed as introducing a spectral turbulence model. Rather, Veers refers to existing models (e.g. Frost, Kaimal, von Kármán, Solari). This distinction should be clarified to avoid ambiguity.

  The corresponding text has been re-written: “Usually, the calculation of turbulence and hence TI considers only surface-driven three-dimensional (3D) turbulence; the 3D turbulence is described through spectral models through, for instance, the Kaimal model (Kaimal et al. 1972), the Mann model (Mann 1994) and the various spectral models reviewed in Veers (1988)”.

- Section 2.1 – 2D vs. 3D Turbulence and Historical Context. Additional references are needed to support the discussion of 2D versus 3D turbulence. These concepts significantly predate the 2010s, and placing them in their historical context is important. Relevant foundational references include: • Kraichnan, R. H. (1967). Inertial ranges in two-dimensional turbulence. Physics of Fluids, 10(7), 1417–1423. • Charney, J. G. (1971). Geostrophic turbulence. Journal of the Atmospheric Sciences, 28(6), 1087–1095. • Nastrom, G. D., Gage, K. S., & Jasperson, W. H. (1984). Kinetic energy spectrum of large- and mesoscale atmospheric processes. Nature, 310(5972), 36–38. For pedagogical reasons, it would also be helpful to briefly clarify what is meant by “turbulence” in the context of this manuscript. While the distinction between 2D and 3D turbulence is a good idea, many practitioners in wind energy implicitly define turbulence as a strictly 3D phenomenon and classify mesoscale 2D motions as “non-turbulent.” Clarifying that this distinction is largely terminological and discipline-dependent would help avoid misunderstandings.

  It is a good idea to include several more references on the 2D turbulence, and we implemented this suggestion by adding 10 more references including the above mentioned three publications by the editor. We also commented on the names for the large-scale or mesoscale fluctuations and decided to go with the two-dimensional (2D) turbulence, following Kraichnan and Lindborg. The corresponding text has been edited.

- On the definition of the turbulence intensity For wind loading on structures, turbulence intensity is defined based on the standard deviation of each velocity component (along-wind, across-wind, and vertical), rather than on the wind speed magnitude itself (cf. Eq. 5). Some wake deficit models, however, may use a turbulence intensity definition consistent with Eq. (5). When applied to wind load modelling, this formulation, based on the standard deviation of wind speed, may therefore be misleading. Notably, IEC 61400-1 provides two different definitions of turbulence intensity, which are not directly compatible. It is therefore important to clearly warn the reader about these limitations and the context in which each definition is used.

We follow the editor's advice and add the following after Eq. 6: "Note that in practice, sometimes rather than the standard deviation of the wind speed, it is the standard deviation of the wind components that is used to define the turbulence intensity". Specifically, it is stated in the last but one paragraph in Section 1: "The output is valuable for an initial evaluation of the cost related to the design of the wind turbine and the wind farm".

- Self-Citation Rate and Literature Context The self-citation rate approaches 30%, which is relatively high. This may indicate that parts of the broader literature on turbulence intensity have been under-represented. Turbulence intensity has been studied for more than six decades, and while the authors have made important contributions to the field, it is important to more explicitly situate recent work within this extensive body of foundational research. Emphasizing this continuity, "standing on the shoulders of giants", would strengthen the manuscript's positioning. Citation: https://doi.org/10.5194/wes-2025-245-EC1

We added several references and removed some of our own, and now the self-citation rate is 13%.