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%.

Responses to reviewer 1

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

General comments:

This manuscript presents a global offshore atlas for turbulence intensity from 10 m to 200 m. Generally, the manuscript is well written. The methodology is presented clearly.

In my opinion, constructing a global atlas for turbulence intensity is never easy. Therefore, while I think the errors/uncertainties in the adopted methodology might be significant, my suggestion is that the manuscript can be accepted for publication after minor revision. Some comments are given below for the authors' consideration. Major comments:

1. There is a general lack of qualitative or quantitative assessment of the possible errors/uncertainties in the methodology. Some models used by the authors may be too simple, leading to high uncertainty when applied to turbulence intensity estimation under various atmospheric and oceanic conditions worldwide. Please consider including a qualitative (or quantitative if possible) discussion on the uncertainty in the manuscript.
   This is a very good suggestion. We elaborated the discussion on uncertainties and their sources, and re-wrote most part in the section Discussion. This part of text is now more systematic, including all steps involved in the method.

2. It is suggested to clarify the applicable range for the equations in Eqs. 1 to 24, e.g., surface layer/boundary layer, neutral/unstable/stable stability conditions, low/moderate/high wind speed, etc.
   It is a very good suggestion. We went through these equations and made remarks wherever possible and where they were missing in the original draft.

3. Section 2.3, if I understand correctly, the effect of atmospheric stability is considered only through the modification to the wind profile according to the flux-profile similarity (i.e., the MOST). However, according to the flux-variance similarity, the variance of wind speed normalized by friction velocity is also a function of z/L (like Eqs. 23 and 24). This effect of atmospheric stability on variance is non-negligible and should be considered.
   The reviewer is right about the effect of stability and how it is included in the current calculation. We also agree that it makes better sense to also include stability effect in describing the turbulence spectrum. However, it is not an easy task.

As a matter of fact there are no existing models that have been proven generally useful for that purpose, even though there have been numerous attempts. Some studies have suggested revised models for unstable conditions (e.g. Højstrup J (1982) Velocity spectra in the unstable boundary layer. J Atmos Sci 39:2239–2248), whereas it is more challenging for describing stable conditions due to relatively smaller eddies and less organized structures in the atmosphere. The turbulence spectra and co-spectra are the base for calculating the variance of wind speed, and co-spectra of u and w (hence friction velocity). The current study uses a 2D turbulence model to merge with the 3D turbulence model, which was argued in Larsén et al (2021) as useful approach to account for the effect caused by convection, as the stability parameter z/L is not universal across the boundary layer.

We admit that we cannot solve the stability issue entirely in the current study, but we followed the reviewer's thoughts and add discussions on this, see page 16 - 18.

4. Section 2.5, boundary layer height should be a key parameter in the parameterization of turbulence intensity near the turbine hub height, which is 100 m or so nowadays. However, the treatment of boundary layer height seems overly simplistic in this study (based on a friction velocity-dependent formula applicable only under neutral conditions). I speculate that this simplification may introduce significant errors. For example, in the stable boundary layer, the boundary layer height may be lower than 100 m, leading to minimal turbulence at hub heights.

   We restructured the entire Section 2.5, and edited some of the text, so that it becomes clear that our method LUT does not use boundary layer height explicitly. The height dependence in our study is driven by the height dependence in the power spectra, wind speed etc. In Section 2.5, rather, the two existing models from Stull and Högström are used to compare with our calculation. The two models provide description of the vertical variation of variances, where the boundary layer height z_i was used as a parameter. To address the uncertainty associated with zi, we used $z_i = cu_*/f$, but used u_*=0.1, 0.2 and 0.3 m/s to cover some degree of variation of zi, which could be associated with stability. Since we do not use z_i in our model, we did not go into the details of parameterizing z_i.

Minor comments:

1. Over land, the variation of turbulence intensity with wind speed at low wind speeds may be largely related to atmospheric stability. At low wind speeds, unstable conditions may dominate because low wind speeds usually correspond to low wind shear and high thermally generated turbulence in the daytime. In my opinion, this is why IEC suggests a decrease of turbulence intensity with wind speed. However, over ocean, due to the large heat capacity of the water, the diurnal cycle of atmospheric stability may be negligible or absent. As a result, the decreasing trend of turbulence intensity with wind speed may not be observed. The authors are suggested to consider these factors and revise their presentation of the methodology accordingly.

   We agree with the reviewer's arguments on the suggestions from IEC standard. And, over water, the stability associated with diurnal cycle is indeed minor in comparison with land conditions. However, over water, stability is not affected that much by diurnal cycle (except for coastal zones), it is to a great degree affected by passing air masses, causing that the stability distribution is far from being neutral. There are plenty publications on stability distribution over water bodies. For instance, a list of publications suggests that climatologically the North Sea is characterized by unstable conditions whilst the Baltic Sea by stable conditions. The existing unstable stability conditions over the North Sea are also the reason that we do observe the decreasing TI with wind speed at all sites analyzed in the current study. Here are some examples:

Sathe, A., Gryning, S.-E., and Pena Diaz, A. (2011). Comparison of the atmospheric stability and wind profiles at two wind farm sites over a long marine fetch in the North Sea. Wind Energy, 14, 767-780. https://doi.org/10.1002/we.456

Svensson N., Bergstrom H., Sahlée E., and Rutgersson R. (2016): Stable conditions over the Baltic sea: model evaluation and climatology, Boreal environment research, ISSN 1239-6095, E-ISSN 1797-2469, Vol. 21, p. 387-404

Larsén, X.G., Vincent, C. and Larsen, S. (2013), Spectral structure of mesoscale winds over the water. Q.J.R. Meteorol. Soc., 139: 685-700. https://doi.org/10.1002/qj.2003

Following the reviewer's discussion, we feel that there is a need to make this more clear, hence we add some discussions in the Discussion, see page 19.

2. 10 and 18, the logarithmic law is used as the wind profile model. However, it is only valid in the surface layer. Wind turbine hub height is usually above the top of the surface layer and located in the Ekman layer or even in the "free atmosphere" for very stable conditions where boundary layer heights can be as low as several tens of meters. Please justify the use of the log law here.

   The use of these equations are threefold. First, Eq. 10 (in the original version, and it is Eq. 11 in the new version) is used to include the wave effect through the roughness length z_0, which is strictly at 10 m. Second, Eq. 18 (in the original version, and it is Eq. 16 in the new version) is used to connect U with neutral condition wind speed U_N, which is at all heights. Third, we extend the wind speed from one height to another, here in this study from 10 m to 250 m.

   The log-law with stability effect is indeed not a universal solution for describing the wind structure in the whole layer 10 – 250 m, it is however very practical to connect the description of wave, height and stability effects consistently, which makes it possible to do a global calculation. We are aware of the limitations such a simple algorithm is associated with, and this is likely also the reason why at some sites, the agreement between our model and measurements are less good than some other sites. But in general, the agreements in TI at all sites examined here in this study are in acceptable range. Following the reviewer's comments, we add discussions on the uncertainties to the section Discussion.

3. Section 2.2, since "wave" can refer to atmospheric wave, to avoid ambiguity, the authors may consider specifying that "wave" here refers to "ocean surface wave".

   Suggestion taken.

4. Section 2.2, considering wind sea only may be insufficient, as swells are common in windstorms and may influence the high wind speed regime of sea surface roughness. Please include more discussion/justification here.

   The sea state is described through the wave age parameter c_p/u_*, which is used in the parameterization of the roughness length. The expression was derived from field experiments from Fan et al. (2012), with a special interest addressing strong wind conditions. This might also be part of the reasons that the agreement of TI between our calculation and measurements are in general good at strong winds.

There are two types of swell, one is developed locally from the wind sea, and one is propagated from another region, the non-local swell. During storms, the sea surface is often dominated by the wind-generated wind sea, combined with the local strong winds, the wave age, when defined by peak frequency waves, is often rather young. It does not, however, exclude the presence of swell. The effect of swell can be significant at light winds, or when the non-local swell come from another direction from the wind. We admit that our current calculation with global coverage is not able to include the effect of such complicated sea state. We add discussion on swell's effect in the Discussion, see page 19.

5. It has been widely recognized by field observations and laboratory experiments that the sea surface roughness length and drag coefficient may decrease at high wind speeds (e.g., > 33 m/s). Eq. 13 considers this decrease using a parabola model. Please state explicitly this dependence and the underlying mechanism here (although the authors mentioned these in their discussion about Figure 3). This is also the reason why the Charnock model deviates from the SWAN simulation at high wind speeds in Figure 3.

   We re-organized the discussions on the dependence of turbulence-related parameters, including SWAN and the Andreas algorithms, in comparison with the Fan scheme. These discussions can be found in section 4.1 and section 5 (page 19, 2nd paragraph). The underlying mechanism from the original study was provided in section 4.1.

6. 23 and 24, if I remember correctly, these equations are valid only in statically neutral conditions. Eq. 24 is only valid in the "eddy surface layer". Please state these limitations explicitly. How large may the error be if these formulae are applied to non-neutral conditions?

   As explained previously in Point 4 in Major comments, our method does not use Eq. 23 and 24. These expressions (from classical studies) are used to compare with our studies.

   Eq. 23 was indeed prepared for the category of neutral condition as the field campaign data that are used for deriving the expression represented "near-neutral to slightly convective conditions".

   Both studies are meant for the entire boundary layer where the scale u_*/f is relevant and applied. This is also true for Eq. 24, which was from Högström et al. (2002). Eq. 24 was derived for the spectral range (iii) which was denoted by the authors as "for very low wavenumbers", and it was not the "surface eddy range", which is called "range (ii)" in their paper.

   We edited the corresponding text and make these points clearer, see Section 2.5.

7. When estimating the boundary layer height using $h = au_*/f$, a constant a = 0.3 is used, which is somewhat large to my knowledge. Please justify the use of this constant.

   a=0.1, 0.2 and 0.3 are used to include variations associated with potential factors including stability. As we explained in the "Main comments" and point 6 on this subject, we do not use z_i in our method. Eq. 23 and 24 are used to compare with our results.

8. It seems that the validation data come from sites restricted to European seas. Please discuss this limitation. Other ocean basins (e.g., tropical oceans) may have distinct atmospheric and oceanic conditions.

   We point this out in the Discussion in the new version, see page 20, last paragraph.

9. Please improve the presentation of contour plots in Figure 4. Finer resolutions could be used.

   The few contour lines are due to the range of TI is not so big over water. In the new version, we used more contour lines, but they mostly add more details to where there is large variation, e.g. in the tropics where it is of convective condition.

10. Please state explicitly that one limitation of the methodology is the potential for biased estimates in regions where windstorms, such as tropical cyclones and waterspouts, are prevalent.

    Suggestion taken in the new version, see the last paragraph in section Discussion.

Responses to reviewer 2

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

This paper presents a parametric model to derive a global atlas of turbulence intensity (TI), its 90th percentile and it variance wherever forcing parameters (which can be extracted from global reanalyses) are available. It leverages on a previous model built by the same group and on several parameterizations of physical effects, which can introduce variability in the TI. In particular, the present model includes the effect of 2D, large-scale turbulence in addition to the 3D local turbulence. The objective is interesting and the resulting model will certainly be useful for the offshore wind energy community. However, I found the present paper, though well written, nor so well organized neither always clear about the data used and the results. I give below some suggestions to improve its understanding and its scope.

Major comments

1. The hypotheses underlying the building of this atlas should be more clearly presented, probably at the beginning of the methods section. These general hypotheses, as far as I understand, should include the following: a few sites (13, but with an homogeneity of location and maybe atmospheric conditions), in N Europe and conditions representative of offshore wind and turbulence, are used to derive relationships which are considered valid over the global ocean. Maybe a discussion about the use of this model (LUT) over the global ocean — especially at place with conditions very different from those of the observing sites used here — could be added in the final discussion.

   Thanks for the comment, which tells us that indeed we need to improve the description of our method. The first thing is that the data from the 13 stations are used to *validate* the calculation, not to *derive* the relationships. While formulations like the stability and the wave effect are coming from studies across continents, the 2D+3D model has indeed been only verified with North European sites. We elaborated the discussion section on the uncertainties associated with these algorithms we used, and the expectation of more data validation from other seas with different atmospheric and ocean wave conditions.

2. The description of the data used to build the various dependencies of the parameterization and to validate it afterwards are introduced in several parts of the paper in a rather disorganised way. I struggled a bit to understand which datasets

were used when and for which purpose exactly. I suggest to include a table with all the data used, the length, heights and maybe specificities (wave age? stratification? water depth? length of the sample used to compute mean wind and variance, in link with the 2D turbulence inclusion, see 6 below) of the measurements. I understand that the Høvsøre site corresponds to one year of measurements from sonic anemometers at 10 and 100 m, but how long are the FINO measurements used here? Is there a good reason the present separately the comparisons with the 11 sites (Fig 5) and the two additional sites (Fig 6)? Maybe all the graphs shown Fig 5 could be gathered in one graph, as the heights of the different sites are close to each other, and the comparisons look rather homogeneous (and are not further commented in the text).

Section 3 is supposed to be the section where we provide necessary information about data used in this study. We revisited and revised this section and hope it is now more clear.

Specifically, this study makes use of research output from others' publications, hereby the distribution of TI with wind speed at 11 stations from Jeans (2024), at one station from Peña et al. (2016) and one station from Wang et al. (2014). That is also the reason Fig 5 and Fig 6 in the original version are separated. We agree with the reviewer that these two figures can certainly be put together and we take this suggestion in the revised version, now the new Fig. 5.

One of the most important reasons that we use directly the research output from others' studies is that we do not have access to most of these measurements. Another reason is to keep us "objective" by citing others' calculations which have been done by different authors with different approaches.

Regarding the 2D turbulence model, we did not use the measurements from Høvsøre in the current study. The model was developed conceptually but verified and validated by measurements from Høvsøre and Østerild in multiple studies previously. We only use the model here.

We take the advice from the reviewer and made a Table in Section 3.2 listing the measurement data used in this study.

In the revised version, we commented that the 13 sites used for data validation here are all from the European seas, dominated by similar weather. We welcome measurements from other regions with other relevant weather phenomena, e.g. Tropical cyclone and water sprouts (suggestion from Reviewer 1)

3. The quantification of the comparisons with measurements could be strengthened and made more explicit. Line 330 and below, only the MAE are given. Is this comparison done on the TI for the different bins of wind speed (as in Fig 5) or is it a comparison of the individual measurements of TI (time series?). Please specify. Also, it could be useful to include other metrics like root mean square of the difference or even correlation (if the comparisons are made on time series). A mean bias alone is often not representative of the agreement with the data. Also, the values given for the MAE could be commented: is it a lot? what is the corresponding relative error (percentage of the TI)?

This is a very fair question on if the reported bias/error is important or not. It is important to add this discussion to the text - thanks for pointing it out. The mean values of TI over water are in general of small magnitude compared to land condition, and with that, the difference between the modeled and observed TI here does not change the turbine class. However, a systematic difference of TI on the order of 0.01, which is about 20%, could be non-negligible for fatigue load, depending on yaw misalignment, as well as for wake modeling. This is now added to the text.

As explained in the previous comment, we do not have access to time series of most of the stations, and therefore comparison on that level is not possible. So yes the comparison is on TI for different wind speed bins. We revised the text accordingly.

4. The legend of the figures is not always precise. Fig 2: how long are the FINO measurements used here? Fig 4: is it the mean TI over the whole ERA5 time period?

   Good points. The details are now added. The length of the FINO measurements is addressed to Table 1. The period of ERA5 data, and the reason, have been provided in section 3.1 in the original version. It is now also added to Fig. 4 caption.

5. The results and their physical meaning could be discussed a bit further. I understand that the objective of the paper is really to present the TI atlas, but some results maybe worth commenting. Fig 5, the agreement looks really good and does not depend on the wind speed range. Is the inclusion of the 2D turbulence responsible for this agreement at low to moderate winds? Fig 6: opposite to Fig 5, the TI looks a bit underestimated by the LUT here, especially in the 4-12 m/s range. Is there a specific reason for that? Fig 4: Is the higher TI level in the intertropical zone only related to weaker winds there, or is there also an effect of the stratification? Is it strongly related to the inclusion of the 2D turbulence (large eddies, stronger instability)? If so, this is a real change with respect to previous TI modelling and should be emphasized.

   These points raised by the reviewer are very helpful for improving the discussion and interpretation of the results. We re-wrote section 4.2 and 4.3.1 in the new version.

6. I am not familiar with the use of '2D turbulence' term. I guess it relates to the larger (up to meso scale) eddies that are sometimes termed 'coherent structures' in boundary layer meteorology. Could you add a paragraph to define it more clearly and maybe specify a typical cutoff length or time scale (in link with Fig 1) and some examples of structures (rolls or plumes) and conditions susceptible to generate then? Also, it seems that the inclusion of the 2D turbulence in the TI computation is a major advance in better representing it at moderate and low wind. Is this true at every wind speed, or mainly for low and moderate winds? Is there a link with the stratification?

   In section Introduction, we added more content on the 2D turbulence with additional 10 publications, with the intention to help readers to get more familiar with the concept of the large scale wind fluctuations. 2D turbulence is ever present, and it is more dominating at higher elevations as the eddies are larger and the surface-driven turbulence becomes weaker there. The linkage of the presence of the large-scale turbulence and stratification is also discussed qualitatively in Discussion where uncertainties on the description of stability are addressed.

Line-by-line comments:

- l. 14: the question of the length T of the time scale use to compute mean/turbulent values can be linked to the 2D/3D turbulence and information about T in the data used to build/validate the model could be added.

  Yes, this is given at the beginning of Section 2.1.

- l. 23: 'with water areas reaching 200 km from coastlines'; the present model is global, but still based on observations collected less than 200 km offshore, is it correct?

  No. The concept of 200 km offshore was relevant in the completed project (GASP) reported in a publication from 2021 - 200 km from coastline was a compromise made to match the existing Global Wind Atlas for resource assessment. This study goes beyond it, as can be seen in Fig 4.

- l. 47-52: this is linked to the inclusion of 2D turbulence in the model, is it correct? Please comment.

  The study of Wang et al. tries to describe the decreasing dependence of TI with U at weak winds by imposing climatologically unstable condition. The outcome resembles the that calculated with 2D turbulence, except that the use of 2D turbulence does not involve assumption of climatologically unstable condition.

- l. 67-70: this mention of using LES modelling should be separated from the list of datasets and corresponding parametric relationships. I even wonder is this is relevant here?

  This short message indeed is a bit off, compared to the rest methods presented here for calculating TI. They, however, do belong to the category of using physics for the calculation of TI, in contrast to the empirical methods.

- l. 73: 'we model turbulence'—> we use of the Larsen 2016 model ?

  We realize that it is an odd expression 'we model turbulence'. The sentence is now revised: 'As a first step, we model the turbulence from 1 hour to 10 Hz'. We hope the text that follows in the revised version is clear that we use the 2D+3D model for this range.

- l.124: 3D turbulence is rather weak, relative contribution from 2D turbulence is bigger ??

  This sentence refers to atmospheric layer above water, where the surface-driven, 3D turbulence on average is weak due to the water surface being generally smooth (compared to land condition). The 3D turbulence decreases with height and becomes very weak at 100 m in comparison with 10 m. At the same time, the large-scale turbulence, that is associated with the background weather, is comparable at 10 m as at 100 m over water.

– l. 142: have all the data used for building or validating the model been sampled at (at least) 1h too?

This is explained previously in point 2 in Main Comments.

– l. 147: '°K' -> K

Suggestion taken.

– eq 10 to 13: I do not understand why using a direct formula for computing u_* from U is necessary here; the use of the Fan model (eq 12) in conjunction with eq 10 and 11 is enough to define a bulk model, provided one start from an a priori value either for u_* or z_0 and iterate (3 times typically) from that; introducing a new formula (Andreas et al 2015) could only bring inconsistency here.

Very good point. We now removed the use of the formulation from Andreas et al 2015, and only used the Fan scheme together with the logarithmic wind law to iterated u_*, and then calculated z_0. The corresponding text has been re-written. Just for the sake of curiosity, we plotted the relationship between u_* and U_{10}, and found that for the predominant wave age group, the two methods are very close to each other. Thus none of the conclusions presented have been changed. Nevertheless we updated all the affected figures and shared data.

– 2.3 stability effect: could be introduced earlier, for instance close to eq 10

Good point! We swapped section 2.2 and 2.3 and brought section 2.3 for the stability effect earlier.

– l. 272: Wang et al 2004 -> 2014

Correction done.

– l. 282: why using the SWAN Cd expression when another Cd expression (Andreas et al 2015) has been used previously?

Good point. All content on the SWAN and Andreas expressions has been re-written, both in section 4.1 and in Discussions. Their roles are minor, rather to compare with the LUT where a comprehensive sea state dependence is used.

– fig 7: the legend is not accurate (figs c and d missing); it seems that, at least at 30 and 50 m, the 90th percentiles for wind speed under 12 m/s are significantly higher in the observations than in the model; is it related to the model for

2D turbulence or to the variability of stratification in the observations? Please comment. Thanks for spotting this. The caption is now corrected. We also added discussion on the observed variation of TI at 30 m and 50 m, for which we can not rule out the possibility of flow distortion caused by the tower base.

– Data availability: the site cannot be accessed A new link is provided: https://figshare.com/s/29a94dde0a972e3825af

References

Jeans, G.: Converging profile relationships for offshore wind speed and turbulence intensity, Wind Energ. Sci., https://doi.org/10.5194/wes-9-2001-2024, 2024.

Peña, A., Floors, R., Sathe, A., Gryning, S.-E., Wagner, R., Courtney, M., Larsén, X., Hahmann, A., and Hasager, C.: Ten Years of Boundary-Layer and Wind-Power Meteorology at Høvsøre, Denmark, Boundary-Layer Meteorology, 158, 1–26, https://doi.org/10.1007/s10546-015-0079-8, © The Author(s) 2015. This article is published with open access at Springerlink.com, 2016.

Wang, H., Barthelmie, R. J., Pryor, S. C., and Kim, H. G.: A new turbulence model for offshore wind turbine standards, Wind Energy, https://doi.org/10.1002/we.1654, 2014.