Responses to reviewer 1

Xiaoli Guo Larsén[1], Marc Imberger[1], and Rogier Floors[1]

[1]Wind and Energy Systems Department, Technical University of Denmark, Frederiksborgvej 399, Roskilde, 4000, Denmark
[1]ADDRESS

Correspondence: Xiaoli Guo Larsén (xgal@dtu.dk)

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.