Interactive comment on "From standard wind measurements to spectral characterization: turbulence length scale and distribution" *by* Mark Kelly

M. Kelly

mkel@dtu.dk

First I (the author) would like to thank the anonymous reviewer (#1) for constructive criticism, towards improvement and clear dissemination.

I'll include the reviewer's comments as I respond point-by point, starting with the summary and proceeding through the comments.
The reviewer's comments are indented, including their original numbers.

> "Summary:
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> This paper presents a derivation of the turbulent length scale as a function

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> of standard deviation and wind profile using the Mann model (Mann, 1994) and also closely following the derivations provided in de Maré and Mann (2016). This reviewer believes that the manuscript has potential to be published, but first several clarifications are needed. Please see the full list of my comments below."

I would argue that the derivations in this paper *do not* 'follow' those in de Mare & Mann (2016). While the new expression (5) can be compared to an analogous one in de Mare & Mann, most of the expressions I derived don't have any correspondence or equivalent in de Mare & Mann—e.g. the simple practical (and perhaps most important) expression $L_{MM} \simeq \sigma_u/(dU/dz)$. I should add that the derivations in this work were done in 2015–16 (except the new generic vonKarman-simplification in eq.7); i.e. the work was done independently and concurrently in a different project than de Mare & Mann (2016).

> 1. "After Eq. (3) define L_{MM} as the turbulent length scale in Mann-model. You described all other parameters except for L_{MM}."

This error due to editing is now corrected in the revision.

> 2. "Although this reviewer is not a native English speaker, I would suggest that the authors uses less parentheses and footnotes if possible. For instance, the last sentence in Section 2.1.1 (around Line 20 on Page 4) contains many commas and a semicolon and parentheses that makes it difficult to understand. Similar examples can be found elsewhere in the manuscript."

I have attempted to use footnotes in such a way as to preserve the flow of the main text, so that details are available to the interested reader while minimally interrupting the flow.

However, as reviewer #1 points out, there are some relatively convoluted sentences. I have worked to clean up/clarify these in the revision.

> 3. "Sometimes you are using Figure and sometimes Fig. for figures (in Section 3.1 and later). Please be consistent."

I intentionally use 'Fig.' in some passages to avoid overusing the word 'Figure' in places where more references to figures occur. Checking the WES manuscript guidelines, this appears to be ok (I'd prefer to leave it, unless WES objects per their English style preferences).

> 4. "Font size in your figures is very large. I am not sure if this will be handled in the production stages if the manuscript gets accepted, but if not, you should decrease the font size."

Such 'big' figures were made for scaling to 1-column width (half of current size) in the final publication.

> 5. "Please specify the frequency of the occurrence of wind speeds above $7\,\mathrm{m\,s^{-1}}$ at the Høvsøre mast. Why $7\,\mathrm{m\,s^{-1}}$ and not, for instance, $5\,\mathrm{m\,s^{-1}}$?"

As written/mentioned, this was done with consideration of loads in a concurrent project—given the relatively infrequent occurence, lower impact on loads, higher difficulty fitting spectra in that regime, and larger spread of results.

Considering winds above cut-in, $P(U > 7\,\mathrm{m\,s^{-1}})$ for the period analyzed is 66% for the land case and 81% for the offshore case.

The conditional dependence of L_{MM} on wind speed is beyond the scope of this paper, but is the subject of ongoing work.

I am considering re-doing the plots and updating the analysis to be for the range 4–25 m/s (where $U > 7$m/s is noted for its slight difference)—but this involves many thousands of periods, hundreds of thousands of Fourier transforms and fits, which requires substantial time/work.

> 6. "Section 4.1 (Implications and Applications) should not be a part of the concluding section. Conclusions should conclude the study and not elaborate on the applications of the result. Please move Implications and Applications prior to Conclusions and remove the subsection title Summary of conclusions (Section 4.2). It is not typical to have subsections in conclusions."

I revise based on your suggestions.

> 7. "To this reviewer, current Section 4.1 is a typical discussion section and not implications and applications. I suggest the author renames this section to discussion."

I updated to make this part of the discussion section.

> 8. "The author concludes (e.g., Line 21 on Page 17) that L_{MM} is influenced by atmospheric stability but the analyses in this study are not conducted for unstable, stable and neutral conditions separately. Nothing has been said about the fluxes of sensible heat, Richardson number, Obukhov length, etc.
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> 9. Related to my previous comment, the paper by Peña Diaz et al. (2010) clearly lists the stability classes that were investigated (Table I in that article), so it would be useful to see similar analysis in this paper."

Responding to points 8–9 together: explicit stability considerations are beyond the scope of the current article. Part of the point of this paper is that for application to loads, where one is concerned most with $\{\sigma_u, U, \alpha, L_{MM}\}$, which are affected by stability, one then needs to get L_{MM} (the other 3 are easily obtained). We are not concerned here with stability itself—as the turbines are *not directly affected* by stability, as (re-)stated in the article and references cited.

However, in parallel work (in preparation for publication) and in related recent articles with Chougule et al (cited) we/I have examined treatment of stability.

Again, this is the subject of another paper, particularly because stability does not have a direct affect—but acts through $\sigma_u, U, \alpha(dU/dz)$ and L_{MM}.

> 10. "Please clarify the purpose of Section 2.2.2 (Modelled spectra: Covariances, anisotropy and Γ) and Section 2.3 (Ideal, neutral and surface-layer implications). All figures referee [sic] to Eq. (15) and the expressions prior to that equation. I don't see how these sections contribute to the manuscript."

Section 2.2.2 shows the theoretical self-consistency of the derived τ_M and model, with regard to u_* (i.e. shear stress) and σ_u and w.r.t. the mixing-length relation. Along the way, §2.2.2 also gives practical/understandable expressions for how Mann-model σ_u and u_* depend on Γ.

Section 2.3 shows the surface-layer limit of the derived L_{MM}; previously it was assumed that the Mann-model is basically designed to work in this limit. Further, §2.3 derives the expected asymptotic (neutral/equilibrium) relation connecting observed σ_u and the model-constraining σ_{iso}.

> 11. "Please discuss the reasons why the peak in the Mann model in Figure 6 is not captured by the other two models? This peak, although at small wavenumbers, is very prominent and should be explained. Please discuss."

As discussed in the text, this minor peak is not prominent ('probability well under 1%'). Note Fig. 6 is plotted in log-log coordinates, and these larger L_{MM} in the minor bump are less than 1/1000 times likely than the values occurring around the major peak. I should adjust 'under 1%' to become 'under 0.1%'.

This minor peak is likely not captured because information related to its cause is not carried through dU/dz, but rather within horizontal gradients—which implicitly affect the fit parameters including L_{MM}. I did not wish to speculate, without more detailed measurements; this kind of advective artifact is not trivial do pick apart, given the conditions and the difficulty of fitting spectra to the Mann-model when the observed spectral-peaks are at smaller wavenumbers.

> 12. "What is the sampling frequency of the lidar data? The peak in Figure 6 seem not to appear in Figure 5, so is it possible that the lidar measurements contain some bias or some filtering was applied (or something else)?"

As mentioned above this peak is rather rare and corresponds to the distances to a forest edge. The LIDAR are not the cause, as the peak comes from the sonic anemometer; using data from the sonics only (over smaller vertical extent), the same trend (no peak) arises as when using the LIDAR. Further, the LIDAR and sonic data at 45/44 m are giving values almost identical to each other.

13. "Please specify the source for Eq. (6)."

Equation 6 follows from τ_M integrated explicitly using the vonKarman spectrum: eq.3 is equal to eq.5. I now add mention of (3) and (5) being equal in the text preceeding (6), to avoid confusion.

14. "I recommend that the author writes the alternative equation for L_{MM} in Line 27, Page 5 as a numerated equation and not an in-line expression [i.e., Eq. (16)] since some researchers might prefer the usage of turbulence intensity and shear exponent over standard deviation and wind profile (or maybe they already have the data in the form of I and α)."

Amusingly in an earlier working draft I had actually done this, but removed it, thinking I had too many equations. But I agree and will switch back to having a separate numbered equation for $L_{MM} \simeq z I_{obs}/\alpha$.