Summary

This paper presents a derivation of the turbulent length scale as a function 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.

Comments

1. After Eq. (3) define L_{MM} as the turbulent length scale in Mann-model. You described all other parameters except for L_{MM}.
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.
3. Sometimes you are using Figure and sometimes Fig. for figures (in Section 3.1 and later). Please be consistent.
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.
5. Please specify the frequency of the occurrence of wind speeds above 7 m s^{-1} at the Høvsøre mast. Why 7 m s^{-1} and not, for instance, 5 m s^{-1}?
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.
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.
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.
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.
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 to Eq. (15) and the expressions prior to that equation. I don't see how these sections contribute to the manuscript.
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.
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)?
13. Please specify the source for Eq. (6).
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).