

Interactive comment on “Inter-annual variability of wind climates and wind turbine annual energy production” by Sara C. Pryor et al.

Anonymous Referee #1

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Synopsis

The ms analyses the inter-annual variability of wind speed and wind turbine energy production over the contiguous United States from WRF simulations and turbine data. They find that the usual assumption of 6 % variability is too high.

The ms deals with an interesting and relevant topic that deserves publication. I suggest publishing the ms after minor revisions according to the points listed below.

Revision items

(1) Figure 4b shows the power spectrum of the simulated wind speeds. It could be interesting to quantify how much the spectrum is deformed on its right-hand side (frequency larger than 1 per day) due to the turbulence parameterisation active in WRF.

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At least, a possible influence of the turbulence parameterisation on the high-frequency part of the spectrum should be mentioned.

(2) I wonder whether the authors know the study by Larsén et al. (2016). This study deals with the shape of the wind speed power spectrum and identifies a height dependence of this spectrum. The curves shown in Figure 4b of this ms should be discussed taking into account this height dependency.

(3) Parts of the ms (especially in Section 3) are a bit difficult to read, because the text contains so much abbreviations and percentage numbers. I wonder whether a slight rewording (and maybe the insertion of additional subsections) would help to increase the readability. Not all readers will digest the paper as a whole but usually they would like to pick from parts of it which refer to their needs.

(4) Part of the difficulty to read the paper comes from the fact that a Gaussian statistics (and the “6 %” value stems from such statistics) is compared to a distribution-free statistics throughout the paper. This produces vague statements such as (see, e.g., p.12, lines 10 and 11) “would appear to be conservative”. This makes it very difficult for the reader to extract a clear and simple “take-home message”.

(5) As a consequence of the item mentioned before, no clear new value is found which could replace the doubted “6 %” value. What would be the new IAV to be applied in future (at least over the contiguous US)?

Reference

Larsén, X.G., S.E. Larsen, E.L. Petersen, 2016: Full-Scale Spectrum of Boundary-Layer Winds. *Boundary-Layer Meteorol.*, 159, 349–371. DOI 10.1007/s10546-016-0129-x

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