

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

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General comments: Thank you for putting this work together, as you say in your paper there is a definite need for more work into this area – and your paper is a valuable contribution to the field. IAV itself has been so poorly represented globally, both in the standard 6% assumption (which is so grossly misused) but also the methodology applied to accounting for IAV. I found your comparison of the different metrics interesting, and it seems to me that there are two main areas for future work (1) how to account for IAV (which methodology) and (2) what values/distributions should be applied in different climates. The overall results for Eastern US are in-line with what I would expect, which is reassuring especially as traditional approaches (assuming normal distributions and wind speed IAV instead of AEP IAV) and the methodology that you have presented

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are very different.

Overall the paper was very well written, easy to read and the figures/captions are great.

Specific comments: The first thing that jumped out at me when looking through the report was the reported (and plotted) annual mean wind speed (Table 2 column 2 and again in Figure 3) being above 10 m/s. This seems too high unless there is a different interpretation that I am missing? The NREL US wind speed map at 100m suggests these values should be significantly (about 1/3rd?) lower. Please can you explain why these figures are high (and how this impacts on the results of the work)?

Do you consider the assumption of the generic turbine to have a significant impact on a particular wind farms IAV of AEP? Is this something that you have tested the sensitivity of?

Lastly throughout the report I found it confusing with comparisons made to various different metrics and that these aren't equivalent (P90 is compared with values of "9 in 10 years being within 0.9 and 1.1, and also measures of IQR). Is it possible to convert some of these measures to the same metric (I appreciate the traditional P90 assuming a normal distribution wouldn't work in this case)? That would make it easier for me as the reader to understand the magnitudes of the respective differences.

Technical corrections: Page 1 line 11: should this read "is poorly defined" rather than "poorly constrained"? Page 11 line 2: missing word in "used to \*\*\* monthly capacity factors" Page 17 line 14: "different" instead of "difference"

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