Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2018-48-AC2, 2018

WIND ENERGY SCIENCE DISCUSSIONS

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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Received and published: 27 August 2018

Response to review of Inter-annual variability of wind climates and wind turbine annual energy production

PLEASE NOTE A FULL COPY OF THE MANUSCRIPT WITH TRACKED CHANGES IS ATTACHED AS A SUPPLEMENT TO THIS RESPONSE.

Review 2 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

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Discussion paper

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 or AEP IAV) and the methodology that you have presented are very different. Overall the paper was very well written, easy to read and the figures/captions are great. Response: Thank you for your comments and for this positive assessment.

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)? Response: This is indeed a very important point WRF uses a sigma coordinate system that is approximately terrain following but the height of the 3rd model layer is not uniform across the simulation domain. The mean height above grid cell mean topography varies between 80 and 110 m. We have added a map to show this important point (Figure 2b) and text to section 2.1. The reviewer is quite correct – this may be a source of differences with NREL maps – e.g. the map for 100 m shown at https://www.nrel.gov/gis/images/100m_wind/awstwspd100onoff3-1.jpg, but of course there are other possible sources; e.g. the 2 km resolution noted on the legend on the NREL map might contribute. Also it is not documented which period was used by AWS Truepower to generate that map (so interannual variability may also be a source of any discrepancies).

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? Response: It might (as we now acknowledge in section 4). We think its probably a much smaller impact than for example vertical extrapolation of wind speeds from 10-m using a power law correction. We consider that IAV should be RELATIVELY insensitive to the power curve – which is for the most commonly deployed WT, but it is certainly worth evaluating in future work.

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. Response: Yes, it is quite correct (and is a point raised by the other reviewer also). Using a non-parametric description of dispersion is less accessible for some readers. We have undertaken some rewording to hopefully aid readers in following our discussion (see also response to points 4 and 5). See the tracked changed version of the manuscript given below.

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

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2018-48/wes-2018-48-AC2-supplement.pdf

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2018-48, 2018.