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Comment

Interactive comment on “Characterization of wind power resource in the United States” by U. B. Gunturu and C. A. Schlosser

Anonymous Referee #3

Received and published: 20 June 2012

Summary Statement:

The manuscript covers a lot of ground and provides important characterizations of wind resources over the United States. With some revisions and effort, the manuscript can be recommended for publication in ACP. Many of the revisions are minor (providing clarifications, fixing typos, and other similar changes). Some of the recommended revisions, however, have the potential to alter the interpretation of the conclusions. These revisions may or may not take a lot of effort. Without knowledge of the amount of effort, I have categorized the manuscript as "reconsidered after major revisions." In particular, the authors need to address the issues of the statistical significance of the fitted distributions, coefficients of variation using median values, changes in air density with height, and estimates of the impacts of various assumptions on their conclusions

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(e.g., neutral stability). I look forward to working with the authors to help address these issues.

Sect. 1.1.1:

- * I recommend including and describing the equation for the Weibull distribution.
- * On page 7307, lines 18-21, this section states that "the use of Weibull distribution overestimates the frequencies of the higher wind speeds" for nighttime cases in which winds are positively skewed relative to the Weibull. Shouldn't this be underestimates instead of overestimates? Please clarify.
- * On page 7307, lines 25-27, this section states that the Weibull distribution does not fit wind speed data 'well' in some cases (e.g., Morrissey et al, 2010). Using either MERRA reanalysis or prior work, can the authors provide quantitative measures (i.e., significance tests) for how good or bad the fits are?
- * On page 7307, lines 26, capitalize City in Boise City.

Sect. 1.1.2:

- * The discussion of the effect of the shape factor on the frequency of high wind speeds is confusing and contradictory. The correct statement on page 7308, lines 4-5 appears to contradict other statements in the section. For example, the manuscript states on page 7308, lines 13-15 "if the actual shape factor is less than 2, the frequencies of very high wind speeds are lowered..." Should this be raised instead of lowered? Moreover on lines 15-16 it states that "if the actual shape factor is greater than 2, the frequencies of very high wind speeds are increased..." Do the authors mean decreased instead of increased? Please clarify the discussion so that it is consistent.

Sect. 1.1.4:

- * I recommend combining this section with the previous section (1.1.3). Both make the same point. I.e., that wind resources are overestimated if based on short observational

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records made during particular phases of climate oscillations.

* Be consistent in referring to the work of Boccard ('they' on line 15 vs. 'he' on line 16).

* Please spell out the acronym AWEA. Likewise for all subsequent acronyms (e.g., NCEP/NCAR, ...).

Sect. 1.1.5:

* What is meant by "and different schemes" on page 7310, line 5?

* The statement on page 7310, lines 22-23 makes it sound like you aren't using wind speeds from MERRA. I recommend changing it as follows. "We computed the wind speed at different heights using boundary layer flux data from MERRA and boundary layer similarity theory."

Sect. 2.1.1:

* Equation 2 is incorrect. The psi function should not be included as an argument in the logarithm.

* Please provide evidence to support the neutral stability assumption made in going from equation 2 to 3 for the extrapolation of wind speed to other heights. How would your conclusions change if you included buoyancy corrections in your extrapolation? Moreover, can you compare your extrapolations to measured wind profiles?

* I also echo the concerns raised by Anonymous Referee #2 about the appropriateness of the extrapolation formula for highly stable and shallow nocturnal boundary layers. Wouldn't it be better to use the wind speed data from MERRA directly (not the surface diagnostics) for these cases?

Sect. 2.2:

* On page 7313, line 13, do you mean hourly-average values from MERRA instead of instantaneous values?

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* What are the implications of the assumption of constant air density (page 7313, lines 17-18)? Will your analysis and conclusions change if you account for variations in air density?

* The S term in the exponent of equation 5 is undefined. Moreover, page 7314, line 4 refers to V_z and P_z . Do you mean V_r and P_r ?

Sect. 2.3:

* The discussion of the distribution defined in equation 6 is good, but it isn't clear how you are applying the distribution to your analysis. After fitting the distribution, are you using it only to compute statistics (mean, median, cov) of episode lengths? Please clarify.

* Moreover, you can fit the data to any kind of distribution, but is it the 'right' distribution? Please provide information about the significance of the fits (e.g., Kolmogorov-Smirnov test). If the maximum likelihood fits are not good, you shouldn't use this distribution.

Sect. 3.1.1:

* This discussion starts in this section with Fig. 3. What happened to Fig. 2? Please change the order of figures or discussion so that the references to the figures are consecutive. It is confusing to bounce around out of sequence. This comment is applicable to the other sections of the manuscript too.

* This section also describes the median, so please change the name of the section to "Mean and Median WPD."

* On page 7317, line 6, do you mean the "center of the surface" layer? Please be explicit.

* On page 7317, line 14, I cannot see 800 W/m² over eastern Wyoming.

* On page 7317, line 20-12, the sentence "So, this figure implies..." doesn't make sense. How can the mean be less than half the mean? Please rewrite.

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Sect. 3.1.2:

* In the previous section you make the case that, given the highly skewed distributions, the median is a better metric than the mean. Why don't you then use the median instead of the mean in the coefficient of variation?

Sect. 3.1.3:

* The discussion of IQR could be moved to section 3.1.1.

Sect. 3.1.4:

* Reference of Figure 2 is out of sequence. (Ditto for other figures)

Sect. 3.1.5:

* The sentence starting on page 7320, line 6 is a little confusing because the mean and median are not consistent over most regions. Perhaps rewrite it as: "In the central US region the consistency between mean and median values indicates that the wind episode distributions are nearly symmetric. In the southeastern states, however, the mean and median values differ, indicating that the wind power is very steady only for isolated periods."

Sect. 4.1:

* On page 7321, lines 6-7, what is the difference between "highly uncertain" and "very uncertain"? Should one of these be changed to "certain"?

* On page 7321, lines 14-16, it is not clear why the Weibull fits lead to systematic overestimations. Can't the differences between the data and Weibull be distributed about zero, and hence give underestimations too?

Sect. 4.2:

* On page 7321, line 21, should this be 2.5 km x 2.5 km instead of 50 m x 50 m? Figure 6b has the former, not the latter.

* On page 7322, lines 14-15, I do not see any 'green' pixels in these regions corresponding to wind speeds between 6 and 7.5 m/s.

Sect. 4.3:

* Is equation 8 for illustrative purposes, or are you actually using it to extrapolate WPD to different hub heights? If the former, why not show the exponential dependence on height to make this point? If the latter, you should use MERRA data and the hydrostatic equation instead. Also, please provide units for the variables your equations.

* On page 7324, lines 6-7, the statement "increase in the quantity as the height is raised" is not consistent with Figs. 3 and 7. There's a decrease in WPD with height. Also please check the sign in Fig. 8.

* On page 7326, line 1, what do you mean by 'globally' in "...the variability of a quantity globally?" Please make this more explicit (e.g., the variability of a quantity relative to its central value).

* On page 7326, line 8, what do you mean by 'back-up'? Back-up resources?

* On page 7326, line 19, please add reference to Fig. 14a.

* On page 7326, line 23, the statement "...mean and median WPD in these regions is very less" is unclear. Less than what?

* On page 7327, lines 2-5, the statement "Not only are the median values..." implies a change in the direction in the skewness of the distributions. Can you provide an explanation for this behavior?

* On page 7327, lines 18-19, indicates that there are no geographical patterns in Figs. 15b and 15c. The spatial distribution of episode lengths at different heights look random. Why is this the case? This behavior does not occur for any other metrics or quantities. Is this somehow related to using a composite distribution and taking differences between short and long duration episodes? Please verify that equation 6 is

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implemented properly.

* On page 7327, line 22 states that "... southern tip of Texas - have very high variability." Glancing at the figure, the ratios are close to 1, and hence have low relative variability.

* On page 7328, lines 4-6 states that "The increase is more pronounced ..." I don't see any red pixels over the cited regions in Figs. 16b and 16c. What are you referring to?

Sect. 5.2:

* On page 7329, line 9, please add layer after surface in "...at the center of the surface..."

Figures:

* In all of the figures displaying spatial maps, the Great Lakes regions are masked out. Why? The offshore regions are not masked, so please show the computed quantities over the Great Lakes regions too.

* Fig. 2, please add units (e.g., fraction of time unavailable).

* Please add the height in the caption (i.e., Figs. 3, 4, ...).

* Fig. 6, can you use the same scale to make it easier to compare with NREL?

* Fig. 8, double check the sign of the difference.

* Fig. 11, what are the black contours in (a)? Please use the same color scale as NREL. Fix the caption (WPD not wind speed).

* Fig. 12, double check the sign of the differences (i.e., Fig. 12a minus Fig. 4 is negative).

* Fig. 17, is the WPD scale correct?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7305, 2012.

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