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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

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We are very grateful for the constructive comments by the anonymous reviewer #3. We tried to implement the detailed suggestions like the use of the robust coefficient of variation instead of the normal coefficient of variation. Although revision of the manuscript needed major effort, we are happy for the resultant clarity in the document.
In the following list, we respond and provide clarifications to the comments by the reviewer.
The original comment is in blue color, our response in magenta and the changes we made to the manuscript in black italics.

- I recommend including and describing the equation for the Weibull distribution

We have included the equation and the following description:

The Weibull distribution, which is commonly used to fit wind speeds, is a function of two parameters:

$$f(V) = (\frac{k}{c})(\frac{V}{c})^{k-1}e^{-(\frac{V}{c})^k}$$

where V is the wind speed, c is the scale factor and k is the shape factor which is dimensionless.

- 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.

  It should be 'underestimated'. The change has been affected.

- On page 7307, lines 25-27, this section states that the Weibull distribution does not ïňĄt 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., signiïňĄcance tests) for how good or bad the ïňĄts are?

  Tuller and Brett (1984) studied the conditions under which the Weibull distribution fits wind speeds very well. They delineate four important conditions: the orthogonal components of wind velocity, when raised to the power of $\frac{k}{2}$, where k is the shape factor of the Weibull distribution:

  - are normally distributed
  - have zero means

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– have equal variances *and*
– are uncorrelated.

The main conclusion from their study was that the Weibull fits the wind speed well for locations that have a circularly normal wind velocity, that is wind blows from all directions with almost the same frequency. They also performed the suggested Kolmogorov-Smirnov test for goodness of fit and they infer: " the test showed that with the extremely large sample sizes available in this study, we must reject the null hypothesis that there is no difference between the ordinary Weibull and the actual wind speed distribution at at least the 0.01 level at all stations".
They found that the fit was the poorest at the locations where the wind direction was circularly most asymmetric. So, circular normailty of wind velocity is an essential condition for wind speeds to follow the Weibull distribution.

- On page 7307, lines 26, capitalize City in Boise City.

  Made the suggested change.

- 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.

  The attached figure 1 shows the behavior of the Weibull distribution with changing shape factor (k). As k increases, the tail of the Weibull distribution

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decreases.
Let us imagine a station that has a wind speed distribution with a shape factor $k = 1$ (red line). But instead we use $k = 5$ (green line) which is greater than 1, to fit the wind speed. When we compare the tails of the two lines in the plot, in the case of $k = 5$, the frequencies of very high wind speeds are lowered. Thus, the two parts of the paragraph are consistent.
The uncertainty in the estimation of wind power resource has two distinct sources (in the present context):

- In the absence of a probability distribution that fits wind speeds very well, the Weibull is used. As shown by Tuller and Brett (1984) and Morissey (2010), Weibull does not fit wind speeds robustly.
- Use of a constant shape factor of 2 for all the locations introduces further uncertainty in the estimate.

In these two subsections, we tried to discuss these two points.

- 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 records made during particular phases of climate oscillations.

  We thank the reviewer for the suggestion and combine this section with the previous one.

- Be consistent in referring to the work of Boccard.

  The references have been made consistent.

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

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All acronyms have been expanded.

- What is meant by “and different schemes” on page 7310, line 5? ’Different schemes’ refers to the different methods of adjusting the wind speeds at lower altitudes (for instance, 10 m) to the wind turbine hub height (for instance, 50 m). We changed the lines to: *“Many of these observations are at different heights, and different schemes have been used to adjust the wind speeds to the wind turbine hub heights.”*
- 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 ïňĆux data from MERRA and boundary layer similarity theory.” We use the wind speed at the top of the surface layer to illustrate the descriptive statistics we use. But to compute the wind speed at different hub heights – 80 m, 100 m and 120 m – we do not use the wind speed from MERRA directly. As shown in the section Methodology, we use roughness length, friction velocity and displacement heightto compute the wind speed at the hub height.
- Equation 2 is incorrect. The psi function should not be included as an argument in the logarithm. The equation has been corrected.
- 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 proïňĄles?

  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? The US Wind Atlas by NREL (Elliott, 1987) uses neutral statbility as also most previous studies. As discussed in response to the comments of the anonymous reviewer #2, the present estimation is a first approximation. Further, in a later section of the present paper, we compare the present estimates with those of the NREL at 50 m and 80 m height.

The wind speed data from MERRA does not take the surface roughness into account which is a very important control on the vertical wind shear and hence in the vertical adjustment of wind speed.

Further, in a different set of experiments, we are taking the stability of the boundary layer into account and also are trying to parameterize local and small scale processes like the nocturnal low-level jet.

To show that neutral stability is an important caveat, we include an explicit mention in the limitations subsection.

- On page 7313, line 13, do you mean hourly-average values from MERRA instead of instantaneous values? Changed to 'hourly-average'.
- 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 equation:

$$\rho = 1.225 - (1.194 \times 10^{-4} z)$$

describes the approxiate variation of air density with altitude z according to the US Standard Atmospheric profile for air density. According to this equation, the density for the largest hub height we consider (120 m) is about 1.16%, Thus, our analysis and conclusions are invariant when changes in air density with altitude upto the hub height are considered.

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

  The 'S' term in the equation is defined and the notations are made consistent.

- The discussion of the distribution deïňĄned in equation 6 is good, but it isn't clear how you are applying the distribution to your analysis. After ïňĄtting the distribution, are you using it only to compute statistics (mean, median, cov) of episode lengths? Please clarify. We compute the episode lengths and their statistics (mean, median and the robust coefficient of variation) directly from the wind power density time series by counting the number of hours with WPD greater than $200W/m^2$. Only these statistics are analyzed and discussed in this report. As a different experiment, we fit the episode lengths found above by fitting the episode lengths to the joint distributions in the said equation. We discuss here the interesting and important aspects of the distribution parameters. We do not use the distributions for analysis and discussion in the later sections, but defer to a future communication.

- Moreover, you can ïňĄt the data to any kind of distribution, but is it the 'right' distribution? Please provide information about the signiïňĄcance of the ïňĄts (e.g., Kolmogorov-Smirnov test). If the maximum likelihood ïňĄts are not good, you shouldn't use this distribution. Sigl et al. (1979) showed that this joint distribution fits episode lengths in the US very well.

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

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- This section also describes the median, so please change the name of the section to “Mean and Median WPD.” The title of the subsection has been changed to 'Mean and Median WPD'.
- On page 7317, line 6, do you mean the “center of the surface” layer? Please be explicit. The line has been changed to make it explicit.
- On page 7317, line 14, I cannot see 800 W/m2 over eastern Wyoming. The line has been suitably changed to $500-600W/m^2$.
- On page 7317, line 20-12, the sentence “So, this ïňAgure implies...” doesn't make sense. How can the mean be less than half the mean? Please rewrite. The line has been rewritten as 'median WPD is less than half of the mean WPD'.
- 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 coefïňAcient of variation? Following this suggestion, we used the robust coefficient of variation defined as

  $$RCoV = \frac{median(absolutedeviationaboutthemedian)}{median}$$

  Further, the analysis has been rewritten in view of the robust coefficient of variation. *The robust coefficient of variation defined as:*

  $$RCoV = \frac{median(absolutedeviationaboutthemedian)}{median}$$

  has been used to study the variability of WPD in different regions of the US. For two regions with the same mean power density, the one with a lower median absolute deviation will have lower RCoV and is preferable (i.e. less variable power quality). Similarly, for two regions with the same median absolute deviation, the one with greater median wind power density is preferable and this has lower

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RCoV. Given the impact of variability in wind power on the electric grid and the economics of power generation and distribution, it is desirable to lay wind farms in regions of low RCoV of wind power. Figure 2 shows the robust coefficient of variation of WPD over the U.S.

Eastern and southwestern North Dakota, central and Southern Wisconsin, northwestern Illinois, Nebraska, southern Kansas and western Oklahoma have high mean WPD and moderate RCoV. The near offshore regions have large RCoV and hence greater variability. The central US has an RCoV that is moderate. It is interesting that the western Gulf coast that has higher meanWPD has lower RCoV whereas the eastern Gulf coast that has lower mean WPD has greater variability as measured by RCoV. The Great Lakes region has the same variability as the offshore regions. Largely, the eastern half of the US has moderate RCoV whereas the western half of the US has slightly greater RCoV. Similar asymmetry is shown by the far offshore regions: Pacific has moderate RCoV and the Atlantic has greater RCoV implying greater variability.

- The discussion of IQR could be moved to section 3.1.1. The suggested change has been affected.
- Reference of Figure 2 is out of sequence. (Ditto for other ïňĄgures) The order of figures has been made consistent.
- 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."
- On page 7321, lines 6-7, what is the difference between "highly uncertain" and

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"very uncertain"? Should one of these be changed to "certain"? The line has been correct to: *'1 being highly uncertain and 4 being very certain'*

- On page 7321, lines 14-16, it is not clear why the Weibull ïňĄts lead to systematic overestimations. Can't the differences between the data and Weibull be distributed about zero, and hence give underestimations too? Yes, Weibull fits can also give underestimations. But Justus et al. (1976) showed that the Weibull shape parameters in the central US is greater than 2. This region has the largest resource in the continental US. So, the overestimation of this resource is more significant than the underestimation in regions with low resource.

- 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. The description of the map at http://www.windpoweringamerica.gov/wind_maps.asp describes this map as being 'presented at a spatial resolution of $2.5\ Km$ that is interpolated to a finer scale', that is to $50 \times 50\ m^2$.

- 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. The line has been changed to: *wind speeds of about* 6 *m/s*

- 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. The said equation is only used to discuss the role of air density. As discussed in an earlier section, the air density is assumed constant in the lowest 120 m of the boundary layer. The mean difference owing to this assumption is ~ 1.1 % at 120 m altitude. The units of the variables are included.

- 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. Figure 8 is the difference between figure 7 and figure 11a. Now the order has been changed to make it consistent. Thus the lines are consistent.

- 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). By globally, we mean 'over the whole time series', as opposed to short term variations. We made it explicit as: *Coefficient of variation describes the variability of a quantity over the whole time series.*

- On page 7326, line 8, what do you mean by 'back-up'? Back-up resources? We mean back-up resources. We make it explicit in the line as: 'the back-up resources required to compensate the variability in the wind power would be greater.'

- 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, line 22 states that "... southern tip of Texas - have very high variability." Glancing at the ïňĄgure, 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? In view of changing from coefficient of variation to robust coefficient of variation, we rewrote the analysis and discussion on episode length analysis

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at different hub heights.

The geographical distribution of the mean wind episode length at 80 m shows great variation between 6 h to 38 h. The central US and the offshore regions have longer WPD episodes whereas the rest of the inland areas have shorter WPD episodes. The Gulf of Mexico and the Gulf coast too have longer mean WPD episodes, more than that in the central US. Thus, it is possible that these regions have very highly consistent wind power resource due to cyclonic activity in the Gulf of Mexico. The moderate episode lengths of the central US are due to the stong diurnal cycle in these regions. Thus, the episode length in these regions is very predictable compared to the regions where the episode length is very low or very high.

A similar picture is shown by the median WPD episode length at 80 m, shown in Figure ??. For the non-central US region, the mean and median values are close. The central US and the offshore regions have greater median values than the rest of the areas. The two figures also show that as the mean and median episode lengths increase, the distributions of the episode lengths are positively skewed.

Figures ?? and ?? show the geographical variation of the mean wind episode lengths at 100 m and 120 m compared to the mean WPD episode lengths at 80 m. It is interesting to see that the mean wind episode increases everywhere except the mountaineous region in the west. Further, like in the case of the other measures, the change in mean wind episode length also slowed down with height.

It is interesting that teh greatest change in mean episode lengths in the continental US is in teh northeast and easter US. Because of the greater roughness length of this region, wind speeds increase with height resulting in longer episodes. Thus, these regions benefit the most due to increase in hub heights.

While geographic patterns are discerneable in mean episode lengths, patterns

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are not clean until 120 m in the case of the median episode lengths. The difference median episode length plots seem to show random variation. The reason for this appearance is that median is a rank statistic and so, the difference is an integer number of hours (1 h or 0 h in the present plots) and in cases of even number of episodes, a 0.5 h difference in red color is seen at some points.

But at 120 m, the pattern clearly emerges that in the northeast, east coast, in some regions in the central US and in California, the median increases by an hour. Thus, these regions benefit the most in terms of raising the turbine hub height.

Robust coefficient of variation of WPD episode lengths at 80 m is shown in Figure ??. The central US region consisting of Montana, North Dakota, South Dakota, Nebraska, Kansas, Oklahoma and northern and southeast Texas, Iowa and Wisconsin have higher variability in episode lengths compared to the rest of the inland USA. The offshore regions have the greatest variability in episode lengths.

Figures ?? and ?? show the change in the robust coefficient of variation of the episode lengths as the hub height is raised to 100 m and 120 m respectively. The lack of geographical patterns at 100 m and their presence at the 120 m atltitude are explained by the fact that robust coefficient of variation is a rank statistic. The evoloving patterns at 120 m show increases and decreases in variability of episode lengths and the difference in variability from that at 80 m is negligible.

- On page 7329, line 9, please add layer after surface in "...at the center of the surface..." The line has been rewritten to make it consistent.
- In all of the ïňĄgures 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. All the figures have been

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redrawn to show the variation over the Great Lakes too and this variation has also been included in the discussion.

- Fig. 2, please add units (e.g., fraction of time unavailable). Units have been added.
- Please add the height in the caption (i.e., Figs. 3, 4, ...). The captions have been rewritten to be consistent.
- Fig. 6, can you use the same scale to make it easier to compare with NREL? Used the same scale as that of the NREL map.
- Fig. 8, double check the sign of the difference. The sign of the difference has been checked and it is consistent.
- Fig. 11, what are the black contours in (a)? Please use the same color scale as NREL. Fix the caption (WPD not wind speed). The figure has been redrawn with the same colormap as that by NREL.
- Fig. 12, double check the sign of the differences (i.e., Fig. 12a minus Fig. 4 is negative). Figure 4 pertains to the top of the surface layer, whose altitude is variable. Figure 12a shows the median at 80 m. So, they are consistent.
- Fig. 17, is the WPD scale correct? The figure has been drawn to make it consistent.

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

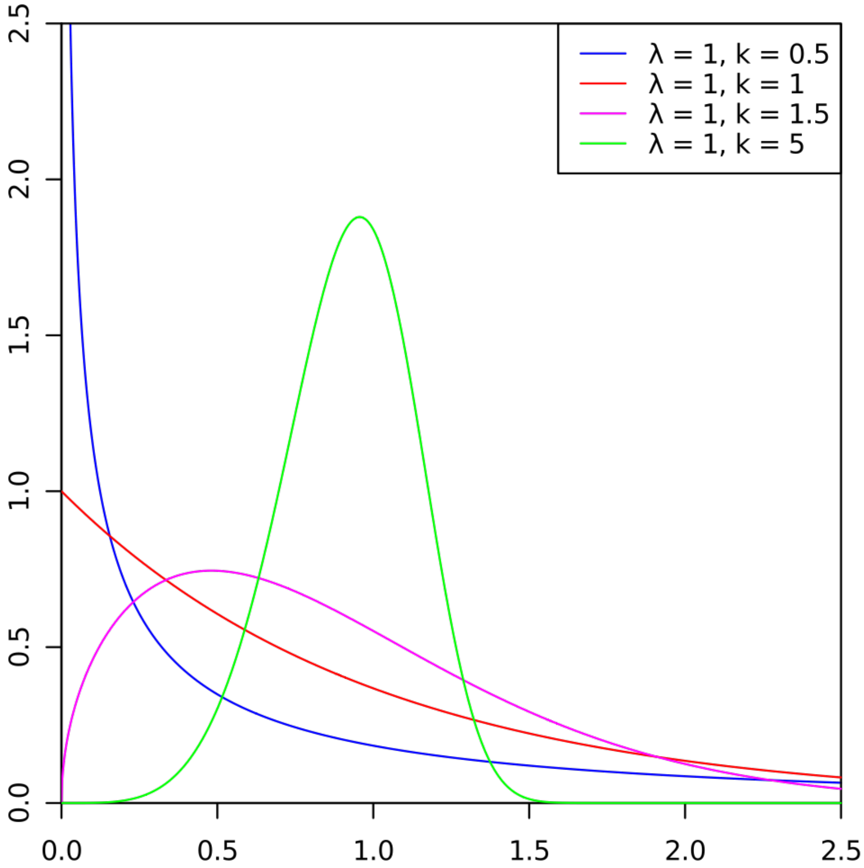

Fig. 1. Graph of the Weibull distribution for constant scale factor and varying shape factor (from Wikipedia).

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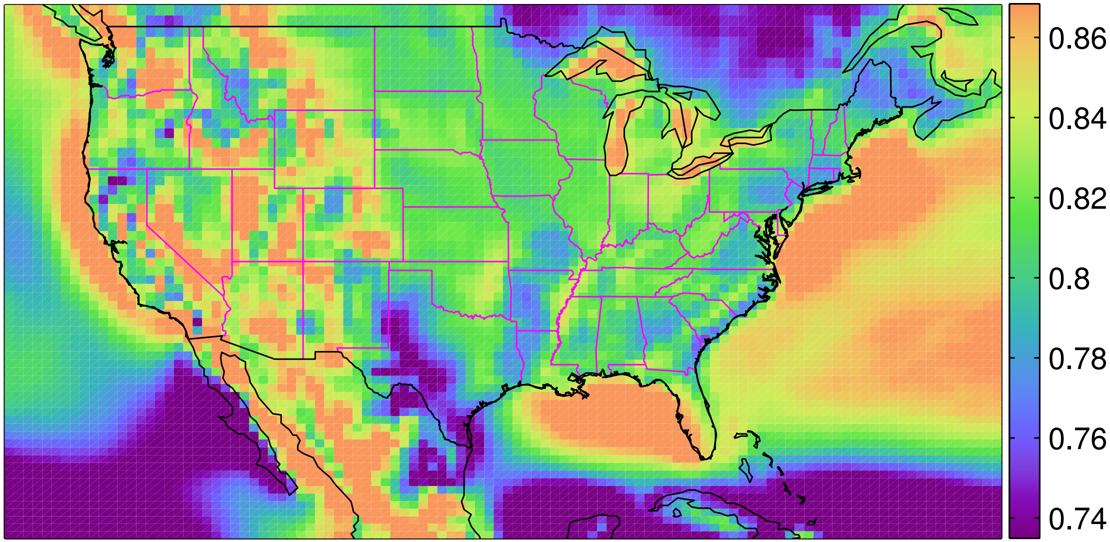

Fig. 2. Geographical variation of the robust coefficient of variation of WPD across the US at the top of the surface layer.

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