Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-16-SC4, 2020

Interactive comment on "US East Coast synthetic aperture radar wind atlas for offshore wind energy" *by* Tobias Ahsbahs et al.

George Young

g3y@psu.edu

Received and published: 20 March 2020

SUMMARY

This is a valuable manuscript demonstrating data quality control and analysis methods that should be of considerable use to the offshore wind energy community. The manuscript provides a well described application of methods from prior studies to the offshore waters of the United States' East Coast. The downside of this approach is that the current study retains all of the meteorological and statistical shortcomings of these existing methods. The advanced data sources, particularly WRF model analyses, used in this study, provide the authors with a, so far, unexploited opportunity to correct these short comings and set a new standard for SAR wind power analysis. My comments

below focus on highlighting these opportunities.

STRENGTHS

- Good choice of geophysical model function

OPPORTUNITIES

Opportunity 1 - Neutral vs stratified surface layer.

A longstanding challenge in SAR wind analysis has been that neutral stratification of the surface layer "must" be assumed. This has resulted in SAR retrieval algorithms returning estimates of neutral-equivalent wind rather than of true wind. The resulting neutral-equivalent wind is actually a proxy for surface stress, just expressed as wind via the neutral drag law.

The effect of this assumed neutral stratification of the surface layer is a wind speed bias that depends on the stability of the atmospheric surface layer. The SAR-derived wind speeds are too low in regions where the surface layer is stable, because wind speed must compensate for the too high (i.e. neutral rather than stable) drag coefficient assumed. Likewise, the SAR-derived wind speeds are too high in regions where the surface layer is unstable, because wind speed must compensate for the too low (i.e. neutral rather than unstable) drag coefficient assumed. Basically, SAR-derived wind is having to compensate for the lack of the stability dependence of the vertical mixing of momentum in the surface layer. This is reflected in the present study in the observation that SAR winds are faster than buoy winds over the Gulf Stream (where the atmospheric surface layer is destabilized by the warm underlying water) and slower than the buoy winds over the cold waters north of the Gulf Stream (where the atmospheric surface layer is stabilized by the cool underlying water).

For most of the history of SAR, that was the best anyone could do, because there were no good sources for surface layer stability estimates over the ocean. This study, however, has the access to WRF analyses from which surface layer stability can be easily calculated. In Section 3.2.1 - The TOGA COARE bulk flux algorithm is used to account for the effects stability on the vertical extrapolation of buoy winds. This same stability correction could be used to convert SAR-derived surface stress to stability-aware SAR-derived winds. All it would take would be to use the neutral drag law to convert the neutral-equivalent SAR-derived winds to surface stress and then the equations from the TOGA COARE bulk flux algorithm to convert that surface stress back to a stability-aware 10 m wind. This would be a major advance for SAR wind analysis, one the authors are perfectly positioned to make given that they are already using both WRF analyses (from which surface layer stability can be calculated) and the TOGA COARE bulk flux algorithm which allows their affects on the flux/wind relationship to be computed.

Locations where this issue comes up include: Page 2 lines 14-15 Page 5, line 15 Section 3.2.1 - all Page 11, lines 15-16 Page 13, Line 12 Page 17, Figure 8 - The Gulf Stream's northwest edge is so prominent in this figure precisely because of the lack of stability correction in the neutral-equivalent SAR-derived winds. Page 18, Figure 9 - Same. Page 20, lines 12-14 - This is another sign that the change in surface layer stability across the northwest edge of the Gulf Stream is contributing to the gradient in neutral-equivalent SAR-derived winds observed there. Page 21, line 4 - This is due to the cross-talk between surface layer stability and neutral-equivalent SAR-derived winds. Page 25, lines 22-24 - Here is where you basically outline the method I'm suggesting above. In short, you're most of the way there already, so you might as well make the advance and claim the glory.

Opportunity 2 - Weighting cases in Weibull fitting

The authors wisely weight cases to equalize monthly contributions to the mean, but forebear from doing so when fitting the Weibull distribution parameters. I was curious if this latter process was as hard as the authors assumed, so I looked up how Weibull distributions are fit and discovered that weighting data from different months differently in finding the parameters of a Weibull distribution should be straightforward.

See the link below for a clear discussion of how the method of moments is used to find the Weibull parameters. http://www.real-statistics.com/distribution-fitting/method-of-moments/method-of-moments-weibull/Since the inputs to this method are just mean and standard deviation, both of which can be computed with weighted observations, Weibull distributions can be fit with weighted observations with very little coding effort.

Publishing this trivial, but currently unused advance would be of great help to the SAR wind climatology community and would also impact other meteorological communities which are using the method of moments to fit various distributions to data that is unevenly distributed in space or time.

MINOR ITEMS

Page 1, line 22 - "vary" is vague. Some readers will read this sentence as meaning the mean wind speed is under 1 m/s rather than the intended meaning of the mean wind speed varying by this much across a wind-farm lease area. This issue of too general terms being used for statistics for which precise terms or phrases are available recurs in this manuscript. I have attempted to point out each location where reader confusion may arise.

Page 2, line 16 - "at scales around" - This wording will make most readers think the resolution rather than the swath width is several hundred kilometers.

Page 3, line 1 - "variation" is too vague a term. Please specify if you mean temporal or spatial variation and over what time or space scale.

Page 4, Table 1 - I suspect most readers would like a column with SAR pixel size. Also, incidence angle and swath width need units. Degrees and Kilometers, I suspect.

Page 5, Section 2.4 - It is not clear from this paragraph how these pieces fit together. In particular, it should be made clear whether or not WRF part of WTK?

Page 5, lines 19-21 - Please explain why the data source switched.

Page 6, lines 7-8 - "from modeled wind speeds" - It would help readers to know which modeling system you're referring to here.

Page 8, lines 6-8 - What are these numbers and why are they being discussed here. Are they extreme cases? Means? The discussion is to too terse for clarity.

Page 10, paragraph below the second equation - Would it be better to aggregate spatially before fitting the Weibull distribution rather than after? One worries about the order of fitting and smoothing when the fitting is a nonlinear process as it is in this second order moment approach. This is an issue of Jensen's Inequality, I think.

Page 13, line 12 - While the difference is small in the mean, that is in all likelihood because stable cases and unstable cases are roughly equally likely. The stability impact on the tails of the distribution could thus be quite large. The spatial distribution of biases noted by the authors speak strongly to the impact of surface layer stability on the errors in neutral-equivalent SAR-derived winds, even in the mean.

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