

Wind Energ. Sci. Discuss., referee comment RC1  
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## **Comment on wes-2022-16**

Anonymous Referee #1

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Referee comment on "Offshore reanalysis wind speed assessment across the wind turbine rotor layer off the United States Pacific coast" by Lindsay M. Sheridan et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2022-16-RC1>, 2022

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The manuscript by Sheridan et al. compares wind data from several numerical products against floating lidar buoy observations from two sites along the US Pacific Coast. This is relevant and interesting in particular because of the future relevance of the region for offshore wind energy utilization. This is also the main strength, why I like the manuscript not really the scientific novelty.

The manuscript is in general well written and readable. However, I have several major and minor aspects to be clarified before I can recommend publication in Wind Energy Science. This applies to the representation of the figures, interpretation of the results and description of underlying mechanisms.

### **Major Points:**

- **Readability of panel figures.** Later in the document there are several panel figures where the font size is very small and thus hard to read. As these are partly showing same quantities there are ways to increase figures sizes by e.g. sharing axes. This is for example the case for Figures 5, 10, 11, 12

- **Discussion of results:** There were several points in the presentation of the results where a phenomenon is correctly described but the interpretation and discussion is much too short or completely missing.

A few examples:

- Line 273 You mention that RAP is the best performing model at both sites. But there is no reason / interpretation given why.
- Line 301: The two sites tend to different stabilities. Why is this the case? No reason is given. Or is it just the different availability (see major comment #3)
- Line 321: Where do I see strong gradients in boundary layer depths? In what figure/reference? Or is this just guessing?
- Line 337: Models are found to slightly overestimate the slowest wind speed and strongly underestimate the fastest wind speed. What is the reason for this?
- Line 410: Shallow ABL are described to be observed during summer months. Why is this the case? This is especially important as the opposite is true onshore
- Line 465: model performance is described differently but no reasons are given why this is the case? Is it resolution? Assimilation of different data?
- Line 484: None of the models can capture the wind reversal. Is that such a small scale phenomenon that they all don't resolve?

Please go through the whole manuscript and especially the results section and improve the document with respect to this.

- **Comparison of Humboldt and Morro Bay:** I think it is fair to show results for two different sites but a comparison of model performance for the two sites is just not possible at all. There is a distinct annual cycle in the wind condition along the coastline and the measurements do not overlap in time. Please remove any comparisons of the two sites from the manuscript that cover data from periods where the Humboldt measurement data weren't available.
- **Ramp Events:** Ramp events are typically considered on time scales below 1h. I don't understand the motivation for investigating hourly data here. The thread to the grid is much lower on these time scales as with a deep penetration of wind energy there be

balancing between plants that are apart. What are the underlying mechanisms that the models resolve? Ramp events are e.g. in the North Sea often found for mesoscale phenomena that the reanalysis datasets aren't resolving. In the current version, I don't see the benefit of this section at all.

### **Minor Points:**

- Figure 1: The color of the wind farm areas are similar to the terrain color, please chose a different one
- Line 100: I think this sentence fits better at the beginning of section 2
- ERA5: 0.5 degrees. ERA5 is provided in 0.25 degree resolution. What is the reason for the averaging here?
- Line 170: Is the daily cycle covered with these satellites? I guess the overpass is always at the same time of the day?
- Line 226: underestimates the average at 50m by 1.6m/s. I guess you mean -1.6 m/s?
- Line 313: The impact of stability is also commonly observed in MOST, which typically shows larger errors during stable conditions -> I don't understand this sentence at all, why observed?
- Line 327: Similar model bias trends are observed when classifying as function of wind shear and turbulence intensity. As these ones, especially the shear one are much easier to measure with lidars than stability or turbulence, I think it is valuable to also show this, even when results are quite similar.
- Line 360: ... at very high wind speeds surface roughness does not increase with wind speed.... Do you have a reference for this? Isn't the surface roughness offshore increasing with increasing wind speed, e.g. due to wave breaking?
- Line 365: ... or beyond turbine cut-out... Many offshore wind turbines nowadays derate the power slowly after a certain threshold which is more grid friendly. This should be mentioned somewhere as this is relevant for e.g. the ramping discussions.
- Line 439: Wind reversal events comprise 11% of Morro Bay and 23 % of Humboldt deployment. This is such an example where a comparison is just not possible and shouldn't be made due to the different measurement periods.