

Response to Reviewers: Sheridan et al., 2026:

Performance of reanalysis and mesoscale models off the coast of Hawai'i

We would like to thank the reviewers and editor for their careful reading of our manuscript and comments to improve it. Each comment is reproduced below along with our accompanying responses.

Reviewer #1

Accept as is.

Thank you for reviewing our revision and considering our changes to the original manuscript.

Reviewer #2

I appreciate the large amount of work that the authors have done on the manuscript, which is much improved relative to the previous version. The analysis of the unusual or severe events that impacted the 'unusual' year of 2023 was very interesting.

However, the synthesis of the results and links between the sections needs to be improved, so that the manuscript doesn't read a collection of unconnected results. For example, the authors did some nice analysis showing the impact of turbulence intensity, wind shear, cloudiness, waves and ramps on the skill of the two datasets, but these results are probably all part of the same story of what scenarios are being poorly represented in the model.

We can see how the presentation of the different regime analyses of wind speed bias could appear independent, which was not our intention. So, after showing the overall stronger bias in ERA5 than in UH-WRF at the beginning of Section 3, we have added an additional paragraph at the end of Section 3.1 stating our motives for doing the different regime analyses of the wind speed biases. We also have added additional connecting sentences for each section of the regime comparisons and a transition paragraph at the end of Section 3.7 prior to presenting the specific events.

Another inconsistency I can see is that that authors have decreased the wind energy focus of the paper in the introduction, but later in the manuscript, it reverts to being a wind-energy motivated paper, with discussion of power curves, the impact of wind shear on turbines etc. Perhaps if it's not going to be explicitly a wind energy paper, then the results should be discussed in more general wind engineering terms (e.g. turbulence intensity is also an issue for other marine applications)

We appreciate the reviewer's perspective. Our original motivation for the paper remains focused on evaluating reanalysis products and mesoscale model performance off the coast of

Hawaii, particularly near the DOE buoy deployment. We do highlight wind energy applications in the introduction several times and discuss how model performance can influence wind resource characterization. To further guide the reader, we have added additional supporting statements in Section 3.1.

Line 18: Less complex -> less complex than what?

This is now clarified as less complex than land-based wind resource characterizations.

Line 69: The sentence seems back to front. Should it be: “Kalverla et al. (2020) attributed underestimation of observed offshore wind ramps to ERA5’s horizontal resolution, due to”

We have corrected this sentence.

Line 99: I guess it’s not really due to the concurrence of observations. Should it be something like “The concurrency of the observations and model estimates allows a detailed analysis of ...”

Yes, we phrased this poorly. We have modified this sentence.

Figure 9: Could you add (n=) to each category, to show how much data went into each set of bars?

Done.

Figure 16c: Why not add UHWRF and ERA5 to the pressure plot?

We have added the ERA5 and UH-WRF surface pressure to the time series in the plots of the February and April events, Figures 15 and 16. We find that UH-WRF more accurately simulates the surface pressure magnitude, i.e., a smaller positive bias versus the buoy, but ERA5 has a better correlation. This new comparison brought to our attention a systematic pressure difference between the two models, which we think is due to a non-zero elevation for the ERA5 grid cell containing the buoy. We added a paragraph discussing this in Section 3.8.2.